ADVISORY COUNCIL ON UNEMPLOYMENT COMPENSATION:

BACKGROUND PAPERS

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VOLUME II
JULY 1995

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PREFACE

In November 1991, the Congress of the United States passed the Emergency Unemployment Compensation Act (P.L. 102-164). The act included a section that created the Advisory Council on Unemployment Compensation, which was charged with the task of evaluating "the unemployment compensation program, including the purpose, goals, countercyclical effectiveness, coverage, benefit adequacy, trust fund solvency, funding of State administrative costs, administrative efficiency, and any other aspects of the program and to make recommendations for improvement."

The Advisory Council is made up of eleven members who represent the interests of business, labor, state governments, and the public. Five of the members are appointed by the President, three members are appointed by the Senate, and three members are appointed by the House of Representatives.

The Advisory Council has generally approached its work by focusing its attention on broad, fundamental elements of the Unemployment Insurance system. During 1993, its first year of operation, the Council examined the need for reform in the Extended Benefits component of the Unemployment Insurance system. Its work during the second year focused primarily on those issues related to benefits, eligibility, financing, and coverage. During its third and final year of operation, the Council is considering issues generally related to program administration, including appeals and federal-state responsibilities, as well as issues such as nonmonetary eligibility and program data.

In carrying out its mandate to evaluate and analyze the Unemployment Insurance system, the Advisory Council has relied on a diverse collection of information sources. The Council receives regular briefing materials from its staff and has also held a series of public hearings across the country in order to allow interested individuals and organizations to present their views to the Council. In addition, the Council has planned a number of academic conferences to facilitate the exchange of ideas and the presentation of works of research on Unemployment Insurance. These forums include two economics research conferences, one held in August 1994, and another planned for August 1995, and a legal symposium, sponsored jointly with the University of Michigan Journal of Law Reform in March 1995.

These two volumes contain much of the research that has been undertaken to date on behalf of the Council, both by the Council's staff and outside researchers. Additional reasearch will be published later this year. The papers presented at the legal symposium will be published separately by the University of Michigan Journal of Law Reform in 1996.

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The Incidence of the Unemployment Insurance Payroll Tax

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I. Introduction

The incidence of the unemployment insurance (UI) payroll tax is important for three main reasons. First, to assess the policy effects of a UI program, we need to know who ultimately pays for its costs. Is it consumers through higher prices, workers through lower wages, or owners of capital through lower profits? Second, an important part of the past research on UI focuses on the incentives created by different UI tax systems. In particular, the role of imperfect experience rating in encouraging the use of layoffs has been emphasized.[1] An underlying assumption of this work, however, is that the employer bears at least some burden of the UI payroll tax. Experience rating induced increases in tax rates do not provide an incentive to reduce layoffs if increases in the tax can be shifted on to workers or consumers. Third, evidence on the incidence of the UI payroll tax is useful to answer the larger question of the effects of other payroll taxes and mandated benefits on wages and employment. This question has drawn much recent attention with employer mandated health insurance under consideration.[2]

Unfortunately, there is currently almost no empirical evidence on the incidence of the UI payroll tax. More generally, the effects of firm-level variation in tax rates on wages and employment have not been extensively examined. Such variation (often based on experience rating or firm size) is a key part of many payroll taxes or mandates such as UI, workers' compensation, parental leave, advance layoff notice laws, and health reform proposals. Using administrative data from eight states, this paper directly examines the relationship between a firm's UI tax rate and the wages paid to its employees. In the next section, we discuss the theory of tax incidence and review some past work on the subject. Section III discusses the empirical implementation of the model, while Section IV presents the empirical results. Section V then concludes.

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[1]See Topel (1983, 1990), Card and Levine (1994), Anderson and Meyer (1993), for examples.

[2]See Gruber and Krueger (1993), Gruber (1994) and Aaron and Bosworth (1994), for examples.

II. Theory and Background

FINANCING BENEFITS THROUGH PAYROLL TAXES

A tax levied to finance fringe benefits is a standard textbook example. In response to the tax, the demand curve shifts downward and to the left, reducing the equilibrium wage and employment. The magnitude of the effect on the wage and employment depends on the demand and supply elasticities of labor. This textbook version can be modified for the case where the worker values the new benefit provided.[3] If the worker values the new benefit there will be a downward and rightward shift of the supply curve that will exaggerate the wage effect, but which may counteract and possibly even reverse the adverse employment effects of the tax. This case is neatly summarized in Gruber and Krueger (1991) in the equation:

$$(1) \qquad \frac{dW}{d\tau} = -\frac{\eta^d - \alpha\eta^s}{\eta^d - \eta^s},$$

where

W=the wage rate,

τ=the cost of the benefit,

η^d=the elasticity of labor demand,

η^s=the elasticity of labor supply, and

α=the worker's valuation of the benefit as a fraction of its cost.

Thus, the change in the wage rate depends on the elasticities of labor demand and supply as well as on the workers' valuation of the benefit. In the case where workers value the benefit at its cost to the firm ($\alpha=1$), there is no change in employment as the wage fully

[3]See Burkhauser and Turner (1985) and Summers (1989).

adjusts for the increased costs. In the general case, the proportionate change in employment following the tax is:

$$(2) \qquad \frac{dL}{L} = \frac{W_1 + \tau - W_0}{W_0} \eta^d,$$

where W_0 and W_1 are the wage rate before and after the tax, respectively. This equation states that the proportional change in employment is the proportional change in the cost of labor (wage rate plus tax) times the elasticity of labor demand. For a given elasticity of demand, if the wage falls more after the imposition of the tax, the adverse employment effect is diminished.

The incidence of the UI payroll tax is more complicated than this textbook case of a tax which is the same for all firms. Unemployment insurance taxes vary across states and across firms within states. The federal part of the UI tax which all firms face is currently 0.8 percent on the first $7,000 of wages, while the state tax usually has a wide range of rates and is levied on a slightly higher base. In fact, variation in tax rates or the costs of mandates is very common. The variation in Workers' Compensation (WC) premiums is similar to UI taxes. The Clinton health care reform proposal would levy rates ranging from 3.5 to 7.8 on firms depending on their size and wage level. Similarly, employer mandates such as the Worker Adjustment and Retraining Act (WARN), the Family Leave Act, and the Americans with Disabilities Act (ADA) only apply to large firms (over 100 employees for the WARN, over 14 for the ADA). Despite the fact that variation across firms in tax rates is quite common, to our knowledge there has been little attempt in past work to differentiate between the effects of this variation at the firm level and that at the market level.

The importance of tax heterogeneity for incidence can be illustrated by the case of variation in tax rates within a competitive industry. Within a given market for labor, the wage rate is fixed and cannot be lowered in response to variation in taxes at the firm level.

Thus, differences in tax rates within a given market cannot be shifted to labor. This example makes it clear that one should expect different wage and employment responses to variation in taxes within markets compared to variation across markets. To determine the likely incidence of a tax like the UI payroll tax, one needs to separately examine the effects of variation within markets and variation across markets.

In the case of UI it is unclear what is an appropriate value of α, the worker's valuation of UI as a fraction of its cost. In a cross-section, much of the variation in taxes reflects differences in the risk of layoff and its compensation through UI benefits. However, workers may put positive or negative value on this package of layoffs and benefits. To motivate the possibility of a positive value, we note that many union contracts require that more senior workers be temporarily laid off first. Presumably, when we examine changes in taxes below, much of the variation in taxes reflects increases in state UI tax schedules that have little to do with benefits, or the tax changes reflect changes in layoffs at the firm level a year or more in the past. These changes do not reflect changes in benefits received, so that α should probably be set to zero for analyses of these changes.

These observations suggest two issues that empirical work on incidence needs to consider. Keep in mind the UI case where there is roughly a fixed per worker tax on all firms, plus a tax that varies across firms within industries. It is the variation across firms that allows estimation of an effect of taxes on wages, but one also cares about the incidence of the fixed per worker tax. First, the incidence of the variation in taxes used to estimate the effects on wages may differ from the incidence of other components of the tax. Second, workers' valuation of the benefits associated with the variation in taxes may differ from their valuation of benefits associated with the flat tax on all firms.

PAST WORK ON UI INCIDENCE

A thorough discussion of the incidence of the UI payroll tax can be found in Lester (1962) who argued that most of the incidence of the UI tax will fall on firms. Lester assumed that most firms operate in competitive industries and will not be able to shift most of the tax by lowering wages or raising prices. He also rests his argument on evidence from the 1950's that there are firms at both the minimum and maximum rates in most industries (generally defined at the 2-digit SIC level), and that most of the minimum rates are very low. Thus, most of this component of variation in taxes should not be shiftable if firms are price takers in the labor and product markets. He concluded that only the federal tax (which is currently .8 percent) is potentially shiftable. Since on average taxes are 2.1 percent of taxable wages, this would imply that only about forty percent of the tax might be shiftable. In past work, we have established that a wide range of tax rates within a state continues to exist within 2-digit industry groups (Anderson and Meyer, 1992). The 2-digit industry classification is likely to be too broad a definition of competing firms in many cases, however. On the other hand, some of the industries (particularly manufacturing) have national output markets rather than state ones.

Hamermesh (1977) assumes that in the long run the incidence of the UI tax will fall about half on workers and half on consumers. He argues that the federal tax cannot be avoided by workers by changing jobs. Furthermore, the low level of excess profits in our economy prevents the UI tax from falling heavily on profits. Thus, the state part of the tax will likely result in long run increases in prices, though it may fall on firms in the short run.

PAST EMPIRICAL WORK ON COMPENSATING DIFFERENTIALS AND FRINGE BENEFITS

While the past work on the incidence of the UI payroll tax has been theoretical, there is a large body of empirical work on wage changes in response to increased costs of fringe benefits or changes in working conditions. This literature has not been particularly successful in estimating effects on wages except for finding compensating differentials for the risk of injury or death (Smith (1979)). Recently, the effects of mandated workers' compensation and health benefits on wages have been examined (Gruber and Krueger

(1991), Gruber (1994)). These studies find that most of the costs of these mandates are passed on to workers through lower wages. Neither of these studies, however, is able to differentiate between firm-level variation in tax rates and market-level variation.

III. Empirical Implementation

THE DATA

As was indicated above, based on theoretical considerations it is clear that we would like to compare the effects of both firm-level tax rates and market-level tax rates. While this has typically not been possible in the past, we are able to differentiate between the components of the UI tax. We construct the data sets for our analysis using administrative records from the UI systems of 8 states which participated in the Continuous Wage and Benefit History (CWBH) project.[4] For each of these 8 states, quarterly wage records and weekly UI receipt records were collected for a sample of between 5 and 20 percent of the state's covered workers from 1978 to 1984, with the exact sample period and sampling rate differing by state. This procedure results in a sample with approximately 30 million quarterly wage records. Since working with a data set of this size becomes unwieldy, we have drawn a random subsample of close to 1 million quarterly observations.[5]

Unfortunately, since we do not have information on hours or weeks worked in the quarter and therefore cannot calculate hourly or weekly wages, we must use quarterly earnings as our measure of the wage.[6] Thus, it is important to be able to exclude those quarters in which a separation or accession occurs, in order to be reasonably certain that we

[4]The eight states are Georgia, Idaho, Louisiana, Missouri, New Mexico, Pennsylvania, South Carolina and Washington.

[5]Additionally, only individuals with identification numbers unique to 7 significant digits were retained, allowing us to truncate identifiers and therefore make enormous savings in storage space.

[6]Information on weeks worked was collected in Pennsylvania and Washington, however.

are comparing full quarters. In the Appendix we describe the identification of these separations and accessions. Additionally, since it is likely that for multiple job holders at least one of those jobs is part time, we exclude all such observations. Additionally, we exclude all observations with quarterly earnings below \$1,000 or above \$29,000.[7] While such precautions should limit the number of observations which are not full quarters of full-time work, nonetheless there will be some part-time workers and those with short periods of uncompensated unemployment remaining in the data.

Additionally, we have an alternative earnings measure available to us. Each individual wage record also includes information on the firm's total quarterly payroll (in thousands of dollars) and average monthly employment over the quarter, allowing us to calculate average quarterly earnings at the firm.[8] Thus, we also create a firm-level data set. In both cases, the earnings measure is indexed using state average weekly earnings. For both the individual-level and firm-level data set, we calculate the firm's UI tax rate in the same way. Each state assigns firm tax rates based on a state schedule that relates a firm's tax rate to its past experience with the UI system.[9] This tax rate is reported on each wage record. In addition, there is a small federal tax of 0.7% prior to 1983 and 0.8% after that. However, the statutory tax rate does not equal the proportion of wages paid in taxes, because the taxable wage base in each state is usually much lower than average wages. Thus, we adjust each rate by multiplying it by the appropriate state or federal taxable wage base divided by 4 times the average of earnings over the current and past quarter. In the empirical work that follows, we will use changes in earnings over these two quarters, so

[7]This is measured in 1978 dollars. Note that a worker employed for 30 hours per week at the minimum wage of \$2.65 would have quarterly earnings of \$1,034. For the individual-level data, .128 of the observations are excluded due to the lower limit on earnings, while .003 are excluded due to the upper limit. The comparable figures for the firm-level data are .088 and .004.

[8]The same quarterly earnings restrictions are applied to the firm data as were used with the individual-level data.

[9]See Anderson and Meyer (1993a) for additional details on the features of the UI system.

this average wage measure should not be spuriously correlated with this dependent variable.

Having expressed the firm's tax rate as a proportion of earnings, it is now straightforward to construct a similar measure at the market level. To define a market, we have grouped similar 3-digit standard industrial classifications (SIC) into 138 different industries. Since we are mainly concerned with labor market effects, we define the market to be local (i.e. state level), rather than national. Thus, for a local market rate, each observation is assigned the average tax rate over all firm observations in the state that quarter within the grouped 3-digit industry. However, if there are less than 4 firms in the local market, the observation is dropped.[10] Note that for many nonmanufacturing industries, including the largest ones of services and retail trade, this labor market-based definition is probably also a reasonable definition of the output market. A final variable of interest is the level of employment. As was noted above, each individual wage record also contains the firm's average monthly employment over the quarter. Thus, our firm-level data set will allow us to investigate both the earnings and employment effects of the UI payroll tax. Recall that theory would imply smaller employment effects the larger the earnings effects are and vice versa. Finally, we also aggregate the firm-level data to create a market-level data set.

MODEL SPECIFICATION

A drawback to the available data is a lack of any individual-level controls and of most firm-level controls. To deal with this omission, we estimate all of our models in differences. For the individual data, we calculate annual changes within job matches, that is, we use only individuals that remain in the same job across two years. Correspondingly, for the firm-level data we calculate annual changes within firms. Thus for the models that

[10]This implies dropping an additional .125 of the individual-level observations and an additional .140 of the firm-level observations .

we estimate, the variation in tax rates is due solely to changes over time within the firm. At the firm level, these changes are due to changes at the state level in the tax schedule and taxable wage base, as well as due to movements of the firm along a given state tax schedule. These movements along a tax schedule occur due to the experience rating of the UI system. Thus, all else equal, a firm which increases its use of the system should see an increase in its tax rate. At the market level, the variation in tax rates is due to the same state-level changes, as well as to the aggregated effect of changes due to individual firms movements along a given schedule.

In all cases, we use the change in the natural log of the appropriate earnings or employment measure as our dependent variable. For the earnings equations, not only is a log specification the standard, but since the tax rates are expressed as a proportion of earnings, this specification provides us with a straightforward interpretation of the key tax coefficient. If the incidence is fully on the firm, we would expect the coefficient on the tax rate to be 0, while if the incidence is fully on the worker, we would expect the coefficient to be -1. More generally, the coefficient should be in the interval [-1, 0], where the absolute value represents the proportion of the tax which is passed on to the workers in the form of lower earnings. This restriction does not hold for all market structures, however. For example, in the case of oligopoly, the coefficient may be less than -1, as shown in Katz and Rosen (1988). While there is not a similar [0, -1] restriction on the tax coefficient in the employment equation, if the full amount of the tax is passed on in the form of lower earnings, the employment effect should be zero, while in all other cases the effect should be negative.

Recall, however, that for a firm which is a price taker in the labor market, we expect a competitive wage to be set at the market level, theoretically implying a firm-level tax coefficient of zero in the earnings regression, while the market-level coefficient is expected to be negative. Accordingly, since the cost of labor (including the tax) for a price-taking firm rises with the tax, we expect an employment decline in response to the tax. To the extent that markets are not perfectly competitive, and given that our definition of the market and our market tax rate are approximate, both the firm-level and market-level rates may be significant in each case. Thus, we first regress the change in log earnings and the change in log employment on both the change in the firm-level tax rate and on the change in the market-level tax rate. Additionally, we estimate models in which changes in the firm-level and market-level rates are included separately. This last specification is most similar in spirit to what has been done in past empirical work. Descriptive statistics for the key variables in each data set are presented in table 2.

In all cases, we estimate the various models first allowing only for aggregate time effects, and then allowing the time effects to be state-specific. Note that in this latter case we are essentially removing any variation which is due solely to changes in tax rates which are common across all firms in the state. While it is often assumed that variation in tax rate due to changes in the state tax schedule is more exogenous than variation due to movement of the firm along a schedule, this is likely not to be a valid assumption for this work, especially when considering the relationship with employment.[11] It is often the case in our sample that upward shifts in the state tax schedule occur after a recession, when state trust funds have been depleted. Thus, positive changes in the tax schedule will tend to be correlated with economic recovery, when employment gains are likely to be positive. In such a case, reliance on variation due to changes in state schedules is likely to result in estimates with a positive bias.

IV. Empirical Results

ESTIMATES USING INDIVIDUAL DATA

[11]Besley and Case (1994) address the issue of endogenous policy change more generally, as well as providing an application to WC.

We begin by using individual-level data to estimate the effect of the UI tax on individual quarterly earnings. Table 3 presents the results from several alternative specifications of regressions using the annual change in quarterly earnings for those individuals not changing firms. In model (1) both the change in the market-level tax rate and in the firm-level tax rate are included, while in models (2) and (3) only the change in either the market-level or the firm-level rate is included. Models (4) to (6) repeat these specifications, but allowing for state-specific time effects. Looking first at models (1) to (3), we see that only one of the coefficients on the change in the tax rate has the expected negative sign, and none of the coefficients are significant. The results of models (4) to (6), however, are much more in line with our prior expectations. Here, we see that all of the coefficients are negative, and those on the change in the market-level rate are significantly so. Note that while we cannot reject that the coefficients on the change in the firm-level rates are different from zero, we are able to reject that they are equal to -1. Thus, we can conclude that firms do bear some burden of the experience rated tax. However, we cannot reject that the coefficients on the change in the market-level rate are equal to -1.

Not only are the results of models (4) to (6) more in line with the theoretical predictions, they are also based on an arguably more exogenous source of variation than are the estimates of models (1) to (3). Since we are using quarterly earnings, our individual earnings measure is likely to be influenced by hours and weeks of employment, despite our precautions. Thus, we would expect this earnings measure to be correlated with the business cycle in a manner similar to that of employment, as was discussed above. Recall that the fact that shifts up in the tax schedule generally occur during upswings in the cycle implies that estimates using this source of variation are likely to be positively biased. In fact, when we rely less on this type of variation, the estimates are much more negative. Thus, based on the individual data, and models (4) to (6), we would conclude that at the market-level, much of the UI payroll tax is passed on to the worker in the form of lower wages, but that within markets, firms are unable to pass on all of the tax. Since we are also interested in the employment effects of the tax, though, we now turn to firm and industry data.

ESTIMATES USING FIRM AND INDUSTRY DATA

Table 4 presents the results from using firm data to estimate the effect of the UI tax on firm earnings and employment. Models (1) to (6) correspond to those in table 3, with the top panel using the annual change in average quarterly earnings at the firm as the dependent variable, and the bottom panel using the annual change in employment at the firm. The results of models (1) to (3) in the top panel, however, are quite different from those of table 3. Here, the estimated coefficients on the change in the market-level rate are significantly negative. In fact, in both cases we can reject that the coefficients are -1, indicating that the estimates are outside of our expected range for competitive markets. When entered alone, as in model (3), the coefficient on the change in the firm tax rate is negative, and significantly different from both zero and -1. However, in model (1) it is positive and insignificant.

Turning to the bottom panel, models (1) to (3) continue to provide unexpected results. When the changes in both market-level and firm-level tax rates are included, the effect of the market rate is positive and significant. When the change in the market rate is included alone, it remains positive, but is no longer significant. The coefficient on the change in the firm-level rate is negative and significant in each case. Recall, however, that the correlation of shifts in state tax schedules with the business cycle is likely to positively bias estimates of the effect of the UI tax on employment in these models. Indeed, when we compare the results of models (1) to (3) with those of models (4) to (6) we see that the former are generally much more positive. While the coefficient on the change in the market rate in model (4) remains positive, given the large standard error, we cannot reject that the true effect is negative. Also, although the coefficient in model (5) is negative, it is not significantly different from zero. The coefficients on the change in the firm-level tax rate remain significantly negative.

Returning to the top panel, we see that the results of models (4) to (6) are somewhat similar to those obtained in table 3. The coefficients on the change in the firm-level tax rate are never significantly different from zero, but in each case we can reject that the coefficient is equal to -1. The coefficients on the change in the market-level rate are negative, but due to the large standard errors we can reject neither that they are different from zero nor that they are different from -1. In the case of model (4), the magnitude of the point estimate is not far off from that of table 3, though. Comparing these results to those of models (1) to (3), though, we see that they are less negative, implying that our earnings measure is countercyclical. While this is perhaps an unexpected result, it may be plausible given the definition of earnings that is used. Recall that total payroll is divided by firm employment in order to obtain average earnings at the firm. If payroll is less procyclical than employment, this overall measure will appear countercyclical. As a result, models which use variation due to shifts in state tax schedules will be negatively biased.

Overall, then, based on the firm-level data and models (4) to (6), we can conclude that firms are unable to fully pass on the UI tax in the form of lower wages, and that we thus see firm employment decline with an increase in the tax. However, we cannot reject that the tax is passed on at the market-level and that there is no overall employment effect. Unfortunately, the standard errors are such that we also cannot reject that the burden remains on the firm at the market-level, and that there is a negative effect on overall employment.

It is useful at this point to step back momentarily and look at the bigger picture. Table 5 presents the results of estimating several models using data aggregated to the market level. Model (2) allows for state-specific time effects, while model (1) does not. As before the top panel looks at annual changes in average earnings, while the bottom panel looks at annual changes in average employment in the industry. These models are similar in spirit to the aggregate estimates in Gruber and Krueger (1991), which use ten year state-industry cell differences. Just as we saw in table 4, when we use variation which is due to shifts in state tax schedules, as in model (1), we find an effect on earnings which is too negative, while we find a positive but insignificant effect on employment. The results of model (2) are in line with both expectations and the previous results. Again, we estimate a large negative coefficient on the change in the tax rate, but as with the firm data the large standard error implies we cannot reject that it is zero or -1. Similarly, looking at employment in the bottom panel, the large negative coefficient on the change in the tax rate is only marginally significantly different from zero.

POSSIBLE BIASES IN ESTIMATION

There remains still an additional issue to consider, given our reliance mainly on firms' movement along state tax schedules. Recall that these movements are due to firm behavior, since the UI system is experience rated. Thus, a firm which increases its use of the UI system will generally find its tax rate increasing, while the opposite is true of a firm which decreases its use of the system. If changes in individual earnings or average earnings or employment at the firm are correlated with changes in the firm's UI experience, our estimated coefficients may be biased. For example, consider the idea that a job providing a higher risk of unemployment may require a positive compensating wage differential.[12] If the change in layoff behavior which led to the change in the tax rate is permanent, then there has been a change in the risk of layoff, and there should be a resultant change in compensation. In such a case, our estimated coefficient for the effect of the UI tax on earnings would be positively biased. We should note, though, that a firm's tax rate is only adjusted to its layoff behavior with a lag. Thus, if compensation is adjusted

[12]Topel (1984) finds a significant compensating differential for unemployment, while Abowd and Ashenfelter (1981) find estimates that suggest the opposite. We should note that these authors do not account for the incidence of differences in taxes across high and low layoff firms.

contemporaneously, our estimates should be unaffected.[13] Additionally, if the change is only temporary, there should be no change in compensation policy.

V. Conclusions

In this paper, we have investigated the effects of the UI payroll tax on earnings and employment, using both market-level and firm-level tax rates. For a program such as UI, in which tax rates vary across firms within markets, it is necessary to consider both effects. However, past work on similar programs, such as workers' compensation, has been able to explore only market-level effects. Results based on market-level rates can be useful for the evaluation of any program which includes a uniform mandate which does not vary across firms. However, due to such things as special considerations for small business, few mandated benefit proposals fit this mold of uniform tax rates across firms. Thus, while one can draw conclusions about market level effects from the coefficients on market-level rates, firm-level rates are necessary to fully explore the impact on the firm. Beyond the more general applicability to any mandated benefit whose financing varies across firms, the ability to estimate firm-level effects is especially important for experience rated programs such as UI and workers' compensation. The incentive effects of an experience rated system will be dissipated to the extent that a firm can shift the full amount of its tax to the worker.

While our estimates of the effects of both market-level and firm-level tax rates on earnings and employment remain somewhat imprecise, we have seen several patterns. First, our estimates suggest that at least part of the market-level rate is passed on in the form of lower wages, with the point estimates generally implying close to complete

[13]Tax rates are generally changed on January 1, based on experience through June of the previous year We have also estimated models using only the 3rd and 4th quarters, which implies an additional lag. The standard errors are quite large, precluding the making of any definitive conclusions. However, the point estimates actually somewhat more positive, rather than more negative as would be expected if this simultaneity were a problem.

shifting. Second, estimates using the firm-level rate strongly indicate that at least part of this rate is not shifted to the worker, with the point estimates implying that the full incidence falls on the firm. The estimated employment effects are for the most part consistent with these conclusions. We find that firm employment falls more in response to increases in the firm's own rate than to increases in the market rate.

While the empirical results leave us with fairly imprecise estimates for the incidence of the UI payroll tax, they do still provide us with several lessons which are applicable to the study of mandated benefits. For example, past studies have generally explored only the effect of the market-level rate on wages, often finding a large negative effect on wages. The implication is then drawn that the employment effects of the mandate will be minimal. While this is certainly true in the aggregate at the market-level, our results indicate that in the absence of a universal mandate, large amounts of employment reallocation may take place at the firm-level.

Appendix on Identification of Turnover

In order to identify separations and accessions, we use the person and firm identifier available on the wage records to create job-match histories for each individual. If a specific job match last appears in a quarter other than the final quarter of data collection, we identify a separation to have occurred at that time. Similarly, if a specific job match first appears in a quarter other than the first quarter of data collection, we identify an accession to have occurred at that time. A drawback to this method is that separations which are followed by a return to the same job without a full calendar quarter intervening will be missed. However, since the second type of data consist of UI claims records, by matching these to the wage records we can identify those short temporary separations that result in UI receipt. We then are able to drop any quarterly record for which we have coded an accession or separation or any weeks of UI receipt. Additionally, we drop the first and last quarters of data collection, since we cannot tell whether or not an accession or separation occurs in those quarters.

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Table 1

Key UI Program Parameters
on July 1, 1980 and July 1, 1983

	Georgia	Idaho	Louisiana	Missouri	New Mexico	Penn-sylvania	South Carolina	Wash-ington
Taxable Wage Base ($)								
1980	6,000	10,800	6,000	6,000	7,200	6,300	6,000	9,660
1983	7,000	14,400	7,000	7,000	9,300	7,000	7,000	11,400
Maximum Weekly Benefit ($)								
1980	90	132	149	105	106	170	114	150
1983	125	159	205	105	142	213	118	185
Minimum Tax Rate (%)								
1980	.07	.90	.13	.00	.60	2.35	1.30	3.00
1983	.06	1.70	.37	.80	.60	2.50	1.30	3.00
Maximum Tax Rate (%)								
1980	5.71	4.00	3.33	6.00	4.20	6.45	4.10	3.00
1983	5.38	5.60	5.50	4.40	4.20	6.60	4.10	3.00

Table 2

Descriptive Statistics
for
Key Variables

	Mean	Standard Deviation	Minimum	Maximum
Individual-Level Data				
Δ Firm-Level Tax Rate	-0.0004	0.004	-0.055	0.037
Δ Market-Level Tax Rate	-0.0004	0.003	-0.017	0.020
Δ Ln(Earnings)	0.012	0.213	-2.857	3.271
Firm-Level Data				
Δ Firm-Level Tax Rate	-0.0004	0.004	-0.055	0.049
Δ Market-Level Tax Rate	-0.0004	0.003	-0.017	0.020
Δ Ln(Earnings)	-0.016	0.243	-3.134	3.303
Δ Ln(Employment)	0.003	0.355	-8.981	8.833
Industry-Level Data				
Δ Market-Level Tax Rate	-0.0004	0.003	-0.017	0.020
Δ Ln(Earnings)	-0.019	0.141	-1.724	1.019
Δ Ln(Employment)	0.001	0.160	-2.199	1.438

Table 3

OLS Estimates of the
Effect of UI Taxes on
Individual Earnings

Dependent Variable	ΔMarket-Level Tax Rate	ΔFirm-Level Tax Rate	Calendar Quarter Only	Calendar Quarter by State	Number of Obs	R^2
Only Individuals in Same Job Match						
(1) ΔLn(Earnings)	0.319	-0.004	YES	NO	138886	0.005
	(0.325)	(0.221)				
(2) ΔLn(Earnings)	0.316	--	YES	NO	138886	0.005
	(0.256)					
(3) ΔLn(Earnings)	--	0.130	YES	NO	138886	0.005
		(0.173)				
(4) ΔLn(Earnings)	-1.276	-0.029	NO	YES	138886	0.011
	(0.495)	(0.221)				
(5) ΔLn(Earnings)	-1.300	--	NO	YES	138886	0.011
	(0.463)					
(6) ΔLn(Earnings)	--	-0.231	NO	YES	138886	0.011
		(0.207)				

Note: Changes are annual changes calculated using quarterly data (i.e 1983:1 - 1982:1, 1983:2 - 1982:2, 1983:3 - 1982:3, 1983:4 - 1982:4, etc.).

Table 4

OLS Estimates of the
Effect of UI Taxes on
Firm Average Earnings and Employment

Dependent Variable	ΔMarket-Level Tax Rate	ΔFirm-Level Tax Rate	Calendar Quarter Only	Calendar Quarter by State	Number of Obs	R^2
(1) ΔLn(Earnings)	-2.161	0.387	YES	NO	91257	0.008
	(0.434)	(0.277)				
(2) ΔLn(Earnings)	-1.774	--	YES	NO	91257	0.007
	(0.334)					
(3) ΔLn(Earnings)	--	-0.493	YES	NO	91257	0.007
		(0.213)				
(4) ΔLn(Earnings)	-0.826	0.387	NO	YES	91257	0.052
	(0.637)	(0.271)				
(5) ΔLn(Earnings)	-0.439	--	NO	YES	91257	0.052
	(0.576)					
(6) ΔLn(Earnings)	--	0.238	NO	YES	91257	0.052
		(0.245)				
(1) ΔLn(Employment)	2.920	-2.363	YES	NO	91257	0.005
	(0.632)	(0.434)				
(2) ΔLn(Employment)	0.557	--	YES	NO	91257	0.004
	(0.487)					
(3) ΔLn(Employment)	--	-1.173	YES	NO	91257	0.004
		(0.311)				
(4) ΔLn(Employment)	1.383	-2.363	NO	YES	91257	0.031
	(0.938)	(0.399)				
(5) ΔLn(Employment)	-0.979	--	NO	YES	91257	0.030
	(0.849)					
(6) ΔLn(Employment)	--	-2.113	NO	YES	91257	0.031
		(0.361)				

Note: Changes are annual changes calculated using quarterly data (i.e 1983:1 - 1982:1, 1983:2 - 1982:2, 1983:3 - 1982:3, 1983:4 - 1982:4, etc.).

IMPROVING EMPLOYER COMPLIANCE WITH UNEMPLOYMENT INSURANCE TAX REPORTING REQUIREMENTS

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ABSTRACT

Using a unique data set, we have demonstrated in previous work that employer noncompliance with Unemployment Insurance (UI) tax reporting requirements is potentially widespread and costly to the UI system (Blakemore, Burgess, Low and St. Louis, 1993). Nationally, we estimated that employers underreported over $70 billion of total wages and $728 million of UI taxes for 1987. Our empirical results from this earlier paper also show that the noncompliance conforms to theoretical predictions derived from an economic model of deliberate tax evasion. Since employer tax evasion appears to be systematic, it is conceivable that statistical profiles of employer reporting tendencies can be designed to detect the noncompliance of individual employers. In this paper, we use the same data set to investigate systematic noncompliance by performing in-sample simulation experiments. Specifically, we test the cost effectiveness of various statistical profiles (based on our economic model of tax evasion) for allocating auditing resources. We also compare the results of targeting audit resources on "high-risk" firms identified in our statistical profiles to the results of using alternative auditing strategies, including random auditing which is commonly used by most states. We find that statistically-based systematic profiles perform far better than currently practiced auditing strategies in detecting large amounts of unreported tax liabilities. We also show that some model features, which are data intensive (i.e., costly), are very valuable in detecting noncompliance. Finally, in the context of our sample, we compute cost effective audit penetration levels -- the point in the distribution of firms selected for audit beyond which the expected cost of additional audits exceeds the amount of additional taxes expected to be collected. By using similar profiles, the UI System potentially could collect substantial tax revenue that is currently unreported, as well as induce many more firms to voluntarily comply with UI tax reporting requirements to avoid detection. It is important to note that improved employer compliance also is important for other reasons, such as reducing both incorrectly denied claims for UI benefits (due to employer underreporting) and unpaid worker compensation premiums.

I. INTRODUCTION[1]

Burgess and St. Louis [1990], Burgess, Blakemore and Low [1991] and Blakemore, Burgess, Low and St. Louis [1993] previously addressed employer compliance with the tax reporting requirements of the UI system.[2] Specifically these studies variously find that, based on a sample of Illinois employers, underreporting of employer tax liabilities is a major problem for the UI system. Moreover, the underreporting that was detected conforms with theoretical predictions from an economic model of deliberate tax evasion fully developed in the latter study (BBLS, hereafter).

Intentional tax evasion poses many problems for the UI system, not the least of which is a substantial drain from the FUTA trust fund. For the nation, BBLS estimate that 11.1 million UI eligible workers and $70.6 billion in total wages are not reported to UI state agencies by their employers. This causes an estimated $728 million of UI taxes to go uncollected <u>annually</u>. Closing a revenue gap of this amount obviously is important to the UI system. It also is in the best interests of complying firms and workers to close the revenue gap. The failure of employers to accurately report worker UI eligibility causes workers to encounter difficulties collecting (and UI agencies difficulties in correctly paying) benefits to which workers are entitled. Additionally, by misreporting their tax liability, noncompliant employers shift the tax burden to complying firms. This places compliant firms at a competitive disadvantage and may breed general discontent with the UI system.

The purpose of this paper is to develop multivariate models of employer noncompliance and analyze which characteristics of employers are statistically associated with the amount of underreporting.[3] The parameter estimates from alternative models are then used in conjunction with the cumulative probability density functions implicit in the statistical specification to calculate a predicted value of various measures of noncompliance for each firm in the sample, such as: (1) the expected probability of underreporting taxable wages; (2) the expected amount of unreported taxable wages; (3) the expected amount of taxable contributions due (unreported taxable wages evaluated at the employer's tax rate); and (4) the expected net audit yield (expected additional contributions due less the expected cost of auditing). Finally, employers are arrayed from those with the largest expected amounts of misreporting to those with the smallest (or no) expected noncompliance. We refer to this as a profile. Obviously, an ideal profile that correctly ranks all employers is not a practical possibility. Instead, a realistic goal is a profile that effectively identifies reporting accuracy, on average, even though some large errors may occur in ranking individual employers. Then, depending on the costs of conducting audits, UI agencies could select firms for audits so as to maximize the expected net audit "return" from any level of audit resources used.

If underreported taxable wages were randomly related to employer characteristics, the type of profiling described above would not be a useful management tool. But when much of the underreporting is intentional, as BBLS suggest, then statistical profiles of employer reporting tendencies are potentially useful in detecting deliberate or systematic noncompliance. Indeed, the economic model of tax evasion developed in BBLS suggests that firms weigh the costs and benefits of tax evasion when deciding on their reporting strategy. To the extent that most states rely on random auditing with low audit penetration rates, firms know that the probability of incurring a cost for noncompliant behavior is low. From this perspective, profiles of noncompliant firms can improve significantly the collection of UI taxes in two ways. First, UI agencies may be able to detect and collect a large proportion of the taxes that continue to go unreported. Second, employers may be induced to voluntarily report the correct amount of taxable wages to avoid the more certain detection of tax evasion that results from using the profile.

In the process of experimenting with various statistically-based profiles, we compare the detection efficacy of the models. It is one thing to show that variables used in an empirical model have theoretically correct impacts and appropriate statistical inferences, as is done in BBLS. But it is entirely possible that a model with these statistical characteristics performs poorly when used for prediction purposes -- in this case, accurately identifying tax evading employers. We use traditional in-sample tests of the predictive capability of the profile models. The efficacy of a model is judged by ordering firms from those with the largest predicted amount of unreported taxable obligations measured in various ways, to those with the lowest. Then, we compare the predicted with the actual reporting accuracy, provided in the detailed auditors' analysis we have for each firm in the sample.

Two types of sensitivity analysis are performed. First, we analyze alternative model specifications to identify the variables most important for detecting tax evasion. Second, we analyze the detection efficacy of alternative policy variables (alternative measures of noncompliance) upon which the simulations are performed. We find that the UI tax rate, in conjunction with two key variables (the firm's turnover rate and the percent of its work force paid as independent contractors), are important in identifying tax evaders regardless of how noncompliance is measured; and that a statistical profile using these variables, among others, is far superior to random auditing or other auditing strategies that do not rely on economic modelling and statistical profiling. Through in-sample simulations, we also calculate cost effective audit penetration levels -- the point in the ranked distribution of firms selected for audit beyond which the cost of additional audits exceeds the amount of additional taxes expected to be collected. This exercise, probably more than any other, demonstrates the administrative effectiveness of statistical profiling models as an auditing strategy. We conclude with a discussion about the policy implications of the economic model and corresponding simulations. The discussion emphasizes how the costs and benefits of UI tax evasion relate to detection probabilities and noncompliance penalties.

II. DATA SOURCES

A. Construction of the Data Set

The main data source used for this project comes from a sample of 875 firms that form a stratified sample of the population of Illinois firms in 1987. The data contain information on both noncompliance and detailed firm characteristics, allowing us to analyze the nature of noncompliance for firms with varying characteristics. Among the firm characteristics available in the data set are routine information collected by UI agencies, such as the employment size of the firm, the statutory UI tax rate for each firm, one-digit SIC codes and UI reporting punctuality. In terms of noncompliance, the data set contains nonroutine information, collected through intensive and specialized audits, on unreported workers, underreported (total and UI taxable) wages, and UI taxes due on underreported taxable wages. In addition, the number of employees reported by each firm in the sample as independent contractors to the Internal Revenue Service was obtained and merged with the Illinois UI data.

The data we analyze were constructed for a study by Burgess and St. Louis [1990], who provide a detailed discussion of the study population and how the study sample was selected. Briefly, the "main' sample of regular firms was a systematic sample selected by a computer routine from a file sorted by industry, tax rate and company size. This sorted file was used to ensure that the sample was representative of the 1987 population of Illinois firms in terms of potentially important characteristics for investigating employer reporting accuracy. The precise details on how the systematic samples were selected are provided in Appendix I of Burgess and St. Louis [1990]. Ideally, the population should include all employers who meet the Illinois definition of a covered employer, including those who have not identified themselves for IDES reporting purposes. However, identifying the latter employers, particularly those in the "underground" economy, was beyond the scope of the study. The other notable exclusion from the sample is that the audits were not conducted for firms with pertinent UI accounting records located outside of Illinois. Thus, the audit information analyzed in this report relates specifically to the population of covered employers with tax records located in Illinois as of the third quarter of 1987; and the firms selected for the systematic samples were representative of this 1987 target population with respect to certain key characteristics.

B. Statistical Reliability of Population Estimates and the Study Data

Comprehensive procedures were adopted by the Illinois Department of Employment Security (IDES) to ensure accurate data for our sample. First, consistent audit procedures/policies were developed for audits conducted throughout the state; and, because of an emphasis on thoroughness and consistency, IDES made several changes in audit practices that were important in ensuring uniform results through the state. The entire IDES Operations Bureau made extraordinary efforts to complete extremely thorough audits for this special study. Second, the completed audit findings for each firm were subjected to an intensive review process to ensure consistency and accuracy. Each audit was reviewed carefully by the auditor who completed the work, the auditor's immediate supervisor and by a Central Office Review Team. This latter review step involved at least two audit supervisors reviewing every case. When questions arose at any point in this review process, the accuracy of any questioned item was verified. Moreover, data coding accuracy was verified carefully by IDES. Finally, computer routines were used to perform numerous checks on the data received from IDES. Overall, the review/data verification procedures utilized to construct this sample were unusually comprehensive, even for special studies such as this one.

Given the stratified nature of our sample, it is necessary to weight sample data to obtain representative results for the population. In this case, weights were assigned so that the probability of sample inclusion was proportional to the number of employees in the company. The procedure for assigning weights, the specific weights for each firm sampled and considerable detail on these and other sampling issues are provided in Appendix I of Burgess and St. Louis [1990].

We believe our results provide a sound basis for assessing noncompliance in Illinois, and can be extended to the nation since reporting requirements as well as noncompliance detection probabilities and penalties in Illinois are typical of those in other states. Moreover, a major form of noncompliance -- treating actual employees as independent contractors for tax purposes -- is associated with federal reporting requirements that evidently are subject to routine detection procedures by either very few or possibly no state agencies.

C. The Extent of Employer Noncompliance

Summary data on Illinois employer reporting errors for 1987 are reported in BBLS and far more detail is found in Burgess and St. Louis [1990]. A very high percentage (45 percent) of all employers in the Illinois sample are estimated to make some underreporting error. This includes almost 500,000 worker cases that employers erroneously excluded from their reports on covered UI employees to IDES. These reporting errors by Illinois employers caused total earnings to be underreported by an estimated $2.6 billion while unreported UI taxable wages amount to more than $1 billion.

Not surprisingly, the propensity to make a reporting error (of at least a small amount) is highest for large firms (firms with at least 500 employees). Seventy-six percent of the large firms made some reporting error, compared to no more than fifty-six percent of the firms in any other size category. But in proportional terms, underreporting of workers and wages is considerably more significant for smaller than for larger firms. Smaller firms in Illinois underreport 14 percent of their taxable wages and 56 percent of their UI covered work force, whereas larger firms have errors of less than one percent in each category.

The data also provide detail about the sources of misreporting. As shown below in the profile simulations, this information is very important for purposes of detecting UI tax evasion. The main source of unreported wages and workers for Illinois employers is misclassifying workers as independent contractors. (The other forms of misreporting are failure to file reports or failure to report casual or part-time workers, tips, bonuses, other types of irregular compensation, and other minor items.) Workers classified as independent contractors are removed from regular payroll reports that employers provide to government agencies, including the UI reports, and instead are issued IRS 1099 forms. This misclassification by employers accounts for almost 50 percent of all unreported workers found in the Illinois sample. Note that, besides avoiding UI taxes by issuing 1099 forms, employers also may save the employer's share of FICA contributions to the Social Security System and premiums for Workers' Compensation on underreported earnings.

III. A GENERAL MODEL OF EMPLOYER UI TAX EVASION

Considerable attention has been given to the economic basis for both criminal activity and routine noncompliance with various laws and regulations by firms and individuals. The fundamental economic concept of this analysis is that intentional noncompliance decisions reduce to a comparison of the expected benefits and the expected costs of noncompliance. From this perspective, noncompliance activity is "rationally" considered. The notion of benefits and costs can be generalized to include a broad spectrum of factors. It includes the obvious factors such as the expected penalties, as well as subtle (unmeasurable) consideration such as the value of reputational capital. Since neither costs nor benefits can be known with certainty, this framework of analysis requires that both be put into probability terms. When considering noncompliance, the <u>expected</u> benefits of the action must be weighted again the <u>expected</u> costs.

The original statement of the above general model is found in Becker [1968]. Although there have been many extensions and applications of the model since Becker's classic article, of particular interest is the study by Ashenfelter and Smith [1979] that analyzes employer noncompliance with minimum wage laws. Studies of personal income tax evasion (see for instance, Allingham and Sandmo [1972], Yitzhaki [1974], Feinstein [1991] and Alm, Bahl and

Murray [1990]) also provide considerable guidance for our analysis.

The employer model analyzed here follows the general approach found in the above studies of compliance, and a formal theoretical derivation of the employer's UI noncompliance decision is provided by BBLS [1993]. An employer, making the decision whether to comply with UI regulations, compares the benefits of retaining the taxes owed to the UI system with the expected costs of that action in a profit maximizing framework. Costs include (among other factors) a judgment about the probability of being audited, the penalty if detected, and any losses in reputational capital and auditing costs resulting from detection. The tax advantage gained from evasion is expected to depend positively on the UI tax rate -- at higher tax rates, all else constant, the gains from concealing a dollar amount of taxable wages is expected to increase.

IV. EMPIRICAL ESTIMATES OF THE TAX EVASION MODEL

We estimate our main noncompliance models with the tobit maximum likelihood procedure as in the income tax evasion models of Clotfelter [1983], Alm, Bahl and Murray [1990] and Feinstein [1991].[4] Tobit is applicable when the dependent variable is truncated or censored at some limiting value, either artificially by experimental design, or naturally by the type of data. In our context, the research problem involves data where all possible measures of noncompliance are naturally truncated at zero (a large cluster of firms have no underreporting), but there also is a full range of continuous values above zero. From a conceptual perspective, tobit is applicable when the process that forms the dependent variable involves simultaneous events as opposed to two discrete independent decisions. The noncompliance decision considered in this paper plausibly can be regarded as a simultaneous decision about underreporting UI taxes, the amount of which is determined by calculating the level of evasion that maximizes profits according to expected costs and benefits. As such, our problem seems to fit the spirit of the tobit technique quite well, allowing us to circumvent identification difficulties encountered in other approaches such as two stage estimation procedures.

Because the original sample in Burgess and St. Louis [1990] was stratified to ensure representation of the various types of firms found in the population, we assign an appropriate population weight to each employer observation to make the sample representative of the Illinois population of firms. Thus all regression estimates reported below are weighted.

A. Model Specification

When choosing variables for the specification of the empirical model, we consider the value of the variables for: policy matters (i.e., collecting unreported taxes); their theoretical suitability as measures of costs and benefits of noncompliance; and their affordability. In all but two cases, variables are constructed from data that are more or less routinely available within the UI data system. The two exceptions are discussed below, and their importance is demonstrated later by comparing simulations with and without the variables.

As the dependent variable, we have chosen unreported taxable wages (UTW) for theoretical reasons as well as for its flexibility. However, from a policy perspective, the net amount of taxes owed on unreported taxable wages for any firm, referred to as additional-contributions-due (ACD), may be the variable of ultimate interest to UI administrators. For a firm with no gross refund due, ACD is calculated simply by multiplying UTW by the employer's UI tax rate.[5] Since this calculation can be performed easily when constructing the statistical profiles after the estimation of UTW, the choice between UTW and ACD as the dependent variable reduces to a decision of whether the tax rate belongs on the left hand side of the equation or the right hand side as an explanatory variable. Fortunately, theory can be a guide in this decision. As already discussed, we have shown in BBLS that tax rates alter the employer's compliance decision, thus making the tax rate an explanatory variable and UTW the preferred dependent variable.

Now, the stochastic model can be written as:

$$(1) \qquad \begin{array}{ll} UTW_i = X_i\beta + \varepsilon_i & \text{if } X_i\beta + \varepsilon_i > 0 \\ UTW_i = 0 & \textit{otherwise,} \end{array}$$

where X_i is a vector of explanatory variables that represent firm i's expected costs and benefits from misreporting, β is a vector of coefficients and ε_i is an independent, normally distributed error term with zero mean and constant variance. We follow the usual approach in previous empirical research on income tax evasion and estimate (1) directly from a sample of audited tax returns, assuming all tax evasion has been detected by auditors.[6]

The independent variables included in the X vector can affect the cost-benefit calculation by either changing the probability that a benefit (cost) will be received (incurred), and/or changing the conditional expected dollar amount of the benefit (cost). A listing of the variables is contained in Table 1.

B. Estimated Results

The results of estimating various models of UI tax evasion are shown in Table 2. The tobit results are reported in Table 2a, and the corresponding partial effects evaluated at the mean values of the variables are reported in Table 2b. For estimates of our main model, we use tobit procedures on: UTW with a full set of variables; and a restricted version of the full model in which TURNOV and/or 1099% are removed from the set of explanatory variables because these data are more difficult to obtain.[7] Table 2 also contains tobit results for the proportional amount of UTW, as in BBLS, for the full set of variables, as well as probit results on UTW. In each case, the models are alternatively estimated with the statutory tax rate (STATTAX) or the effective tax rate (EFFTAX) as a key policy variable.

In general, the variables are interpretable from the perspective of the expected costs and benefits of noncompliance. Five of the variables are expected to have a positive influence on underreported taxable wages by increasing the probability of noncompliance or its expected magnitude. These are DELINQ, 1099%, TURNOV, employer size, and the UI tax rate.

DELINQ, a delinquency rate for filing payroll reports, indicates a propensity to disregard or at least not promptly respond to UI regulations. It may represent an element of risk-taking on the part of the employer, or simply poor bookkeeping practices. In either case, it should increase noncompliance. As expected, DELINQ is always positive in Table 2, although its level of significance is low when the full set of variables are included in the main tobit results, referred to as our "base" model.

1099% is an indicator of the firm's tendency to pay workers as independent contractors. As already discussed, misreporting employees as independent contractors on 1099 forms was a major factor in the underreported taxable wages detected by IDES auditors (workers who receive IRS 1099 forms are not reported as employees subject to the employer's UI tax). 1099% is always positive and its statistical significance is generally high.

Another variable expected to have a positive effect on UTW is employer size. Larger firms will have greater amounts of UTW for a given propensity to evade taxes. There may be a limit, however, with respect to firm size. The largest firms likely have fewer inadvertent compliance problems because of more routinized reporting procedures or better accounting systems. It also may be the case that the largest firms are more often unionized and, hence, tend to report workers and wages more accurately because of potential union scrutiny. Finally, large firms may desire to avoid any adverse impacts on their reputational capital from detected underreporting. It is much more likely that detected noncompliance will be publicized more widely for large firms than for small ones. To accommodate any nonlinearities of this sort, we include SIZESQ. Again, the estimated results conform to our expectations. The only exceptions are for the two models that have the percent of UTW (unreported taxable wages as a percent of total taxable wages) as the dependent variable. In these cases, the effect of size has been normalized, and the statistical significance of the coefficients for SIZE and its squared term are quite low, not surprisingly.

TURNOV is a crude measure of employee turnover. It is expected that firms with high turnover rates have greater noncompliance incentives because a larger proportion of their total payroll consists of taxable wages (wages that do not exceed the taxable maximum) than is the case for otherwise similar firms with low turnover rates. Possibly reinforcing this effect is the greater difficulty of accurately reporting all wages for high turnover firms. Without exception, TURNOV is positive and highly significant in our estimation of noncompliance. However, there is an interesting interaction between TURNOV and EFFTAX, a point we return to shortly.

QUARTERS, the length of time a firm has been in business, has a theoretically uncertain effect on the probability of noncompliance. It indicates how recently a firm has been established under current ownership, and it can be regarded as an indicator of how familiar a firm is with UI reporting requirements. As such, firms with longer histories are expected to be more familiar with reporting requirements and perhaps less likely to make errors. Alternatively, a firm may lower its expectations of having any noncompliance detected the longer a firm has been in existence without encountering an audit, thereby raising the probability of noncompliance. QUARTERS is always negative but statistically insignificant when TURNOV and 1099% are included in the base models in Table 2. However, it does become significant in cases without one or both of these variables, perhaps due to collinearity problems.

The industry variables are used to control for otherwise unobservable employer characteristics. We have no expectations about the manner in which they influence the expected benefits and costs of noncompliance. After controlling for the other variables in Table 2, our results show there is a tendency for construction firms to comply more fully with UI tax regulations, in contrast to a tendency for firms in the transportation industry to underreport their taxable wages. No other industry variable attains statistical significance with much regularity.

Finally, we consider the effect of tax rates on noncompliance. As already discussed, the effect of tax rates on evasion is expected to be positive. In the current study, we experiment with two measures of the UI tax rate -- the firm-specific statutory tax rate assessed by the UI system and the effective tax rate. The effective tax rate is defined as the total UI taxes due for a year divided by total annual earnings of all workers employed sometime during the year. The distinction between the two measures may be important because limitations on the earnings subject to UI taxes ($8,500 in Illinois in 1987) causes the statutory and effective tax rates to diverge. Firms that have higher average wages have a lower percent of their payroll subject to the tax, lowering the effective tax rate for any given statutory rate. Employee turnover is an important determinant of the average wage paid to covered workers over the course of a year since workers terminated prior to the end-of-year report are less likely to exceed the UI earnings limitation for the year. As such, we expect EFFTAX to perform better in models that exclude TURNOV, one of the two variables that cause some data collection difficulties for UI agencies.

The results support our expectations. With the sole exception of the base tobit model that utilizes EFFTAX, the firm-specific UI tax rate, however measured, has a statistically significant and positive impact on noncompliance. For the one exception that we found, it is clear that collinearity between TURNOV and EFFTAX has diminished the significance of EFFTAX, as speculated above, although EFFTAX is appropriately signed.

It is safe to conclude that the results in Table 2 confirm that employer tax evasion is not merely a random occurrence perhaps reflecting complex UI reporting requirements. Rather, employer underreporting is systematic. The next step is to use the parameter estimates from Table 2 to construct simulated estimates of noncompliance for each firm in the sample from the characteristics of the density functions; and then to compare projected values of the various measures of noncompliance with actual auditor reports for each firm in the sample. Of course, the ultimate test of the model is its ability to forecast out-of-sample. But data are not available to permit such a test. Nonetheless, note that the validation process attempted here is not a trivial matter. Having an empirical model that yields theoretically correct statistical inferences does not imply that the model can forecast well, even in-sample. In our case, for example, finding statistically significant explanatory variables for UTW is much different than using the model to identify the firms with the largest relative expected amounts of UTW. The latter requires more overall precision from the estimated tobit function than the former.

V. THE DETECTION EFFECTIVENESS OF STATISTICAL PROFILING

The empirical models reported above can be useful as a management tool only if they are "successful" in identifying the firms least (most) likely to comply with the UI reporting requirements, and/or those with the most (least) additional contributions due (ACD). Importantly, a successful profile can be used either for preventing employer underreporting by identifying high-risk firms that need education/assistance, or cost effectively detecting underreported wages and taxes through targeted audits.

A traditional test of the predictive capability of profile models is provided by in-sample simulations. For our purposes, the stochastic models from Table 2 are utilized to calculate three policy variables for use in selecting firms for audit. Two are indicators of noncompliance and a third is a measure of auditing efficiency. First, we compute each firm's expected dollar amount of underreported taxable wages, referred to as E(UTW), from the estimated models in Table 2. We also calculate expected additional contributions due, referred to as E(ACD), for these simulations by multiplying the UI tax rate for each firm by its expected UTW. Finally, we incorporate a measure of expected UI agency costs of conducting audits to identify firms predicted to have the highest expected net yields (expected ACD minus expected costs). The expected cost is formed from a regression of auditing hours on firm size for the 875 sample firms. The Appendix provides the method of formulation for expected auditing costs. Our audit cost estimates include only state agency costs, and we do not consider firm audit costs. "Optimum" audit penetration rates from a social viewpoint would consider private as well as public costs, but we have no estimate of private costs.

The calculations for these policy variables are then matched against the firm's actual amounts of unreported taxable wages, additional contributions due or net audit yield, as determined by the IDES auditors, to evaluate the relative predictive power of the various models.[8] The efficacy of our noncompliance models in terms of detecting noncompliance also are compared to three possible benchmarks: a process of random audits over the entire sample; audits conducted for the largest firms according to the number of employees reported for 1987.3; and audits based on the estimated probability that a firm is in noncompliance with the UI tax reporting regulations (from the probit results). Although none of the benchmarks presumably would be useful in assigning all audit resources, all are interesting for comparison. The benchmark based on random auditing is interesting because it is the predominant audit strategy used by UI agencies. The large firm comparison is useful simply because large firms may have relatively large underreported dollar amounts, even if those amounts represent only a small percent of the firm's total taxable wages. And the strategy that targets auditing resources on firms with the greatest likelihood of underreporting conforms with an auditing philosophy that stresses the need to discourage noncompliance in general, disregarding the magnitude of the underreporting or the agency costs of the audits.

In the simulation profiles reported below, we aggregate the 875 firms from our sample into groups of 58 firms each (except for the 15th group which contains 63 firms), ranked from highest to lowest in terms of the respective criteria being investigated.[9] Because we are interested in an effective management tool for selecting firms for audit, we analyze a large number of alternative profiles. For various noncompliance specifications displayed in Table 2, we analyze the profile's detection effectiveness in terms of simulations of the three policy instruments, E(UTW), E(ACD) and E(NAY), for these 15 groups of firms. From these simulations, we hope to determine which model specification (in terms of the choice of dependent variables and the choice of independent variables) performs best; and which of the policy instruments used to select firms for audit is most effective.

B. Benchmark Strategies

Table 3 contains the results expected for an auditing strategy that randomly selects firms for audit, the most common method of assigning firms for audit in the UI system. In this case, there is no criteria for assigning firms to one of the 15 groups -- the assignment is purely by chance. As seen in the table, randomly assigning firms for audit does not detect noncompliance effectively, regardless of how compliance is defined. Aggregating 58 firms with "average characteristics" into GROUP 1 accounts for only 6.6% (the average amount) of the outstanding UTW and ACD. Possibly more importantly, the costs of the audits exceeds the additional tax revenue received from the audits. Net audit yield for these 58 firms is a loss of $12,115.

Another ad hoc strategy, selecting firms for audit by size, is more effective than random auditing. As seen in Table 4, the 58 largest firms from our sample that make-up GROUP 1 are responsible for $5,760,876 of UTW, 26.1% of the total UTW attributable to these 875 firms. The additional contributions due is nearly as large in percentage terms. However, it is extremely costly to audit large firms. While the additional contributions due from firms in "GROUP 1" is

$178,767, the UI system nets only $15,367 after auditing costs. And if the 58 next largest firms (GROUP 2) are audited, the system will spend more on the 116 audits than it collects in taxes, in spite of detecting 41.6% of all UTW in the sample.

Our third benchmark -- selecting firms for audit with the highest predicted likelihood of noncomplying -- is actually a sophisticated strategy in a statistical sense. As such, we use it for comparison with the other statistically-based strategies reported below; and do not suggest that it is "ad hoc" in the same sense that the first two auditing strategies are. The probit results on UTW from Table 2 are used to form this simulation profile. GROUP 1 firms are the 58 firms (6.6% of the sample) with the highest predicted probabilities of having unreported taxable wages; Group 2 contains the 58 firms with the next largest expected probabilities; etc. This procedure, reported in Table 5, clearly performs better than random auditing for all evaluation criteria; and also outperforms the "large-firm strategy" in terms of net audit yield once audit penetration extends beyond the 58 firms in GROUP 1. Thus, it appears that use of some systematic profiling procedure to identify "high risk" firms is preferable to the two ad hoc strategies. In terms of the probit profile itself, the specification using the statutory tax rate (top panel of Table 5) is slightly preferred to the specification using the effective tax rate (bottom panel of Table 5).

C. Profiles from the Full Base Model

Tables 6, 7 and 8 contain the simulation profiles for our "base" models. The profiles are formed from the tobit specification using the amount of UTW as the dependent variable and all of the independent variables from Table 1. Table 6 reports the profiles that select firms for audit based on the expected amount of UTW. GROUP 1 firms are the 58 firms (6.6% of the sample) with the largest predicted <u>amounts</u> of unreported taxable wages; Group 2 contains the 58 firms with the next largest values for E(UTW); etc. The results utilizing the statutory tax rate are displayed in the top panel of Table 6. The bottom panel provides directly comparable results for the specification utilizing the effective tax rate.

In terms of their efficacy in detecting actual (audited) amounts of UTW or ACD, the choice between the statutory UI tax rate and the effective UI tax rate appears not to matter. Both profiles detect similarly large amounts (and percents) of UTW and ACD; and both perform far better than random audits. Firms ranked in GROUP 1 have $5,792,000 of actual UTW accounting for 26% of the UTW found by IDES auditors for the 875 firms in the sample; and approximately $170,000 (22%) of all ACD in our sample. Beyond the audits conducted on the GROUP 1 firms, these profiles also outperform a strategy that simply selects the largest firms for audit. However, this profile does no better, in general, than the probit profile from Table 5. In terms of actual net audit yields, the model with EFFTAX has a somewhat higher return than the profile using the statutory tax rate for GROUP 1 firms, but far lower for GROUP 2 firms. This reflects the sensitivity of our simulations to "detecting" a single firm with large amounts of unreported taxable wages. Both specifications significantly improve the cost effectiveness (actual NAY) of auditing large firms beyond the limited number of audits involved for GROUP 1 firms. Finally, according to the results in Table 6, audit penetration rates that go beyond GROUP 3 firms are not cost effective. The cumulated NAY declines beyond that point indicating that the additional costs of the audits exceeds the additional tax revenue that is owed by the firms (on average) for audits conducted beyond GROUP 3.

The simulations found in Table 7 are formed from the same empirical specification as in

Table 6 (the base model), but the simulation results are for an auditing strategy based on predictions of additional contributions due (i.e., GROUP 1 contains the 58 firms from the sample with the largest predicted amounts of ACD). This profile modifies the previous one by incorporating the UI tax rate into the auditing decision. In the ACD profile, a firm with a large expected amount of UTW may not be highly ranked if its UI tax rate is sufficiently low. Once again, measuring the tax rate with STATTAX or EFFTAX provides little basis for choice. But either profile specification dramatically improves on the performances of all previous auditing strategies across all possible criteria for evaluation -- the amount of UTW or ACD detected, and the net audit yield collected from the audits. The E(ACD) profile clearly seems to be a superior policy instrument compared to E(UTW). This is especially true when considering the net audit yield. For audits performed on GROUP 1 and 2 firms, the net audit yield improves tremendously for the profiles based on E(ACD). Auditing the 58 "highest risk" firms (6.6% of the sample) detects more than 30% of all additional contributions due, and increases the net audit yield by a magnitude as much as ten for GROUP 1 firms when compared to the simulations for E(UTW). The improvement in net audit yields for GROUP 2 is equally impressive. Audit penetration beyond GROUP 4 firms is not cost effective in this profile; and in fact, there is very little return to auditing firms beyond GROUP 2. However, UI agencies may prefer higher audit penetration rates at zero net cost if it raises employers' assessment of being audited, and induces greater voluntary compliance.

The final "base" profile (tobit model using the amount of UTW as the dependent variable and all of the independent variables) involves simulations on expected net audit yield (Table 8). The model modifies the previous one by also taking account of the <u>expected</u> cost of auditing each firm. Now a firm with large amounts E(UTW) or E(ACD) may not be highly ranked if the expected auditing cost is large relative to the E(ACD).

Again, the measure of the UI tax rate is not crucial, although there seems to be some slight grounds for preferring the statutory tax rate in these simulations. But in terms of policy instruments, the results are striking -- E(NAY) is unequaled in effectively targeting audit resources. Selecting the highest rated 58 firms for audit based on their predicted net audit yield increases the actual amount of ACD that is detected to about 40% of all additional contributions due in the sample from the previous high of 31%; and raises the net audit yield on these 58 firms by more than $100,000. In fact, the profile is so precise in its ability to detect noncompliance in a cost effective manner that auditing firms beyond GROUP 1 has little actual payoff; and auditing beyond GROUP 2 dramatically reduces the net audit yield. This profile is both highly effective at detecting a large percent of UI tax avoidance and highly productive in terms of the auditing resources it utilizes.

In all of the above profiling results, it should be stressed that the profiles do equally well in another dimension -- they also accurately predict the firms that tend to comply. For instance, for the profile based on E(ACD), firms ranked in the last five groups (more than 33% of the sample population) have only 5% of the actual additional contributions due detected by auditors for the entire population. In short, the model is highly effective in identifying both types of firms important for maximizing the audit yield from any level of auditing resources -- the few with a large percentage of all misreporting and the many with a very small percentage. Note also that it is not until the final five GROUPS are reached that the cumulated net audit yield turns negative, unlike the first two benchmark strategies that exhibit negative returns very quickly.[10]

Of course, this does not imply that all "large noncompliers" are located in the first several groupings of any of these profiles. There are firms that evade large sums of taxes and do not "fit the profile" well. Some large noncomplying firms also may avoid an audit in the profiles of Table 7, based on E(ACD), and Table 8, based E(NAY), if their UI tax rate is sufficiently small and/or their expected costs of audit is sufficiently high. Conversely, some firms that have the profile of a major tax evader, subsequently are found in full compliance. Indeed, the predicted value for any of the policy instruments often differs considerably from the actual value on a firm-by-firm basis. This type of difficulty in accurately predicting the magnitude of misreporting for any single firm is anticipated for models of this type. But that is not the purpose of our profiling exercise. Instead, we hope to identify groups of firms with relatively high levels of noncompliance on average, and the profiles do that quite successfully.

Before preceding to new simulations, a final clarifying comment about the results may be useful. As is obvious from the comparisons with the "large firm" simulations, firms of many different sizes are detected with high levels of noncompliance in our "base" profiles. In the highest risk category for the E(NAY) profile based on STATTAX, for example, 15 firms with fewer than 5 employees were selected for audit, while 6 firms employed more than 10,000 workers. Furthermore, firms in GROUP 1 with fewer than 10 employees provide 19% of the actual NAY for this group; firms with fewer than 50 workers contribute 30% of the net audit yield; and firms with fewer than 200 employees contribute almost 70%. This is an important matter from a policy perspective. A profile that mainly selects firms for audit based on any single identifying characteristic (such as large size) may encounter substantial political resistance from groups that possess the characteristic.

D. Simulation Results For Restricted Models

Having established the viability of the profiling strategy in our base models, we now turn to an alternative specifications. In this section, we continue to use a tobit specification with the amount of UTW as the dependent variable. But we investigate the predictive ability of our noncompliance model without 1099% and /or TURNOV, the two variables that are costly to collect for most UI state agencies. Although profiles based on the restricted models clearly outperform the benchmark auditing strategies of random auditing and "large-firm auditing", the results are not as strong or as consistent across all comparisons as found for the full model.

For all four profiles that we investigate, Tables 9-12, the restricted models are less effective at detecting noncompliance then are the comparable full models. This is particularly true when measuring success according to the net audit yield. Indeed, based on the E(UTW) profile using the effective tax rate (lower panel of Table 9), state UI agencies would incur auditing costs greater than the taxes received if auditing is extended into GROUP 2 firms. The restricted model specification without 1099% and TURNOV is more successful for simulations of ACD and expected net audit yield, but still never approach the degree of success found for the full base model. As for the choice between the effective UI tax rate and statutory UI rate, we conclude that there is no clearly preferable specification. The relative effectiveness of the specifications changes unsystematically according to the criteria of success being considered (the amount of UTW or ACD detected or the amount of NAY), and according to the policy variable chosen for the simulation -- E(UTW), E(ACD), or E(NAY).

Table 12 provides simulation results for the most successful restricted-model profiles -- simulations on expected net audit yield in which 1099% is included, but turnover is not. One thing that is particularly noteworthy in these simulations is the close similarity between the results for the specification using the statutory tax rate and the specification with the effective tax rate. Without TURNOV, the effective UI tax rate does "more of the work" for which it was designed -- accounting for the bias in the statutory UI tax rate caused by the taxable earnings restrictions, as discussed above. However, it still does not outperform the comparable simulations for the statutory tax rate.

Another notable result is that the profiles in Table 12 generally do far better than the profiles that also exclude 1099% (Table 11), indicative of the predictive ability of the 1099% variable. But by the same token, the results do not come close to matching the efficacy of the full base model. Apparently, the turnover behavior of employers is a good predictor of misreporting tendencies, independently of its effect on the statutory tax rate. It is unknown whether TURNOV is "picking-up" the effect(s) of missing variable(s) or has predictive abilities of its own.

To summarize this section, the variables TURNOV and 1099% add considerably to the detection power of the profiles. Recall that neither variable is routinely available in state agency files. The TURNOV variable requires some extra programming to identify the number of different employers reported annually by firms, but the programming is fairly simple. Constructing the 1099% variable requires information on a data tape that can be obtained annually from the Internal Revenue Service. Thus, including the TURNOV and 1099% variables in the profiles involves extra programming and expense. Although we have no precise estimate of the extra cost involved in constructing these two variables, the extremely large increase in the effectiveness of the profiles certainly seems to justify the added cost.[11]

E. Simulations from the Tobit Model Using %UTW

The simulations presented in Tables 13-16 provide an alternative approach that may have additional political appeal for UI administrators. The regression models that generate these profiles are the tobit estimations in Table 2 using the percent of UTW (UTW as a percent of total taxable wages) as the dependent variable. The advantage offered by this specification is that it normalizes for SIZE, deemphasizing it as a factor for selecting firms for audit.

In general, the simulation results are very impressive. (As before, there is little reason to favor the statutory UI tax rate or the effective UI tax rate, so the distinction will not be discussed further.) Only the profile simulations in Table 14 can be regarded as ineffective; and all but this exception clearly perform better than all three benchmark strategies. A strategy that ranks firms as GROUP 1 based on the expected amount of UTW (Table 14) yields very little in tax revenue net of auditing costs when the tobit model uses the percent of UTW as the dependent variable. Alternatively, (and somewhat surprisingly) the same tobit model generates very useful results if firms are selected according to the predicted percent of UTW (Table 13). The difference in performance between the two profiles is entirely due to auditing costs. Indeed, the profile in Table 14 detects more unreported taxable wages and additional contributions due than the profile in Table 13.

The profile in Table 15 is directly comparable in all facets to the profile in Table 7. If the policy goal is to detect the largest amounts of additional contributions due, then the tobit using the percent of UTW as the dependent variable (Table 15) accomplishes the goal just as cost effectively as the tobit using the amount of UTW (Table 7). In fact, at audit penetration rates beyond GROUP 2 firms, the former performs considerably better.

The simulation profile in Table 16 based on expected net audit yield does equally well as the profile in Table 15 at detecting noncompliance in a cost effective manner. However, based on an evaluation criteria of realized net audit yield, this profile falls far short of the directly comparable simulations for expected net audit yield using the base model found in Table 8. In other words, a strategy that selects firms for audit based on the expected NAY generates far more actual net tax revenue if the tobit uses the amount of UTW as the dependent variable instead of the UTW%. Furthermore, the clear superiority of the base model for expected NAY continues to hold across all possible simulation profiles examined in this study.

VI. CONCLUSIONS AND IMPLICATIONS

This study started with a premise, established in BBLS, that UI tax evasion among employers is widespread and intentional. This suggests that empirical models can be devised that estimate the impact of various firm characteristics on the amount of tax evasion. Using the same data source as in BBLS, we estimate a large number of such models not previously analyzed in other studies. All of the empirical models are found to conform with theoretical predictions. Once accomplished, the parameters from the regression models are used to simulate various measures of tax evasion for each firm in the sample; and the forecasted degree of noncompliance is compared to the actual level of noncompliance as determined by UI auditors. The efficacy of the profiles is judged according to three criteria; ability of the profiles to detect unreported taxable wages; ability to detect additional contributions due; and ability to identify the amount of the net audit yield (the tax revenue collected net of the expected costs of conducting the audits).

The simulations reported in this paper clearly demonstrate the potential effectiveness of using statistically-based profiles to detect UI tax noncompliance. Almost without exception, the statistical profiles far outperform benchmark strategies of random auditing or "large-firm" auditing on virtually all criteria used for policy evaluation. More specifically, among the extensive number of profiles examined in this report, one profile stood out in terms of detection effectiveness, cost effectiveness and productivity. An auditing strategy using the profile of Table 8 (simulations of expected net audit yield using the base tobit model) seems preferred to any other that we attempted along three dimensions. First, it detects a very large percentage of the total amount of the tax evasion reported in our sample. Second, it does so at very low audit penetration rates (the vast amount of noncompliance is detected among the 58 firms ranked in GROUP 1). Finally, it generates a very large net audit yield after accounting for the auditing costs.

The profiling procedures used in this study seem to hold great promise for more effectively detecting UI tax evasion among employers. The ideal profiling procedure would be self-supporting in the sense of maximizing audit yield for any given level of audit resources, and our profiles seem to accomplish this goal. It should be noted, however, that the effectiveness of any profiling procedure cannot be evaluated solely in these terms. There also is an important, but difficult to quantify, return to any profiling procedure because the level of voluntary compliance among employers is increased when the profile is effective. This occurs when the profile is used to identify high-risk firms for education and assistance or when employers realize that the probability of audit detection for noncomplying firms has risen significantly. In fact, the "best" profiling procedure in this sense is one that encourages full tax reporting and results in complete voluntary compliance.

Over time, employers might learn the basis for identifying noncompliance according to the structure of this (or any other) model, and adapt their behavior accordingly. If so, this will result in more compliance and increased voluntary payment of amounts owed. This unobservable deterrence effect suggests that the model's effectiveness in identifying noncompliers may decline over time. Nonetheless, cost effectiveness remains high if employers who otherwise would underreport taxable wages voluntarily comply. An alternative for firms with a predisposition to noncomply is to change the modus of their actions in ways previously unobserved and, hence, not accounted for by this study. If this occurs among enough firms, a new model would be required to detect noncompliance.

ENDNOTES

1. The data we analyze were obtained from an experimental study conducted for the Illinois Department of Employment Security (IDES). Organizing the study, conducting the audits and gathering the data required substantial resources from IDES, including the assignment of a large number of Illinois field auditors for about one year. We are indebted to IDES and especially Barbara Despenza and Joseph Wojcik for their work on the study.

2. Compliance with the UI system has been analyzed before, but exclusively from the claimants' perspective (e.g., Burgess, 1992; and Kingston, Burgess and St. Louis, 1986).

3. The model is a modification of the model used in BBLS, which focused solely on empirical verification of theoretical predictions. While theoretical consistency continues to be a concern in this project, the main purpose of the model used below is to assess the potential usefulness of statistical profiling as a management tool for the UI system. Hence, policy effectiveness is also of great concern.

4. The exception to the use of the tobit procedure are two models involving the estimate of a probit on UTW. This is done to examine the efficacy of a profile based on the probability of noncompliance, instead of the conditional dollar amount of noncompliance.

5. In some cases, firms may have overreported taxable wages for some workers, resulting in a refund due from IDES. In such cases, gross refunds due were subtracted from gross additional contributions due on underreported taxable wages. The result of this procedure is NET additional contributions due, referred to more simply as ACD in this report. Few firms in the sample had gross refunds due and even fewer had net refunds due. For the analysis in this report, firms with net refunds due were treated as if they had UTW values of zero. In no case was there much money involved in any refund.

6. Feinstein's study [1991] is an exception. He permits imperfect detection in his empirical model -- a model termed fractional detection -- by incorporating the possibility that only a fraction of the full amount of evasion has been caught.

7. Two explanatory variables that would be desirable to incorporate in the empirical model are not included. One is a measure of the penalties from detection. We have no data on penalties subsequently levied on evading firms in our sample, but this omission may be of little consequence because explicit penalties for misreporting are quite small in Illinois, as in most other states. The other variable we cannot include is a direct measure of the probability of detection. However, as a substitute for a direct measure of the probability of detection, we speculate that firm size and punctuality in submitting mandatory quarterly reports may serve as proxies for the firm's estimate of its detection probability.

8. When computing the actual net audit yield, we use an approximation for actual auditing cost for each firm. It is calculated as the number of actual hours it took to conduct an

audit, including an allowance for travel time (data provided by IDES), multiplied by an estimate of the "full" hourly cost of an audit ($25 in Illinois in 1987).

9. The number of groups was arbitrarily chosen.

10. This does not suggest audit penetration should continue until cumulated NAY becomes negative. Audit penetration decisions must be made on the margin, and the incremental NAY becomes negative very quickly in our profiles (eg. beyond GROUP 2 in Table 8).

11. A recent estimate for one state is that the 1099 information can be obtained from the IRS for $6,000. Although the programming to construct both variables is straight forward, we have no good estimate of these costs.

APPENDIX

FORMATION OF EXPECTED AUDIT COST

To calculate the expected cost of auditing each firm in the sample, we first estimate the number of hours it takes to audit firms of varying size. The initial step is to regress the number of hours spent auditing each firm in the sample, as reported by the IDES auditors, on SIZE for all firms in each of the five size categories reported below. From these regressions, the following algorithm was formed which approximates the expected amount of auditing time for firms of different size.

Firm Size (number of employees)		Expected Audit Hours
From	To	
0	9	23
10	49	23 + .3 hours per employee over 10
50	299	33 + .05 hours per employee over 50
300	999	45.5 + .05 hours per employee over 300
1000	over 1000	95.5 + .0018 hours per employee over 1000

The final step in the cost calculation is to add four hours of travel time to the estimated auditing hours for each firm, and multiply the total time predicted to conduct an audit for each firm by a $25 per hour auditor cost (approximation by IDES for 1987).

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TABLE 1
VARIABLE DESCRIPTION

A. Dependent and Simulation Variables

UTW	Net underreported taxable wages due.
ACD	Additional UI contributions due for net underreported taxable wages.
NAY	Net audit yield, defined as ACD minus audit costs.

B. Independent Variables

QUARTERS	The number of quarters the firm has been in business under current ownership (up to a maximum of 16 quarters).
STATTAX	The 1987 UI tax rate for the firm.
EFFTAX	The effective UI tax rate for the firm, discussed in greater detail in the text.
DELINQ	The percent of quarters (over the last four years or since the formation of the business, whichever is shorter) in which the firm has been delinquent in paying its UI tax.
SIZE	Total number of employees reported by the employer to IDES for the third quarter of 1987.
SIZESQ	SIZE squared.
TURNOV	One minus the quantity SIZE divided by the total number of different employees reported by the firm for calendar year 1987 (referred to as ANEMP).
1099%	The percent of employees paid through 1099 accounts, which is calculated as the number of 1099 forms issued by the firm (IRS 1099) divided by the sum of the IRS 1099 plus ANEMP.
IND-CON	Dichotomous variable indicating the construction industry (1 = construction; zero otherwise).
IND-MAN	Omitted dichotomous variable indicating the manufacturing industry.
IND-TRANS	Dichotomous variable indicating the transportation, communication or public utility industries.
IND-WHO	Dichotomous variable indicating the wholesale trade industry.
IND-RET	Dichotomous variable indicating the retail trade industry.
IND-FIN	Dichotomous variable indicating the finance, insurance, and real estate industries.
IND-SER	Dichotomous variable indicating other service industries.
IND-OTH	Dichotomous variable indicating all "other" industries.

Table 2a
Estimated Results of Models of UI Tax Evasion[9]

	Amount of UTW (Tobit)						Percent of UTW (Tobit)		UTW (Probit)	
CONSTANT[b]	-73.40	-63.43	-39.80	-32.38	-32.63	-23.60	-29.81	-26.80	-0.92	-0.73
	(5.26)	(4.73)	(3.01)	(2.52)	(2.45)	(1.84)	(5.83)	(5.46)	(3.70)	(3.06)
QUARTERS	-0.23	-0.27	-1.24	-1.20	-1.23	-1.23	-0.18	-0.18	-0.02	-0.02
	(0.43)	(0.49)	(2.28)	(2.20)	(2.24)	(2.22)	(0.91)	(0.89)	(2.26)	(2.29)
STAT TAX	2.89	---	3.93	---	3.56	---	1.42	---	0.08	---
	(2.63)		(3.53)		(3.16)		(3.55)		(4.15)	
EFFEC TAX	---	1.13	---	3.59	---	2.51	---	1.36	---	0.08
		(0.84)		(2.68)		(1.88)		(2.80)		(2.95)
DELINQ	14.23	14.35	54.48	52.87	51.98	51.30	61.98	61.69	0.67	0.63
	(0.74)	(0.75)	(2.91)	(2.81)	(2.73)	(2.68)	(9.11)	(9.10)	(1.41)	(1.35)
SIZE	73.43	74.67	67.00	70.08	64.75	67.04	5.87	6.94	1.20	1.31
	(3.18)	(3.22)	(2.83)	(2.95)	(2.69)	(2.78)	(0.68)	(0.80)	(2.06)	(2.17)
SIZE SQ	-2.96)	-3.02	-2.68	-2.82	2.58	-2.69	-0.26	-0.30	-0.05	-0.05
	(2.13)	2.16)	(1.90)	(1.97)	(1.81)	(1.86)	(0.51)	(0.60)	(1.66)	(1.78)
TURN OV	80.81	81.84	---	---	---	---	46.31	45.47	1.57	1.52
	(7.81)	(7.77)					(12.36)	(11.96)	(8.63)	(8.36)
1099%	56.09	54.29	60.30	62.92	---	---	18.51	19.47	0.40	0.44
	4.21)	(4.00)	(4.43)	(4.56)			(3.83)	(3.96)	(1.68)	(1.81)
IND-CON	-29.59	-30.22	-19.14	-20.14	-15.97	-16.04	-11.99	-12.56	-0.48	-0.50
	(2.40)	(2.44)	(1.55)	(1.62)	(1.27)	(1.27)	(2.65)	(2.77)	(2.18)	(2.28)
IND-TRANS	13.04	12.56	33.78	29.75	37.18	35.01	17.04	15.57	0.80	0.71
	(0.97)	(0.92)	(2.49)	(2.16)	(2.70)	(2.51)	(3.45)	(3.13)	(2.85)	(2.55)
IND-WHO	-19.37	-20.51	-18.39	-19.17	-13.45	-14.52	-2.46	-2.69	-0.19	-0.20
	(1.53)	(1.62)	(1.45)	(1.51)	(1.05)	(1.13)	(0.53)	(0.58)	(0.83)	(0.89)
IND-RET	-4.64	-7.49	2.82	-1.18	0.53	-2.88	1.39	-0.07	0.09	0.01
	(0.41)	(0.66)	(0.25)	(0.10)	(0.05)	(0.25)	(0.34)	(0.02)	(0.45)	(0.03)'
IND-FIN	2.84	-2.23	-2.16	-7.;28	0.70	-4.56	6.83	4.95	0.28	0.16
	(0.20)	(0.16)	(0.15)	(0.51)	(0.05)	(0.32)	(1.32)	(0.97)	(1.13)	(0.66)
IND-SER	11.46	7.56	14.06	10.12	13.00	9.01	6.27	4.84	0.44	0.35
	(1.07)	(0.71)	(1.29)	(0.94)	(1.18)	(0.82)	(1.59)	(1.24)	(2.28)	(1.86)
IND-OTH	18.10	14.87	17.99	13.54	20.66	16.74	12.31	10.63	0.88	0.80
	(1.14)	(0.94)	(1.11)	(0.84)	(1.26)	(1.02)	(2.11)	(1.83)	(2.98)	(2.70)
SIGMA	61.77	61.90	63.57	63.73	64.73	64.90	22.81	22.84	---	---
	(27.59)	(27.57)	(27.34)	(27.32)	(27.28)	(27.24)	(26.42)	(26.40)		
Likelihood Ratio Test	142.75	136.53	78.35	72.95	59.45	52.90	278.01	373.18	194.14	185.54

[b]Absolute value of t ratios in parentheses

Table 2b
Estimated Results of Models of UI Tax Evasion[9]

	Amount of UTW (Tobit)[a]						Percent of UTW (Tobit)[b]		UTW (Probit)[c]	
QUARTERS	-0.69	-.080	-.370	-.358	-.367	-.367	-.074	-.074	-.008	-.008
STAT TAX	.862	---	1.172	---	1.061	---	.581	---	.032	---
EFFEC TAX	---	.337	---	1.070	---	.748	---	.556	---	.032
DELINQ	4.242	4.278	16.240	15.761	15.495	15.293	25.350	25.231	.265	.249
SIZE	21.889	22.259	19.973	20.891	19.302	19.985	2.401	2.838	.474	.518
SIZE SQ	-.882	-.900	-.799	-.841	-.769	-.802	-.106	-.123	-.020	-.020
TURN OV	24.089	24.397	---	---	---	---	18.941	18.597	.621	.601
1099%	16.720	16.184	17.975	18.756	---	---	7.571	7.963	.158	.174
IND-CON	-8.821	-9.009	-5.706	-6.004	-4.761	-4.782	-4.904	-5.137	-.190	-.198
IND-TRANS	3.887	3.744	10.070	8.868	11.083	10.436	6.969	6.368	.316	.281
IND-WHO	-5.774	-6.114	-5.482	-5.715	-4.009	-4.328	-1.006	-1.100	-.075	-.079
IND-RET	-1.383	-2.233	.841	-.352	.158	-.859	.569	-0.029	.036	.004
IND-FIN	.847	-.665	-.644	-2.170	.209	-1.359	2.793	2.025	.111	.063
IND-SER	3.416	2.254	4.191	3.017	3.875	2.686	2.564	1.980	.174	.138
IND-OTH	5.396	4.433	5.363	4.036	6.159	4.990	5.035	4.348	.348	.316

[a]The partial of expected UTW amounts = $F(Z)\hat{\beta}_i$ (McDonald and Moffitt, 1980), where $Z = \bar{X}\hat{\beta}/\hat{\sigma}$. Partials are evaluated at mean values, and yield Z = -.53 which is utilized as a base for all 6 models.

[b]The partial of expected UTW percent = $F(Z)\hat{\beta}_i$ where $Z = \bar{X}\hat{\beta}/\hat{\sigma}$. Partials are evaluated at mean values, and yield Z = -.23 which is utilized as a base for both models.

[c]The partial of expected probability of UTW = $f(Z)\hat{\beta}_i$ where $Z = \bar{X}\hat{\beta}$. Partials are evaluated at mean values and yield Z = -.135 which is utilized as a base for both models.

TABLE 3

SIMULATION BASED ON RANDOM AUDIT

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 1465366.	6.6%	$ 51712.	6.6%	$ 63827.	$ -12115.
group 2	$ 2930732.	13.3%	$103424.	13.3%	$127653.	$ -24229.
group 3	$ 4396098.	19.9%	$155136.	19.9%	$191480.	$ -36344.
group 4	$ 5861465.	26.5%	$206848.	26.5%	$255306.	$ -48458.
group 5	$ 7326831.	33.1%	$258560.	33.1%	$319133.	$ -60573.
group 6	$ 8792197.	39.8%	$310272.	39.8%	$382959.	$ -72687.
group 7	$10257563.	46.4%	$361984.	46.4%	$446786.	$ -84802.
group 8	$11722929.	53.0%	$413696.	53.0%	$510612.	$ -96917.
group 9	$13188295.	59.7%	$465407.	59.7%	$574439.	$-109031.
group 10	$14653662.	66.3%	$517119.	66.3%	$638265.	$-121146.
group 11	$16119028.	72.9%	$568831.	72.9%	$702092.	$-133260.
group 12	$17584394.	79.5%	$620543.	79.5%	$765918.	$-145375.
group 13	$19049760.	86.2%	$672255.	86.2%	$829745.	$-157489.
group 14	$20515126.	92.8%	$723967.	92.8%	$893571.	$-169604.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

TABLE 4

SIMULATION BASED ON NUMBER OF EMPLOYEES

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 5760876.	26.1%	$178767.	22.9%	$163400.	$ 15367.
group 2	$ 9206365.	41.6%	$267731.	34.3%	$281250.	$ -13519.
group 3	$12129595.	54.9%	$377991.	48.5%	$365100.	$ 12891.
group 4	$13223898.	59.8%	$411730.	52.8%	$437975.	$ -26245.
group 5	$14117108.	63.9%	$456355.	58.5%	$501350.	$ -44995.
group 6	$16420203.	74.3%	$571766.	73.3%	$562650.	$ 9116.
group 7	$17167727.	77.7%	$597169.	76.5%	$613150.	$ -15981.
group 8	$17965818.	81.3%	$638269.	81.8%	$662350.	$ -24081.
group 9	$18561295.	84.0%	$656795.	84.2%	$709800.	$ -53005.
group 10	$19968071.	90.3%	$678009.	86.9%	$754400.	$ -76391.
group 11	$20359184.	92.1%	$695293.	89.1%	$798700.	$-103407.
group 12	$20708177.	93.7%	$710533.	91.1%	$841725.	$-131192.
group 13	$21257753.	96.2%	$750337.	96.2%	$878375.	$-128038.
group 14	$21477308.	97.2%	$758718.	97.3%	$923600.	$-164882.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

TABLE 5

SIMULATION BASED ON UTW FROM PROBIT MODEL

(full model with STATTAX)

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 5786533.	26.2%	$170519.	21.9%	$149075.	$ 21444.
group 2	$ 8298851.	37.5%	$282141.	36.2%	$250225.	$ 31916.
group 3	$13010233.	58.9%	$454283.	58.2%	$323975.	$ 130308.
group 4	$15375258.	69.5%	$520647.	66.7%	$391925.	$ 128722.
group 5	$16485432.	74.6%	$557512.	71.5%	$455075.	$ 102437.
group 6	$18019738.	81.5%	$617450.	79.1%	$514850.	$ 102600.
group 7	$19153260.	86.6%	$679197.	87.1%	$572400.	$ 106797.
group 8	$19443267.	88.0%	$691162.	88.6%	$625500.	$ 65662.
group 9	$19959791.	90.3%	$708353.	90.8%	$677475.	$ 30878.
group 10	$20377108.	92.2%	$718652.	92.1%	$729950.	$ -11298.
group 11	$20868539.	94.4%	$731486.	93.8%	$779325.	$ -47839.
group 12	$21383842.	96.7%	$751261.	96.3%	$824175.	$ -72914.
group 13	$21567679.	97.6%	$757779.	97.1%	$870550.	$-112771.
group 14	$21704387.	98.2%	$760802.	97.5%	$913325.	$-152523.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

(full model with EFFTAX)

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 5756972.	26.0%	$169205.	21.7%	$154425.	$ 14780.
group 2	$ 8330125.	37.7%	$281353.	36.1%	$252375.	$ 28978.
group 3	$13042630.	59.0%	$416646.	53.4%	$323850.	$ 92796.
group 4	$14809126.	67.0%	$492331.	63.1%	$389100.	$ 103231.
group 5	$16899132.	76.4%	$559128.	71.7%	$459300.	$ 99828.
group 6	$17867002.	80.8%	$606379.	77.7%	$510025.	$ 96354.
group 7	$18708695.	84.6%	$637946.	81.8%	$568775.	$ 69171.
group 8	$19202613.	86.9%	$655959.	84.1%	$623075.	$ 32884.
group 9	$19997642.	90.5%	$703425.	90.2%	$674350.	$ 29075.
group 10	$20553354.	93.0%	$715209.	91.7%	$726525.	$ -11316.
group 11	$21142156.	95.6%	$740336.	94.9%	$773700.	$ -33364.
group 12	$21409097.	96.8%	$749786.	96.1%	$826650.	$ -76864.
group 13	$21523991.	97.4%	$753413.	96.6%	$869175.	$-115762.
group 14	$21698403.	98.2%	$760159.	97.4%	$912150.	$-151991.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

TABLE 6

SIMULATION BASED ON EXPECTED UTW FROM TOBIT MODEL OF UTW

(full model with STATTAX)

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 5792037.	26.2%	$168693.	21.6%	$161600.	$ 7093.
group 2	$10331017.	46.7%	$334547.	42.9%	$267950.	$ 66597.
group 3	$14644058.	66.2%	$490576.	62.9%	$342775.	$ 147801.
group 4	$16287371.	73.7%	$547975.	70.2%	$400775.	$ 147200.
group 5	$17940474.	81.2%	$611904.	78.4%	$466850.	$ 145054.
group 6	$18535657.	83.8%	$634362.	81.3%	$521775.	$ 112587.
group 7	$19535370.	88.4%	$687960.	88.2%	$581675.	$ 106285.
group 8	$19772032.	89.4%	$698146.	89.5%	$632050.	$ 66096.
group 9	$20137456.	91.1%	$711301.	91.2%	$685750.	$ 25551.
group 10	$20721769.	93.7%	$726627.	93.1%	$733575.	$ -6948.
group 11	$21329280.	96.5%	$749188.	96.0%	$785625.	$ -36437.
group 12	$21495414.	97.2%	$755183.	96.8%	$830300.	$ -75117.
group 13	$21617523.	97.8%	$757927.	97.2%	$872925.	$-114998.
group 14	$21704724.	98.2%	$760878.	97.5%	$914800.	$-153922.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

(full model with EFFTAX)

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 5856388.	26.5%	$170610.	21.9%	$160600.	$ 10010.
group 2	$ 9762910.	44.2%	$296596.	38.0%	$262100.	$ 34496.
group 3	$15244904.	69.0%	$497564.	63.8%	$342400.	$ 155164.
group 4	$16247556.	73.5%	$529021.	67.8%	$409025.	$ 119996.
group 5	$17963602.	81.3%	$604175.	77.4%	$468300.	$ 135875.
group 6	$18629228.	84.3%	$626501.	80.3%	$519375.	$ 107126.
group 7	$18945392.	85.7%	$638729.	81.9%	$577000.	$ 61729.
group 8	$19764198.	89.4%	$692586.	88.8%	$632125.	$ 60461.
group 9	$20260011.	91.6%	$707742.	90.7%	$679575.	$ 28167.
group 10	$20739906.	93.8%	$721155.	92.4%	$736575.	$ -15420.
group 11	$21236214.	96.1%	$740398.	94.9%	$783600.	$ -43202.
group 12	$21504176.	97.3%	$753219.	96.5%	$829900.	$ -76681.
group 13	$21611697.	97.8%	$757213.	97.1%	$872200.	$-114987.
group 14	$21689084.	98.1%	$759258.	97.3%	$914300.	$-155042.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-1827

TABLE 7

SIMULATION BASED ON EXPECTED ACD FROM TOBIT MODEL WITH UTW

(full model with STATTAX)

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 6835178.	30.9%	$247549.	31.7%	$159075.	$ 88474.
group 2	$ 9204845.	41.6%	$392092.	50.3%	$232075.	$ 160017.
group 3	$10778185.	48.8%	$464078.	59.5%	$313075.	$ 151003.
group 4	$12551922.	56.8%	$546895.	70.1%	$379700.	$ 167195.
group 5	$13698627.	62.0%	$591552.	75.8%	$440050.	$ 151502.
group 6	$14272028.	64.6%	$614843.	78.8%	$496425.	$ 118418.
group 7	$16190828.	73.2%	$664170.	85.1%	$555950.	$ 108220.
group 8	$18008090.	81.5%	$699334.	89.6%	$616050.	$ 83284.
group 9	$19842769.	89.8%	$730142.	93.6%	$674025.	$ 56117.
group 10	$20290556.	91.8%	$741089.	95.0%	$728675.	$ 12414.
group 11	$20804039.	94.1%	$749991.	96.1%	$780150.	$ -30159.
group 12	$21307627.	96.4%	$756718.	97.0%	$830225.	$ -73507.
group 13	$21528232.	97.4%	$759996.	97.4%	$875275.	$-115279.
group 14	$21849363.	98.8%	$764492.	98.0%	$917725.	$-153233.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

(full model with EFFTAX)

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 6752191.	30.5%	$239338.	30.7%	$159725.	$ 79613.
group 2	$ 9160583.	41.4%	$379909.	48.7%	$237325.	$ 142584.
group 3	$10877219.	49.2%	$464263.	59.5%	$315700.	$ 148563.
group 4	$12692607.	57.4%	$551710.	70.7%	$381175.	$ 170535.
group 5	$13922299.	63.0%	$597368.	76.6%	$444450.	$ 152918.
group 6	$15585121.	70.5%	$639647.	82.0%	$505300.	$ 134347.
group 7	$17256212.	78.1%	$677881.	86.9%	$565050.	$ 112831.
group 8	$18259034.	82.6%	$702645.	90.1%	$622725.	$ 79920.
group 9	$19926295.	90.1%	$731590.	93.8%	$674350.	$ 57240.
group 10	$20498022.	92.7%	$743807.	95.3%	$732675.	$ 11132.
group 11	$20816062.	94.2%	$750161.	96.2%	$781950.	$ -31789.
group 12	$21353090.	96.6%	$757263.	97.1%	$830775.	$ -73512.
group 13	$21527332.	97.4%	$759985.	97.4%	$874925.	$-114940.
group 14	$21831111.	98.8%	$764369.	98.0%	$920650.	$-156281.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

TABLE 8

SIMULATION BASED ON EXPECTED NAY FROM TOBIT MODEL WITH UTW

(full model with STATTAX)

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 7356427.	33.3%	$322448.	41.3%	$126150.	$ 196298.
group 2	$ 9262360.	41.9%	$401645.	51.5%	$197475.	$ 204170.
group 3	$10321963.	46.7%	$456476.	58.5%	$272050.	$ 184426.
group 4	$10675166.	48.3%	$474490.	60.8%	$320600.	$ 153890.
group 5	$11325081.	51.2%	$506214.	64.9%	$384275.	$ 121939.
group 6	$12856388.	58.2%	$581338.	74.5%	$449700.	$ 131638.
group 7	$14263535.	64.5%	$599779.	76.9%	$503300.	$ 96479.
group 8	$15312992.	69.3%	$627883.	80.5%	$550800.	$ 77083.
group 9	$15875768.	71.8%	$642084.	82.3%	$603200.	$ 38884.
group 10	$16489956.	74.6%	$659021.	84.5%	$649125.	$ 9896.
group 11	$16763720.	75.8%	$667338.	85.5%	$689575.	$ -22237.
group 12	$17039268.	77.1%	$673976.	86.4%	$738900.	$ -64924.
group 13	$17409124.	78.8%	$680716.	87.3%	$787400.	$-106684.
group 14	$18993407.	85.9%	$705199.	90.4%	$857125.	$-151926.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

(full model with EFFTAX)

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 7284086.	32.9%	$304270.	39.0%	$128550.	$ 175720.
group 2	$ 9311406.	42.1%	$401269.	51.4%	$199850.	$ 201419.
group 3	$10273454.	46.5%	$449736.	57.6%	$275600.	$ 174136.
group 4	$10675046.	48.3%	$471906.	60.5%	$324100.	$ 147806.
group 5	$11484042.	51.9%	$503902.	64.6%	$378750.	$ 125152.
group 6	$12389107.	56.0%	$540533.	69.3%	$447950.	$ 92583.
group 7	$14381082.	65.1%	$603631.	77.4%	$500450.	$ 103181.
group 8	$15231362.	68.9%	$622120.	79.7%	$549425.	$ 72695.
group 9	$15972021.	72.2%	$642148.	82.3%	$602875.	$ 39273.
group 10	$16219735.	73.4%	$649470.	83.3%	$647100.	$ 2370.
group 11	$16829360.	76.1%	$668713.	85.7%	$690025.	$ -21312.
group 12	$17048752.	77.1%	$673359.	86.3%	$737650.	$ -64291.
group 13	$17440802.	78.9%	$681140.	87.3%	$792475.	$-111335.
group 14	$19047989.	86.2%	$705309.	90.4%	$857675.	$-152366.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

TABLE 9

SIMULATION BASED ON EXPECTED UTW FROM TOBIT MODEL WITH UTW

(restricted model with STATTAX excluding TURNOV and 1099%)

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 5848719.	26.5%	$171276.	22.0%	$158825.	$ 12451.
group 2	$ 8439427.	38.2%	$283476.	36.3%	$272625.	$ 10851.
group 3	$11002386.	49.8%	$364882.	46.8%	$345050.	$ 19832.
group 4	$12488677.	56.5%	$431798.	55.3%	$407550.	$ 24248.
group 5	$13913834.	62.9%	$530231.	68.0%	$463075.	$ 67156.
group 6	$16154820.	73.1%	$597672.	76.6%	$526500.	$ 71172.
group 7	$17293191.	78.2%	$649545.	83.3%	$579425.	$ 70120.
group 8	$17677901.	80.0%	$664527.	85.2%	$630825.	$ 33702.
group 9	$18867882.	85.3%	$693422.	88.9%	$684825.	$ 8597.
group 10	$19128328.	86.5%	$701890.	90.0%	$732250.	$ -30360.
group 11	$20699444.	93.6%	$726725.	93.2%	$780975.	$ -54250.
group 12	$21125833.	95.6%	$745656.	95.6%	$824475.	$ -78819.
group 13	$21348825.	96.6%	$755765.	96.9%	$865550.	$-109785.
group 14	$21706960.	98.2%	$761187.	97.6%	$909275.	$-148088.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

(restricted model with EFFTAX excluding TURNOV and 1099%)

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 5851449.	26.5%	$171336.	22.0%	$158550.	$ 12786.
group 2	$ 8610842.	39.0%	$242771.	31.1%	$269350.	$ -26579.
group 3	$10935927.	49.5%	$351126.	45.0%	$348100.	$ 3026.
group 4	$11988806.	54.2%	$393549.	50.4%	$409850.	$ -16301.
group 5	$14414221.	65.2%	$497860.	63.8%	$465200.	$ 32660.
group 6	$16088734.	72.8%	$576927.	74.0%	$520850.	$ 56077.
group 7	$17147241.	77.6%	$609090.	78.1%	$575000.	$ 34090.
group 8	$18381995.	83.2%	$669084.	85.8%	$629775.	$ 39309.
group 9	$18739318.	84.8%	$680896.	87.3%	$681250.	$ -354.
group 10	$20032959.	90.6%	$697043.	89.3%	$727100.	$ -30057.
group 11	$20654994.	93.4%	$722006.	92.5%	$774600.	$ -52594.
group 12	$20918929.	94.6%	$731184.	93.7%	$820000.	$ -88816.
group 13	$21423235.	96.9%	$750462.	96.2%	$865275.	$-114813.
group 14	$21719601.	98.2%	$758786.	97.3%	$907175.	$-148389.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

TABLE 10

SIMULATION BASED ON EXPECTED ACD FROM TOBIT MODEL WITH UTW

(restricted model with STATTAX excluding TURNOV and 1099%)

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 6477235.	29.3%	$235536.	30.2%	$162475.	$ 73061.
group 2	$ 7827537.	35.4%	$299315.	38.4%	$242550.	$ 56765.
group 3	$ 9881670.	44.7%	$431152.	55.3%	$306975.	$ 124177.
group 4	$11086620.	50.2%	$498506.	63.9%	$363125.	$ 135381.
group 5	$12125903.	54.9%	$534182.	68.5%	$426600.	$ 107582.
group 6	$12777472.	57.8%	$561313.	72.0%	$474575.	$ 86738.
group 7	$14399784.	65.1%	$606589.	77.8%	$525600.	$ 80989.
group 8	$15528105.	70.2%	$650656.	83.4%	$596950.	$ 53706.
group 9	$16858568.	76.3%	$686873.	88.0%	$662450.	$ 24423.
group 10	$17732965.	80.2%	$707238.	90.7%	$720450.	$ -13212.
group 11	$19519986.	88.3%	$738758.	94.7%	$775975.	$ -37217.
group 12	$20263248.	91.7%	$750784.	96.2%	$830975.	$ -80191.
group 13	$20421941.	92.4%	$752807.	96.5%	$875425.	$-122618.
group 14	$21654863.	98.0%	$762943.	97.8%	$911275.	$-148332.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

(restricted model with EFFTAX excluding TURNOV and 1099%)

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 5614392.	25.4%	$175861.	22.5%	$162300.	$ 13561.
group 2	$ 8065293.	36.5%	$319791.	41.0%	$247000.	$ 72791.
group 3	$10145753.	45.9%	$436502.	56.0%	$311775.	$ 124727.
group 4	$11295070.	51.1%	$487294.	62.5%	$370725.	$ 116569.
group 5	$12161392.	55.0%	$533433.	68.4%	$428225.	$ 105208.
group 6	$13029512.	58.9%	$566908.	72.7%	$475400.	$ 91508.
group 7	$14703336.	66.5%	$614002.	78.7%	$537025.	$ 76977.
group 8	$15518415.	70.2%	$647865.	83.0%	$595075.	$ 52790.
group 9	$16837323.	76.2%	$682772.	87.5%	$660775.	$ 21997.
group 10	$17840839.	80.7%	$709378.	90.9%	$720150.	$ -10772.
group 11	$19497139.	88.2%	$738079.	94.6%	$776825.	$ -38746.
group 12	$20265600.	91.7%	$750825.	96.2%	$830725.	$ -79900.
group 13	$20446679.	92.5%	$753078.	96.5%	$875500.	$-122422.
group 14	$21655578.	98.0%	$762954.	97.8%	$910575.	$-147621.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

TABLE 11

SIMULATION BASED ON EXPECTED NAY FROM TOBIT MODEL WITH UTW

(restricted model with STATTAX excluding TURNOV and 1099%)

	UTW amount	pct	ACD amount	pct	actual cost amount	actual NAY amount
group 1	$ 6176935.	27.9%	$228584.	29.3%	$130475.	$ 98109.
group 2	$ 7795229.	35.3%	$328994.	42.2%	$199350.	$ 129644.
group 3	$ 8784247.	39.7%	$391667.	50.2%	$253800.	$ 137867.
group 4	$ 9467902.	42.8%	$430333.	55.2%	$303225.	$ 127108.
group 5	$10449580.	47.3%	$487923.	62.5%	$357250.	$ 130673.
group 6	$11251830.	50.9%	$525839.	67.4%	$418475.	$ 107364.
group 7	$11907668.	53.9%	$552491.	70.8%	$475025.	$ 77466.
group 8	$12655991.	57.2%	$581494.	74.5%	$528300.	$ 53194.
group 9	$13322537.	60.3%	$600435.	77.0%	$588900.	$ 11535.
group 10	$13595698.	61.5%	$606507.	77.7%	$634975.	$ -28468.
group 11	$13889630.	62.8%	$612884.	78.6%	$675875.	$ -62991.
group 12	$15472061.	70.0%	$636071.	81.5%	$725575.	$ -89504.
group 13	$16961001.	76.7%	$664157.	85.1%	$783250.	$-119093.
group 14	$18348548.	83.0%	$702506.	90.0%	$852375.	$-149869.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

(restricted model with EFFTAX excluding TURNOV and 1099%)

	UTW amount	pct	ACD amount	pct	actual cost amount	actual NAY amount
group 1	$ 6550925.	29.6%	$260271.	33.4%	$125100.	$ 135171.
group 2	$ 7443847.	33.7%	$302297.	38.7%	$200125.	$ 102172.
group 3	$ 8577135.	38.8%	$371251.	47.6%	$252075.	$ 119176.
group 4	$ 9555567.	43.2%	$430330.	55.2%	$301625.	$ 128705.
group 5	$10220179.	46.2%	$457371.	58.6%	$363800.	$ 93571.
group 6	$11158783.	50.5%	$517760.	66.4%	$418125.	$ 99635.
group 7	$11658402.	52.7%	$541230.	69.4%	$475600.	$ 65630.
group 8	$12529721.	56.7%	$571946.	73.3%	$530950.	$ 40996.
group 9	$13329602.	60.3%	$599940.	76.9%	$589800.	$ 10140.
group 10	$13561794.	61.3%	$605696.	77.6%	$633525.	$ -27829.
group 11	$13911563.	62.9%	$613020.	78.6%	$675625.	$ -62605.
group 12	$15511252.	70.2%	$637137.	81.7%	$726950.	$ -89813.
group 13	$17101387.	77.4%	$668135.	85.6%	$787375.	$-119240.
group 14	$18044277.	81.6%	$692908.	88.8%	$857175.	$-164267.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

TABLE 12

SIMULATION BASED ON EXPECTED NAY FROM TOBIT MODEL WITH UTW

(restricted model with STATTAX excluding TURNOV)

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 6520260.	29.5%	$246461.	31.6%	$137300.	$ 109161.
group 2	$ 8732972.	39.5%	$381954.	49.0%	$196775.	$ 185179.
group 3	$ 9959511.	45.1%	$445525.	57.1%	$269200.	$ 176325.
group 4	$10617331.	48.0%	$476817.	61.1%	$324700.	$ 152117.
group 5	$10930135.	49.4%	$489888.	62.8%	$375025.	$ 114863.
group 6	$12267900.	55.5%	$558427.	71.6%	$430800.	$ 127627.
group 7	$12552288.	56.8%	$570629.	73.1%	$476700.	$ 93929.
group 8	$13198473.	59.7%	$593896.	76.1%	$534125.	$ 59771.
group 9	$13634958.	61.7%	$607603.	77.9%	$594775.	$ 12828.
group 10	$13745049.	62.2%	$609575.	78.1%	$640575.	$ -31000.
group 11	$15523013.	70.2%	$630826.	80.9%	$683675.	$ -52849.
group 12	$15894347.	71.9%	$642303.	82.3%	$730925.	$ -88622.
group 13	$17300051.	78.3%	$672672.	86.2%	$795200.	$-122528.
group 14	$18595918.	84.1%	$709383.	90.9%	$860200.	$-150817.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

(restricted model with EFFTAX excluding TURNOV)

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 6482508.	29.3%	$242925.	31.1%	$134550.	$ 108375.
group 2	$ 8799804.	39.8%	$380455.	48.8%	$203875.	$ 176580.
group 3	$ 9938288.	45.0%	$440592.	56.5%	$268750.	$ 171842.
group 4	$10662498.	48.2%	$474286.	60.8%	$320825.	$ 153461.
group 5	$10941070.	49.5%	$487480.	62.5%	$370475.	$ 117005.
group 6	$11812842.	53.4%	$521381.	66.8%	$428625.	$ 92756.
group 7	$12305404.	55.7%	$539099.	69.1%	$486025.	$ 53074.
group 8	$12744214.	57.6%	$556032.	71.3%	$539650.	$ 16382.
group 9	$13632569.	61.7%	$605247.	77.6%	$596775.	$ 8472.
group 10	$15367704.	69.5%	$628516.	80.6%	$642350.	$ -13834.
group 11	$15555213.	70.4%	$631334.	80.9%	$683225.	$ -51891.
group 12	$16178231.	73.2%	$647927.	83.1%	$735850.	$ -87923.
group 13	$17048902.	77.1%	$667988.	85.6%	$792350.	$-124362.
group 14	$18602235.	84.1%	$709456.	90.9%	$863150.	$-153694.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

TABLE 13

SIMULATION BASED ON EXPECTED UTW% FROM TOBIT MODEL WITH UTW%

(full model with STATTAX)

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 3664048.	16.6%	$148882.	19.1%	$ 66100.	$ 82782.
group 2	$ 8743778.	39.6%	$339164.	43.5%	$167725.	$ 171439.
group 3	$11020314.	49.9%	$396378.	50.8%	$238025.	$ 158353.
group 4	$13446058.	60.8%	$483531.	62.0%	$319150.	$ 164381.
group 5	$14595516.	66.0%	$524357.	67.2%	$385625.	$ 138732.
group 6	$15165791.	68.6%	$544648.	69.8%	$448000.	$ 96648.
group 7	$17557611.	79.4%	$608543.	78.0%	$520600.	$ 87943.
group 8	$18137050.	82.0%	$628568.	80.6%	$587550.	$ 41018.
group 9	$18655370.	84.4%	$645145.	82.7%	$645600.	$ -455.
group 10	$18948580.	85.7%	$657249.	84.2%	$695100.	$ -37851.
group 11	$20215071.	91.4%	$716499.	91.8%	$760100.	$ -43601.
group 12	$20728631.	93.8%	$734435.	94.1%	$815275.	$ -80840.
group 13	$21435608.	97.0%	$755558.	96.8%	$866625.	$-111067.
group 14	$21655085.	98.0%	$760170.	97.4%	$913525.	$-153355.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

(full model with EFFTAX)

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 4800227.	21.7%	$172116.	22.1%	$ 79025.	$ 93091.
group 2	$ 8880238.	40.2%	$337333.	43.2%	$173550.	$ 163783.
group 3	$11158974.	50.5%	$386190.	49.5%	$240150.	$ 146040.
group 4	$13149785.	59.5%	$465414.	59.7%	$325450.	$ 139964.
group 5	$14897785.	67.4%	$525452.	67.4%	$402750.	$ 122702.
group 6	$16095152.	72.8%	$561445.	72.0%	$463275.	$ 98170.
group 7	$17555687.	79.4%	$598213.	76.7%	$521375.	$ 76838.
group 8	$18100245.	81.9%	$621447.	79.7%	$583500.	$ 37947.
group 9	$18666643.	84.4%	$644474.	82.6%	$642550.	$ 1924.
group 10	$19238068.	87.0%	$661343.	84.8%	$700450.	$ -39107.
group 11	$19840680.	89.7%	$678884.	87.0%	$750700.	$ -71816.
group 12	$20716552.	93.7%	$725525.	93.0%	$811400.	$ -85875.
group 13	$21516091.	97.3%	$756127.	96.9%	$864900.	$-108773.
group 14	$21697423.	98.1%	$759666.	97.4%	$910075.	$-150409.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

TABLE 14

SIMULATION BASED ON EXPECTED UTW FROM TOBIT MODEL WITH UTW%

(full model with STATTAX)

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 5786340.	26.2%	$167029.	21.4%	$166625.	$ 404.
group 2	$13083750.	59.2%	$405060.	51.9%	$298275.	$ 106785.
group 3	$15064433.	68.1%	$511451.	65.6%	$374250.	$ 137201.
group 4	$17504902.	79.2%	$612958.	78.6%	$441525.	$ 171433.
group 5	$18509603.	83.7%	$651556.	83.5%	$505050.	$ 146506.
group 6	$19265659.	87.1%	$676879.	86.8%	$566150.	$ 110729.
group 7	$19981547.	90.4%	$702343.	90.0%	$616475.	$ 85868.
group 8	$20772076.	94.0%	$733021.	94.0%	$671925.	$ 61096.
group 9	$21215011.	96.0%	$748091.	95.9%	$722525.	$ 25566.
group 10	$21493648.	97.2%	$758708.	97.3%	$768000.	$ -9292.
group 11	$21694659.	98.1%	$767317.	98.4%	$813800.	$ -46483.
group 12	$21886813.	99.0%	$774413.	99.3%	$856100.	$ -81687.
group 13	$22024077.	99.6%	$778126.	99.7%	$892900.	$-114774.
group 14	$22072832.	99.8%	$779132.	99.9%	$930475.	$-151343.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

(full model with EFFTAX)

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 5786340.	26.2%	$167029.	21.4%	$167325.	$ -296.
group 2	$12432908.	56.2%	$360113.	46.2%	$293175.	$ 66938.
group 3	$14638753.	66.2%	$476656.	61.1%	$373700.	$ 102956.
group 4	$17426648.	78.8%	$607292.	77.8%	$440925.	$ 166367.
group 5	$18384934.	83.2%	$647043.	82.9%	$502475.	$ 144568.
group 6	$19338468.	87.5%	$679568.	87.1%	$563925.	$ 115643.
group 7	$20204401.	91.4%	$706625.	90.6%	$615125.	$ 91500.
group 8	$20848849.	94.3%	$732378.	93.9%	$672925.	$ 59453.
group 9	$21208533.	95.9%	$746596.	95.7%	$723100.	$ 23496.
group 10	$21501901.	97.3%	$758790.	97.3%	$768100.	$ -9310.
group 11	$21685138.	98.1%	$764869.	98.0%	$814550.	$ -49681.
group 12	$21902234.	99.1%	$774731.	99.3%	$855900.	$ -81169.
group 13	$22026792.	99.6%	$778153.	99.7%	$891950.	$-113797.
group 14	$22069922.	99.8%	$779002.	99.9%	$929975.	$-150973.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

TABLE 15

SIMULATION BASED ON EXPECTED ACD FROM TOBIT MODEL WITH UTW%

(full model with STATTAX)

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 6921258.	31.3%	$248491.	31.9%	$166100.	$ 82391.
group 2	$10440563.	47.2%	$432981.	55.5%	$286975.	$ 146006.
group 3	$14462244.	65.4%	$565963.	72.5%	$368025.	$ 197938.
group 4	$15753048.	71.3%	$613900.	78.7%	$436400.	$ 177500.
group 5	$17790688.	80.5%	$651387.	83.5%	$502750.	$ 148637.
group 6	$19215538.	86.9%	$692815.	88.8%	$560150.	$ 132665.
group 7	$20040754.	90.7%	$723327.	92.7%	$617425.	$ 105902.
group 8	$20615252.	93.3%	$740689.	94.9%	$667900.	$ 72789.
group 9	$20961874.	94.8%	$753346.	96.6%	$718875.	$ 34471.
group 10	$21374211.	96.7%	$765073.	98.1%	$764600.	$ 473.
group 11	$21708434.	98.2%	$773210.	99.1%	$809000.	$ -35790.
group 12	$21854789.	98.9%	$776438.	99.5%	$850900.	$ -74462.
group 13	$21990038.	99.5%	$778831.	99.8%	$894225.	$-115394.
group 14	$22060378.	99.8%	$779566.	99.9%	$928925.	$-149359.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

(full model with EFFTAX)

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 6750704.	30.5%	$234731.	30.1%	$161975.	$ 72756.
group 2	$10541686.	47.7%	$433902.	55.6%	$291850.	$ 142052.
group 3	$14455975.	65.4%	$562980.	72.2%	$370375.	$ 192605.
group 4	$15876929.	71.8%	$614661.	78.8%	$437000.	$ 177661.
group 5	$18304055.	82.8%	$658125.	84.4%	$505250.	$ 152875.
group 6	$19291122.	87.3%	$692958.	88.8%	$563025.	$ 129933.
group 7	$20031066.	90.6%	$721129.	92.4%	$617225.	$ 103904.
group 8	$20619605.	93.3%	$740678.	94.9%	$666900.	$ 73778.
group 9	$20967930.	94.8%	$751773.	96.4%	$719750.	$ 32023.
group 10	$21405261.	96.8%	$764930.	98.1%	$765125.	$ -195.
group 11	$21691761.	98.1%	$772019.	99.0%	$809100.	$ -37081.
group 12	$21858295.	98.9%	$776470.	99.5%	$852750.	$ -76280.
group 13	$21990038.	99.5%	$778831.	99.8%	$894150.	$-115319.
group 14	$22060378.	99.8%	$779566.	99.9%	$929650.	$-150084.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

TABLE 16

SIMULATION BASED ON EXPECTED NAY FROM TOBIT MODEL WITH UTW%

(full model with STATTAX)

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 6921258.	31.3%	$248491.	31.9%	$166100.	$ 82391.
group 2	$10326639.	46.7%	$430596.	55.2%	$285500.	$ 145096.
group 3	$14471085.	65.5%	$567420.	72.7%	$368500.	$ 198920.
group 4	$15666731.	70.9%	$612676.	78.5%	$434775.	$ 177901.
group 5	$17662034.	79.9%	$653352.	83.7%	$503050.	$ 150302.
group 6	$19289579.	87.3%	$697942.	89.5%	$556950.	$ 140992.
group 7	$19987614.	90.4%	$723791.	92.8%	$615425.	$ 108366.
group 8	$20571395.	93.1%	$744457.	95.4%	$666150.	$ 78307.
group 9	$20808226.	94.1%	$753077.	96.5%	$711700.	$ 41377.
group 10	$21205247.	95.9%	$765973.	98.2%	$756450.	$ 9523.
group 11	$21437854.	97.0%	$770400.	98.8%	$800125.	$ -29725.
group 12	$21668896.	98.0%	$773468.	99.1%	$846225.	$ -72757.
group 13	$21765845.	98.5%	$774497.	99.3%	$883700.	$-109203.
group 14	$21810584.	98.7%	$775372.	99.4%	$915000.	$-139628.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

(full model with EFFTAX)

	UTW amount	UTW pct	ACD amount	ACD pct	actual cost amount	actual NAY amount
group 1	$ 6750704.	30.5%	$234731.	30.1%	$161975.	$ 72756.
group 2	$10440563.	47.2%	$432981.	55.5%	$287600.	$ 145381.
group 3	$14534024.	65.7%	$568225.	72.8%	$369075.	$ 199150.
group 4	$16853493.	76.2%	$622100.	79.7%	$436300.	$ 185800.
group 5	$18414870.	83.3%	$666832.	85.5%	$503550.	$ 163282.
group 6	$19358098.	87.6%	$695496.	89.2%	$559025.	$ 136471.
group 7	$20014672.	90.5%	$724743.	92.9%	$615325.	$ 109418.
group 8	$20583508.	93.1%	$742845.	95.2%	$665850.	$ 76995.
group 9	$20917687.	94.6%	$755792.	96.9%	$712850.	$ 42942.
group 10	$21215650.	96.0%	$765460.	98.1%	$758800.	$ 6660.
group 11	$21457924.	97.1%	$770100.	98.7%	$802375.	$ -32275.
group 12	$21667521.	98.0%	$773456.	99.1%	$845675.	$ -72219.
group 13	$21766252.	98.5%	$774484.	99.3%	$884325.	$-109841.
group 14	$21805443.	98.6%	$775173.	99.4%	$915775.	$-140602.
group 15	$22106817.	100.0%	$780137.	100.0%	$962900.	$-182763.

Advisory Council on Unemployment Compensation

Short-Time Compensation: A Literature Review

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Short-Time Compensation - A Summary

Short-Time Compensation (STC) programs allow employers to reduce the hours of their workers for some period of time and for the workers to receive proportionate unemployment benefits for those reduced hours. The purpose of STC is to avoid layoffs during recessions, maintain fringe benefit coverage, reduce the physical and psychological costs of unemployment to individuals who would otherwise be laid off, and spread the cost of unemployment across all workers in the unit. STC has been used in other countries and, more recently, in 17 States.

The available research indicates that STC can reduce layoffs during a recession. Further, these programs have a social cost/benefit ratio in excess of one. The fact that the program is used less in this country than in other countries relates to a lack of information among employers with regard to the program and the use of surcharges and experience rating. The continuation of payroll taxes, which are paid only on covered wages, or fringe benefits, many of which are fixed costs, are probably not large factors.

One concern often raised about STC programs is that they may be used inappropriately by firms undergoing structural adjustment. Relatively little data on the types of firms that use STC programs are available. What exists suggests that use of STC is concentrated among manufacturing and trade firms, industries undergoing structural change. In addition, studies of the Canadian system indicate that 5 percent or less of STC participants engage in training or retraining. This may result from an expectation that participation in STC implies that the individual will return to full-time employment with the firm. Under this expectation, the individual does not have an incentive to engage in retraining.

The Unemployment Insurance (UI) Service is currently developing a screening procedure to separate short-term layoffs, longer term recessionary layoffs, and permanently dislocated workers. One possibility is to provide training or retraining for dislocated workers as part of an STC program. While STC programs appear to increase UI costs, combining screening of UI beneficiaries, as described above, with training programs for dislocated workers may reduce total costs. Alternately, shorter periods of benefits for short-term recessionary unemployment and longer periods of benefits, combined with training or retraining for permanently dislocated workers, may improve labor-market efficiency and reduce program costs.

Background

Short-time compensation (STC) programs, also referred to as work sharing, are programs designed to moderate the effects of the cyclical fluctuations in the economy. As they are implemented currently in Europe, Canada, and the 17 States in the U.S. with STC provisions, these programs seek to achieve stability in the workforce by providing firms with an alternative to laying off workers during times of decreased demand. Under an STC program, it is possible for a business to curtail production by reducing the number of hours in the work week for every worker in a production unit while continuing to employ the same number of workers. The cost of reduced demand is then borne by all of the workers of the production unit, not just those laid off. Thus, although each worker would be faced with lower weekly earnings, job security would be much greater.

Most STC programs function in a similar fashion. A business desiring to reduce production reaches an agreement with its workers as to the specifics of the work reduction plan.[1] This plan is then submitted to the relevant unemployment agency for approval. Once a plan has been approved, employees work only the specified number of hours each week (generally expressed as a percentage of "normal" hours) and receive wages only for the hours which they work. Under the STC program, the workers receive a percentage of their weekly unemployment benefits from the State. The percentage of benefits they receive is equivalent to the percentage of "normal" hours that they do not work. A common example is the firm that decides that a 20 percent reduction in the workforce is necessary. Under a typical STC program, workers get paid for only 80 percent of their normal hours (4 days a week) and receive unemployment benefits from the State government equal to 20 percent (1 day) of the weekly unemployment benefit for which they would qualify if they were laid off.

STC programs have been used since before World War II in various European countries, the most notable being Germany. In the United States, however, with the exception of a brief trial during the Great Depression, the use of STC programs is a recent phenomenon. Although the use of STC had been considered during periods of high unemployment in the middle part of the century[2] no program had been adopted by any State until California passed shared work legislation in 1978.[3] Since then, 16 States have amended their Unemployment Insurance (UI) laws to include a provision for STC programs.

Existing STC Programs

STC Programs in the United States

The 17 States that currently operate STC programs are listed in Table 1. They include large States, such as California and Texas, and smaller States, like Rhode Island and Connecticut. The most recent State to start an STC program was Connecticut, which began its program in 1992.

Table 1
Short-Time Compensation Programs in 1993

State	Initial Year of Program	Maximum Duration of Plans (weeks)	Range of Reduction in Hours (percent)	Full Maintenance of Fringe Benefits	Special STC Tax, Max Pct Rate
Arizona	1982	52	10 to 40	Optional	Yes-2.0
Arkansas	1985	52	10 to 40	Required	No
California	1978	26	10 or more	Optional	No
Connecticut	1992	26[a]	20 to 40	Required	No
Florida	1984	52	10 to 40	Optional	Yes-1.0
Iowa	1991	104	20 to 50	Optional	No
Kansas	1988	52	20 to 40	Optional	No
Louisiana	1986	52	20 to 40	Required	No
Maryland	1984	26	10 to 50	Optional	No
Massachusetts	1986	26	10 to 60	Required[b]	No
Missouri	1987	52	20 to 40	Optional	Yes-1.17
New York	1986	20	20 to 60	Required	No
Oregon	1982	52	20 to 40	Optional	Yes-3.0
Rhode Island	1991	52	10 to 50	Optional	No
Texas	1986	52	10 to 40	Optional	No
Vermont	1986	26	20 to 50	Optional	No
Washington	1983	52	10 to 50	Required[b]	No

a - May be extended.

b - Only health insurance benefits must be fully maintained.

Source: Vroman (1992b, Table 1)

In order to implement an STC program, it is necessary for a State to alter some of the provisions in its UI statutes. Model legislative language was made available to States considering STC programs in 1983. Thus, many of the State laws are similar.[4] Most of the STC provisions concern the eligibility of recipients to receive unemployment benefits. Other alterations to the UI statutes include worker entitlements to regular UI benefits (in the event that a worker placed on an STC program subsequently gets laid off) and financing provisions.[5] STC laws also mandate the permitted range in reduction of hours, the minimum number of employees to be affected, the duration of the plan, and the amount of short-time benefits employees are eligible to receive (usually expressed as the number of weeks an employee can participate in the STC program). The laws also contain the procedures for approving programs and modifying existing STC programs.

Table 1 exhibits some of the characteristics of State STC programs. Most programs have a maximum duration of 52 weeks, though five States limit the maximum duration to 26 weeks. In order for a plan to be approved in any State, an employer must reduce average work hours by at least 10 percent, and all but three States require that hours be reduced by no more than 50 percent.[6] Only Arkansas, Louisiana, and New York require that full fringe benefits be maintained. Massachusetts and Washington require that only health insurance benefits be fully maintained. Finally, only four States currently charge an additional tax (surcharge) to firms that participate in an STC program.[7]

Besides the differences listed in Table 1, other differences exist among the 17 State STC programs. Almost all of the States require that any STC program submitted for approval certify that a temporary reduction of the workforce is necessary and that the program is being implemented in lieu of layoffs. Further, many of the States require the submitted programs to certify that the program will not be used to subsidize seasonal unemployment. Finally, the length of time needed for approval varies from State to State; most States require approval or rejection of such programs within 15 days, but in a few States, there is a 30-day turnaround.

The procedures for submitting and obtaining approval for an STC program are similar across all States. First, each State requires that the individuals involved in the program be specified by name and/or Social Security number. Second, each program must be approved by the bargaining agent for each collective bargaining agreement covering any of the individuals affected by the program. Third, the submitted program must describe the manner in which fringe benefits are provided.[8] Two other provisions of STC programs are common to all States. All STC programs are financed through changes in a firm's experience rating in subsequent years. Also, the STC benefits that employees receive are deducted from the total unemployment benefits for which they are eligible.

STC Programs in Europe and Canada

STC programs are used in a number of European countries, including Austria, Belgium, Denmark, France, Germany, Italy, Luxembourg, the Netherlands, Norway, Sweden, and the

United Kingdom.[9] An STC program also was recently introduced in Canada. As in the United States, the primary objective of these programs is to avert layoffs during cyclical downturns. Although each country's program is administered differently, many of the programs have similar characteristics. Table 2 summarizes the main characteristics of STC programs in Belgium, Canada, France, and Germany, countries for which detailed information on STC programs is readily available.

The procedures for establishing an STC plan vary considerably. In Germany, France, and Canada, firms that plan to use STC are required to prepare an STC plan first. This plan typically describes the economic conditions that require the firm to reduce its production and estimates the number of employees involved and the duration of STC. This plan must be approved by both the Federal agency that administers the UI system and by any affected unions or other groups representing workers. Both Germany and Canada require a minimum reduction in employment to have an STC plan approved. In Canada, employment must be reduced by at least 20 percent in order for an STC plan to be approved. In Belgium, firms do not need to obtain approval from either the government or workers nor are they required to make a minimum reduction in their workforce. Instead, employers simply are required to notify the government and affected workers of their intentions 7 days prior to initiating an STC plan.

Belgium, Canada, and Germany all impose time limits of 6 months for STC plans, except under special circumstances. French workers can receive STC benefits for a maximum of 600 hours per year.[10] The short time limits for the receipt of benefits reflect the fact that STC programs are designed to provide employment stability during short-term economic downturns.

Employers in Belgium, France, and Germany have considerable flexibility in deciding how to reduce working hours across the firm. The reduction in hours can range from 10 percent to 100 percent. Uniform reductions are not mandated in either Germany or Belgium. This allows employers to reduce the hours of some workers more than others, based on worker preference or other criteria. In addition, employers typically can choose to reduce the number of hours worked per day, the days worked per week, or place employees on a rotating layoff schedule. In both Belgium and Germany, state approval is not needed to make modifications to the STC plan once the plan has been approved.

STC programs are funded and run like regular UI programs. In Belgium and Germany, STC programs are funded by uniform payroll taxes and the wage replacement rate for STC workers is the same as for workers receiving regular UI benefits. If a worker is subsequently laid off after collecting STC benefits, however, he or she is still entitled to the maximum UI benefit period. Finally, employers are required to maintain fringe benefits for workers receiving STC.

Table 2
Characteristics of Selected Countries' Short-Time Compensation Programs

Country	Requires Gov't. Approval?	Requires Worker Approval?	Gov't. Approval Needed To Change Plan?	Uniform Reduction in Hours Required?	Maximum Duration of Plan	Range of Reduction of Hours	Replacement Rate	Employers Req'd to Maintain Fringe Benefits?	Benefits Counted Against UI Eligiblity?	Funding	Other
Belgium	no[a,b]	no[b]	no[b]	no	6 months (V); indefinite (A&H)	min: none max: 100%	55-60% (same as regular UI)		no	non-experience rated payroll tax	Only blue-collar workers eligible
Canada	yes	yes	yes	yes	26-38 weeks	min: 20% avg: 38% max: 60%	57% (same as regular UI)	yes	no	uniform employer and employee tax	Cannot participate if reduction is seasonal or firm involved in labor dispute
France	yes	yes	yes		600 hrs/yr		50%[c] subject to ceiling	yes	no	General Revenues	
Germany	yes	yes	no	no	6-24 months	min:3% (A&H) or 10% (V) avg:25-40% max:100%	63-68% (same as regular UI)	yes[d]	no	non-experience rated payroll tax	1. Benefits paid through employer 2. Must demonstrate that downturn not cyclical

[a]Except when due to a possible strike-related reason.
[b]Must notify 7 days in advance.
[c]State reimburses workers for 65% of minimum wage for reductions below 36 hours per week. Employer STC payments raise reimbursement rate to 50% of gross wages subject to a ceiling. State will reimburse employers for 50 to 80% of this payment for 3 to 6 months.
[d]Government subsidizes/pays employee contribution.

Sources: Belgium: Abraham and Houseman (1993); Vroman (1992b); Canada: Employment and Immigration Canada (1993); France: David Grey, University of Ottawa; Abraham and Houseman (1993); Germany: Abraham and Houseman (1993); Abraham and Houseman (1993); Vroman (1992b)

Evaluating STC Programs

STC programs are expected to have different effects on employers and employees. Evaluations of these programs must assess what impact STC has on both of these groups. Furthermore, an assessment of STC programs must examine whether benefits outweigh the costs.

Effects on Employees

The primary effect of STC on employees is the prevention of layoffs. STC spreads the burden of unemployment across all members of a firm, rather than concentrating the effect of an economic downturn on the subset of workers who are laid off. The costs of an STC program are borne by those workers who would not have been laid off in the form of reduced income from wages for the duration of the program. Against this must be set the benefits of increased income among those who would have been laid off and the benefits of increased leisure among workers who would not have been laid off. Workers also enjoy other nonmonetary benefits, such as higher skill retention and reduced psychological and physical effects of unemployment. However, STC programs may also discourage employees from undergoing training to learn new skills. To the extent that this prevents workers from adjusting to structural changes in the economy, STC programs may provide undesirable incentives to delay adjustment.

Effects on Employers

It is unclear whether STC reduces costs for all employers. STC programs are likely to impose a number of costs on employers, including subsequent increases in the UI tax rate, and costs associated with the administration of the STC program. Since fringe benefits usually are not maintained by an employer after a worker is laid off, an additional cost of STC usage for employers is the maintenance of fringe benefit costs. The use of an STC program should, however, help reduce other costs to the employer. For example, costs associated with the recruitment and training of workers hired to replace those workers who did not return to the company after being recalled to work would be eliminated. Other benefits that employers may realize from STC programs are lower staff turnover and higher productivity. Further, since it is likely that the least senior members of the workforce will be the first to be laid off, payroll costs also may be diminished more for firms using an STC program, since senior workers generally receive higher pay.

STC programs may provide negative incentives for employers to delay adjustments to real changes in the economy. In particular, because of the difficulty in distinguishing which industries are experiencing cyclical declines and which are experiencing structural declines, STC may subsidize employment for workers in firms that are becoming less competitive.

Costs and Benefits to Society

STC programs impose additional costs on UI trust funds for several reasons. First, STC programs typically eliminate the 2-week waiting period usually required before UI recipients are eligible to receive benefits. Second, not all workers who are laid off collect UI benefits. Finally, administering STC benefits may be more costly. Previous studies seem to indicate that, although the costs associated with administering an STC program initially are high, costs often drop as State UI administrators become more experienced in operating such a program. In addition, STC programs impose other indirect costs on society if STC is improperly used to postpone layoffs in industries that are undergoing permanent structural change, rather than merely experiencing a temporary cyclical downturn.

The benefits of STC programs can be grouped into two categories. First, STC programs reduce the hiring and training costs associated with laying off a portion of a firm's labor force. Although firms indicate that they expect layoffs to be of a temporary nature when they lay off their workers, this does not guarantee that each firm will be able to recall all of its employees when production increases. STC programs save firms the expenses associated with hiring and training new employees to replace those laid off but who did not return to work when recalled. Second, STC programs reduce the costs associated with the stress of unemployment. Included among the costs of unemployment are the costs associated with increased health care, counseling, and police protection. While these costs often are difficult to accurately quantify, they are, nevertheless, significant. Table 3 summarizes the costs and benefits of STC programs.

Table 3
Benefits and Costs of Short-time Compensation

Benefits	**Costs**
Reduced Hiring and Retraining Costs	Additional Costs to the UI trust funds
Reduced Unemployment-Related Costs	· UI Waiting Period Eliminated
· Reduced health care costs	· Additional UI payments
· Reduced counseling costs	· STC payments for those workers subsequently laid off
· Reduced crime costs	

STC Program Experience

A few studies have been conducted of the existing STC programs, both in the U.S and abroad. In general, these evaluations have found that the benefits of STC outweigh the costs associated with the programs. The following sections briefly describe the findings of these evaluations.

Effect on Layoffs and Employment Stability

The experiences with the various STC programs in the U.S. differ significantly from those in Europe. In the U.S., little empricial research exists on the effect of STC on layoffs. Existing research indicates that employers continued to rely heavily on layoffs to reduce workers' hours, even in States and firms in which STC programs were available. One study estimates that STC programs reduced layoffs by 1.45 percent and that the mean number of hours spent on regular UI for "the mean STC employer...[was] nearly eight times the mean number of hours spent on STC."[11]

> [These results raise] questions about how worksharing plans are structured and how wide...STC coverage [is] within firms with worksharing...As it stands, the widespread use of layoffs by worksharing employers severely limits the amount of employment stabilization that could be expected from STC.[12]

Studies of STC programs abroad, on the other hand, have found that these programs reduce layoffs and increase employment stability. A recent study of Canada's STC program found that it averted 64 percent of the layoffs that would have taken place without such a program.[13] In addition, the Canadian study found that STC firms were significantly less likely to engage in layoffs throughout the firm, even when only a portion of the firm participated in the STC program.[14] Nevertheless, 29 percent of the workers who had their hours reduced by STC were subsequently laid off when the STC program ended.[15] The Canadian study also indicated that less than 5 percent of workers undertook any training during the STC program period compared to 10 percent of workers laid off from firms that did not undertake STC programs.[16]

Two empirical studies on unemployment programs in Europe also lend some support to this conclusion. A study of German STC programs found that when output falls, STC utilization rises in the short run, thereby preventing layoffs and enhancing employment stability. If the decline in output continues to persist for over a year, however, the use of STC programs declines significantly as employers and employees adjust to the continued fall in demand.[17]

The second study, which looked at manufacturing industries that experienced declines in output between 1973 and 1990, found that German, French and Belgian firms reduced employment at a much slower rate than did American firms during the same period. European firms also reduced work hours at a slower rate than American firms. Thus, American firms are more prone to layoff.[18] Although this study provides some evidence that European measures to increase job security have succeeded in reducing layoffs in the short run, while still allowing companies to downsize in the long run, there are two shortcomings to the study's conclusions. First, it is unclear how applicable these results are to other industries. Second, it is unclear how much of this effect is a result of STC programs. An earlier study by the same authors estimated that 21 to 90 percent of the reduction in hours of German firms can be attributed to STC programs; however, similar analyses were not conducted for French or Belgian firms.[19]

Extent of STC Program Use

One facet of the STC programs employed in the U.S. is the relative lack of use by qualifying firms. Figure 1 graphically presents the percentage of total unemployment benefits that STC payments made up (the benefit ratio) by State starting in 1982. STC claims generally represent less than 1 percent of the UI claims in the States with STC programs. The largest benefit ratio occurs in Arizona in 1985, when STC benefits comprised 1.96 percent of regular UI claims. The only other period in which STC claims accounted for more than 1 percent of regular UI claims occurred in 1982 (also in Arizona).

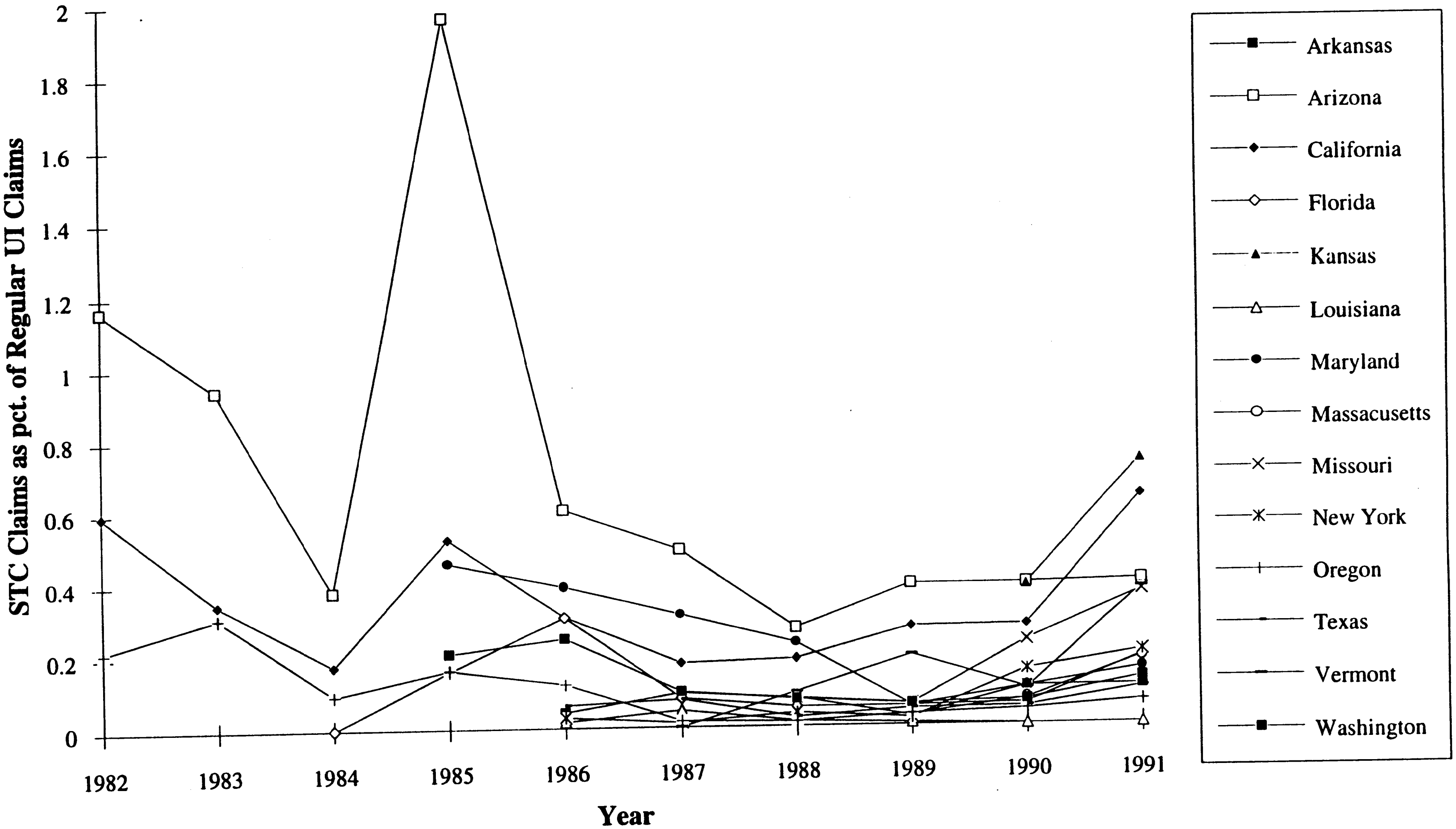
It has been suggested that qualifying firms in the U.S. may not participate in STC programs as much as their European counterparts because U.S. firms in some States must pay an additional surtax on payrolls. By examining the changes in the STC claims as a percentage of regular UI claims in the State of California since 1987 (the year the surtax was eliminated), it might be possible to determine if the surtax discouraged participation in the California worksharing program. From 1987 to 1991, the number of STC claims as a percentage of regular UI claims rose steadily (from 0.18 percent to 0.63 percent), suggesting that the surtax might have deterred firms from implementing a worksharing program. Because other factors, including a broader dissemination of information about the worksharing program, may have been responsible for the increase in worksharing use, it is not possible to attribute the marked increase in worksharing to the elimination of the surtax.

In contrast, program use is much greater in Europe than in the U.S. Between 1970 and 1991, German STC utilization rates (as a percentage of regular UI) were above 1.7 percent for 14 of the 22 years. During 10 of these years, utilization rates were as high as 10 percent. In Belgium, STC utilization rates exceeded 30 percent in 6 of these years. Figure 2 compares program use in the U.S., Germany, and Belgium.

In both the U.S. and abroad, STC program use is highly cyclical. In the U.S., STC usage rises as unemployment increases.[20] In Canada, the highest levels of program participation occurred during recessionary years: in 1982 and 1983 (295,800 workers participated) and 1990 and 1991 (336,800 workers). In between these periods, program participation fell dramatically, ranging from 56,378 in 1984 to 31,282 in 1988.[21]

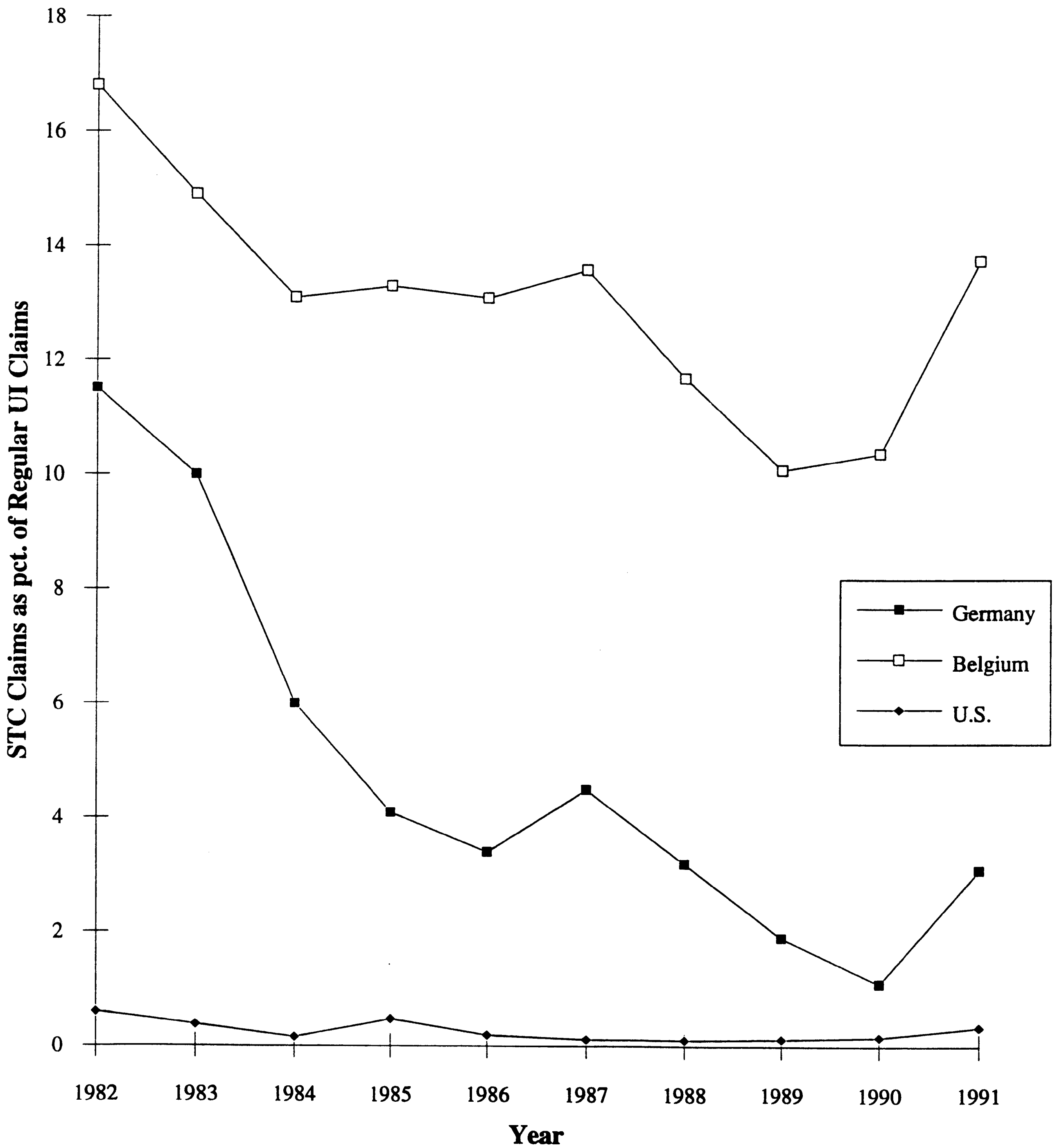
Finally, program use in the U.S. is characterized by low seasonality relative to both Germany and Belgium. In both Germany and Belgium, January and February claims average at least twice the claims filed in July and August.[22] In the U.S., however, the average seasonality of STC claims was found to be relatively modest.[23] This could be the result of the existence of some State provisions prohibiting the use of STC programs to reduce seasonal unemployment.

Figure 1
Comparison of STC Claims Across States



Source: Vroman (1992, Table 2)

Figure 2
Comparison of STC Claims Across Countries



Source: Vroman (1992, Tables 2, 5)

Characteristics of Employers Who Utilize STC

Little research has been conducted on the nature of the firms that choose to participate (or not to participate) in STC programs in the U.S. One study that provides insight into this question is a fairly extensive study completed by the State of California in 1982. It found that the majority of the firms involved in the STC program between 1979 and 1981 were manufacturing firms. Firms that participated in the STC program tended to be large employers. The firm's union status was not related to employer participation. Consistent with results of later research, the California study also found very low levels of participation in the STC program. The study seems to indicate that such low participation was the result of lack of information, which is not surprising considering the relative newness of the California STC program at the time of the study.[24] Given the limited nature of this study, however, it is not possible to draw any concrete conclusions concerning the characteristics of participant firms in STC programs.

Evidence from studies of STC in other countries indicates that STC programs are usually used by manufacturing and trade firms. In 1983, for example, STC participation rates in Germany were particularly high for the industrial region of the country. While other reasons may have contributed to the high usage of STC, the fact that many automobile, mechanical and electrical engineering, textile, and clothing firms are located in this region also may have been a factor.[25] Likewise, the Canadian study found that STC programs were used more frequently by firms in the manufacturing and trade sectors.[26]

Effect on Employer's UI Tax Rate

In the U.S., STC use often increases an employer's UI tax rate. STC programs may raise an employer's UI tax rate in one of two ways: either by requiring participating employers to pay an additional surtax or through normal increases in a firm's experience rated tax rate for firms that are not already at the maximum tax rate. The average UI benefit under STC usually is higher than the average benefit if an employer chooses to lay off employees, since the hours of all workers in the unit are reduced, rather than just those of the least senior employees. Because more senior workers also tend to be more highly compensated, spreading the burden of adjustment across all employees also raises the average UI benefit payment.[27] Unless the firm already is paying the highest tax rate, the higher UI benefit payments also will raise the firm's UI tax rate under the experience rating systems used in the U.S.

The use of STC has no effect on an employer's tax rate in Germany, France, Belgium, and Canada. Employers in these countries pay a uniform payroll tax to finance the program. These rates remain the same regardless of whether a firm chooses to use STC or layoffs to respond to downturns in demand.

Effect on Employees' Fringe Benefits

Although many States do not require the continued payment of full fringe benefits during participation in an STC program, most plans submitted to States for approval include provisions for the maintenance of full fringe benefits. In fact, over 90 percent of all employees participating in an STC program continue to receive full fringe benefits despite the fact that maintaining full fringe benefits drives up the hourly cost of labor to these employers.[28]

In both Germany and Canada, employers must maintain health insurance benefits. In Germany, employers must maintain contributions to the national health insurance and pension fund when an employee is on STC. In addition, the firm is required to pay the difference between the worker's required contribution under full employment and the worker's contribution after STC begins. This additional employer payment is subsequently reimbursed by the government. In Canada, fringe benefits must be maintained by the firm when an STC agreement is in effect.

Measured Costs and Benefits of STC Programs

Three studies have estimated the costs and benefits of STC programs. The California STC evaluation estimated that the benefit-to-cost ratio for the STC program between 1979 and 1981 was 1.4 to 1.[29] The most recent Canadian study (1993) found that the benefits of the program exceeded the costs by a factor of 2.6 to 1,[30] while an evaluation in 1984 estimated the benefit-to-cost ratio as 5.7 to 1.[31]

There are several reasons why the range in estimates of the benefit-to-cost ratio for STC programs is so large. First, and most importantly, the Canadian and California evaluations included different costs and benefits when calculating the benefit-to-cost ratio. The Canadian estimates weigh the additional costs of administering STC programs against the savings in hiring and retraining costs and the avoidance of unemployment stress. The California study, however, includes the loss of government revenues among the costs of STC. On the benefit side, the California study does not include the avoidance of unemployment-related stress among the benefits. Table 4 summarizes the costs and benefits of using STC calculated for each of the different evaluations.

Several adjustments can be made in order to make the benefit-to-cost ratios more comparable. Although the California study includes the reduction in income taxes as a net cost to society, it is merely a transfer from one group to another and should not be counted as a net cost to society.[32] Once this cost is eliminated, the benefit-to-cost ratio for the California program increases to 2.6.

Second, both Canadian studies include the cost of additional unemployment-related costs, such as increased need for health care, counseling, and police protection, while the California study did not. While unemployment-related costs may be significant, they often are hard to estimate.[33] Excluding the costs associated with unemployment-related stress from the benefit to cost ratio, considerably reduces the benefit-to-cost ratio as shown in Table 4. The benefit-to-cost ratio for California between 1979 to 1981 (which does not include the cost of unemployment-related stress and scanning), remains at 2.6, while the ratios for Canada fall to 1.3 in 1984 and 0.35 in 1993.

Table 4
Measured Benefits and Costs of STC Programs
(Per STC Worker Per Year)

	Original Estimates			Adjusted Estimates			Adjusted Estimates Less Cost of Unemployment Related Stress and Scarring		
Benefits	Canada 93	Canada 84	California 79-81	Canada 93	Canada 84	California 79-81	Canada 93	Canada 84	California 79-81
Avoidance of Unemployment Related Stress	656	375		656	375				
Avoidance of Scarring Costs	192			192					
Hiring & Retraining Costs	128	111	757	128	111	757	128	111	757
Total Benefits	**976**	**486**	**757**	**976**	**486**	**757**	**128**	**111**	**757**
Costs									
Additional Costs to the UI Trust Fund	371	85	162	371	85	162	371	85	162
Loss of Other Govt. Revenues			259			0			0
Additional Costs Incurred by Firms			124			124			124
Total Costs	**371**	**85**	**545**	**371**	**85**	**286**	**371**	**85**	**286**
Benefit Cost Ratio	**2.6**	**5.7**	**1.4**	**2.6**	**5.7**	**2.6**	**0.4**	**1.3**	**2.6**

There are several other reasons why the benefit-to-cost ratios differ. First, the Canadian and California programs are administered differently. Under the Canadian STC program, benefits do not count towards a worker's regular UI benefits. Since 28 to 29 percent of Canadian STC participants subsequently are laid off, this represents an additional cost to the UI system that is not usually incurred in the United States. Second, the evaluations reflect the different economic conditions found in different time periods and localities. For example, in the early Canadian study, evaluators estimated that firms recalled 90 percent of their laid-off employees, while the California evaluators estimated that firms could recall 75 percent of their employees.[34] Third, the costs associated with hiring and training workers vary between Canada and the United States.

In summary, the existing estimates of benefits-to-costs range widely. Much of this variation can be attributed to differences in the methodologies used to calculate these estimates. Other differences in these estimates can be explained by differences in how STC programs are operated, as well as the state of the labor market in which they operate. All of the adjusted ratios, however, indicate that the benefits of STC outweigh the costs by a factor of 2.6. Even when the avoidance of unemployment-related costs is eliminated from the benefits of STC, two of the three evaluations indicate that the benefits of STC still outweigh the costs.

Possible Reasons for the Low STC Usage in the United States

Several reasons have been proposed to explain the more extensive use of STC in countries other than the U.S. One explanation for the low STC usage in the U.S. is that the cost of maintaining employee benefits makes using layoffs a more attractive option to employers. Alternatively, the numerous regulations which restrict a firm's ability to lay off employees in European countries may make STC a relatively more attractive option. In addition, DOL restrictions prohibit insolvent states from adopting legislation to support an STC program.[35]

The incentives inherent in the U.S. UI system also contribute to the low usage of STC programs. STC programs abroad are financed by flat payroll taxes. Thus, firms do not realize any additional costs from using STC. In contrast, in the U.S., STC benefits, like regular UI benefits, affect an employer's tax rate through the experience rating system. Further, many of the first States to incorporate STC programs initially imposed surtaxes on firms utilizing STC programs.[36] Currently, only 4 of the 17 States impose such a surtax, so the extent to which additional surtaxes discourage participation in STC programs may no longer be an issue.

STC programs abroad also offer employers substantial flexibility, making them relatively more attractive than those in the U.S. Firms in Europe are generally not required to make uniform reductions in staff when implementing STC programs. Furthermore, changes in STC plans generally do not require government approval.

Low STC use in the U.S. may also be partially attributed to factors that are common to the startup of any new program. Many employers simply may be unaware that the program exists.

Some employers may be reluctant to use the program because they are uncertain of the costs. Finally, workers may react more favorably to the program abroad than they do in the U.S.

Increasing U.S. Use of STC

Several methods have been proposed to achieve greater use of STC programs in the U.S. First, information concerning the existing STC programs needs to be disseminated broadly among firms and State UI staffs and administrative agencies. Second, changing administrative procedures to increase the speed with which STC programs can be approved, implemented, and modified and increasing the flexibility of STC programs might encourage more employers to use them. Third, reducing the effect of STC program use on an employer's payroll tax rate may encourage further use by employers. Fourth, previous evaluations suggest that having employers administer the STC benefits payments in their payroll checks would reduce worker inconvenience and may improve employee participation. According to results of surveys conducted by the State of California as part of its study, 27 percent of the employees who participated in a worksharing program mentioned benefit collection problems as a disadvantage of the program. Sixty-one percent of firm managers felt likewise.[37] Fifth, employee participation might be increased further if STC benefits were not deducted from total unemployment benefits for the year; however, there is no direct evidence to substantiate this claim. Finally, by allowing varying reduction in weekly hours, additional workers might be willing to participate in the available programs.[38]

Experience with STC

Studies of individuals who have had experience with the operation of an STC program have been positive. Public officials, employees, employers and unions all have expressed satisfaction with the STC program. No State has repealed its STC program. Program requirements in these States have become less stringent. Many employers who have used STC have used it more than once. Employees who have participated in an STC program have been satisfied with their experience.[39]

Areas for Further Research

A new evaluation of STC programs in the U.S. might be undertaken to assess whether the results of earlier studies still hold. The most recent evaluation of STC programs in the United States examined STC programs in three States in 1983. The study focused on the program experiences in Arizona, Oregon, and California. Two of these States, Oregon and Arizona, had just initiated their STC programs. Now that several States have operated STC programs for nearly a decade, a new evaluation of STC programs might be useful.

A second area of research that might be undertaken is an evaluation of how firms in States with STC programs decide whether to participate. Possible reasons that many firms may not participate in STC programs might be lack of information, differential cost or fringe benefits, the impact of experience rating on payroll tax rates, the cost of tax surcharges, or possible loss of more productive, and hence mobile, employees. This might be useful in designing STC policies.

Another area of possible research might be to examine the effects that STC has on employees, worker productivity, social and psychological well being, employee demographics, and provision of fringe benefits. These are areas in which information is currently lacking.

Finally, it may be useful to explore when and whether or not it would be useful to match employees on STC programs with training programs. Although STC programs are designed to avert *temporary* layoffs, it often is difficult to tell whether a layoff will be temporary until after a worker is recalled. In cases where layoffs that were anticipated to be temporary become permanent, it may be beneficial to encourage workers to seek training while on STC. A study examining under which scenarios it is most beneficial to encourage training while on STC might be useful.

Endnotes

1. Each State or country specifies the procedures for the implementation of such a plan, as well as the guidelines regarding the length of the STC, the number of workers affected, and the reduction in hours afforded by the plan.

2. Most notably, short-time compensation was considered again during the post-World War II recessionary downturns in the 1960s and in the periods of "stagflation" in the mid 1970s. See Best (1981, pp. 1-12).

3. Sections 1279.5 and 978.5 in the California Unemployment Insurance Code.

4. Representative Patricia Schroeder of Colorado sponsored the Federal legislation (PL 97-248) enacted in 1982 that mandated the Secretary of Labor develop such model legislative language. Only three States had already adopted STC programs prior to the availability of the model.

5. Benefits for STC payments are deducted from employer UI trust fund balances in reserve ratio States or added to the employer's benefit ratio in benefit ratio States.

6. Maryland State law allows the employer to reduce hours more than 50 percent if special approval is given by the Secretary of Employment and Training.

7. Due to State trust fund debts and the fear of adverse effects on State trust funds, both Texas and California had an additional STC tax previously. California repealed its surtax in 1987, and Texas' surtax was in effect only from 1987 to 1989. Also, Arizona's original regulations established a 3.0 percent surtax (it has since been lowered to 2.0 percent).

8. In Arkansas, Louisiana, Massachusetts, New York, and Washington, State law requires that the employer maintain all or partial fringe benefits.

9. Randall and Wright (1993, p. 11).

10. Abraham and Houseman (1993b, Table 2).

11. Kerachsky, et al. (1986, p. 188).

12. Vroman (1992b, pp. 14-15). He also suggests that the results achieved in Kerachsky's study could be the consequence of a number of factors: (1) the worksharing plans may cover only narrow classes of workers, rather than the majority of workers; (2) the firms in the study sample may not have been representative of the "typical" STC firm; (3) there may have been problems with the sampling plan; and (4) the layoff data in the study may have referred to time periods when the worksharing plans were not in effect.

13. Employment and Immigration Canada (1993, p. ii).

14. Employment and Immigration Canada (1993, p. 12).

15. Employment and Immigration Canada (1993, p. 11).

16. Employment and Immigration Canada (1993, p. 16).

17. Vroman (1992b, p. 28).

18. Abraham and Houseman (1992, p. 16). This paper extends the analysis found in Abraham and Houseman (1993a) to include French and Belgian firms.

19. Abraham and Houseman (1993a, p. 90). Percents vary by industry.

20. Vroman (1992b, p. 12).

21. Employment and Immigration Canada (1993, pp. 2-3).

22. Vroman (1992b, p. 24).

23. Vroman (1992b, p. 14).

24. State of California survey (1982, pp. 1.1-1.23, 4.1-5.4).

25. Meisel (1984, p. 59).

26. Employment and Immigration Canada (1993, p. 23).

27. There is some contrary evidence on this point. See Morand (1990, p. 324)

28. Vroman (1992b, p. 9).

29. State of California (1982, p. 1.16-1.17).

30. Employment and Immigration Canada (1993, p. 29).

31. Reid and Meltz (1984, p. 115-116).

32. Reid and Meltz (1984, p. 115).

33. Reid and Meltz (1984, p. 116). Reid estimates that these costs are 25 percent of direct UI costs.

34. Reid and Meltz (1984, p. 110).

35. Morand (1990, pp. 322-323).

36. California, Arizona, and Oregon, the first three States to include STC provisions in their UI laws, all initially imposed an additional surtax.

37. State of California survey (1982, pp. 7.27-7.29). The results of the survey of firm managers suggest that at least one individual from a majority of the workgroups that participated in the program experienced some difficulties in collecting his or her benefits.

38. Vroman (1992b, pp. 31-35).

39. Morand (1990, pp. 338-342).

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Government Funded Relocation Assistance: A Summary

Since the days of the Manpower Development and Training Act (MDTA), out-of-area job search and relocation assistance has been offered as a service to various categories of unemployed individuals, with recent programs often targeted at dislocated workers. Despite the fact that out-of-area job search and relocation often has positive wage effects for those who relocate voluntarily, both those who quit and those who are laid off, government-funded relocation services have been relatively ineffective in reducing unemployment. In the demonstrations outlined in this report, take-up rates for government-funded relocation assistance are relatively low. Further, evidence indicates that almost one-fifth of the relocated workers return to their original geographic area.

Relocation can move individuals from geographic areas of low labor demand to areas of high labor demand and individuals whose skills are no longer needed in one area to an area in which they are in demand. Thus, those in professional occupations operate in a national labor market. Some craft occupation workers frequently relocate, at least temporarily, on a regional and sometimes national basis. Younger workers are more likely than older workers to relocate. Theoretically, out-of-area job search and relocation can also expand the labor market search area for dislocated workers, speed up their reemployment, and improve their wage replacement rate. Considering all of the potential benefits from relocation, why have past relocation programs been so unsuccessful?

Relocation involves both economic and noneconomic costs and benefits. The economic costs include the costs of a job search and moving (including the sale and purchase of a house), possible higher living costs if the move is from a low-cost (rural) area to a high-cost (urban) area, and the cost of a spouse's job search if the household has two earners. Noneconomic costs include separation from relatives and friends, as well as the logistical costs of locating new schools, doctors, supermarkets, banks, auto repair shops, and other service providers.

One 1988 estimate put the cost to a private company of moving an employee at $4,835. If one adds to this the selling and buying of a house, the cost can become substantial. In contrast, most of the relocation assistance programs identified in this report would pay only up to several hundred dollars for out-of-area job search and less than $1,000 for relocation expenses.

Dislocated workers are defined as experienced workers (at least three years of experience with previous employer). They are more likely to be older, married, and homeowners, as well as living in two-earner households. This implies that both the economic and noneconomic costs of relocation are higher than the relocation benefits offered. In addition, the workers face average wage replacement rates of less than one.

All of this suggests that, as a service to dislocated workers, relocation should be offered to younger, single workers who have skills that are marketable on a national basis but for which the usual labor market is local or regional. Further, although relocation appears to be a low-cost service compared with retraining, the relocation costs that may be reimbursed are low relative to the actual costs of relocating, particularly for dislocated workers.

Introduction

Various schemes have been proposed and tested to shorten the duration of unemployment, especially among dislocated workers. Intensive classroom training, on-the-job training, bonuses for early reemployment, programs to encourage self-employment, and relocation assistance are among the alternative uses of unemployment insurance (UI) benefits that have been explored. Several projects that provided relocation assistance to UI recipients looking for jobs outside their geographic areas have been conducted. These programs have been relatively ineffective in relocating workers. This report reviews these programs and the reasons for their lack of popularity.

Mobility in the United States

Between March 1990 and March 1991, 4.5 million people or 17 percent of all Americans 1 year of age or older moved to new residences. This rate includes the 0.6 percent of the population who resided outside the United States at the beginning of the year. Mobility rates have been declining during the past 40 years; in 1990-91, the rate was at its lowest since the early 1980s.[1]

People move for a variety of reasons. Most people move in response to changes in family status and housing needs (for example, marriage, divorce, or the addition of a family member). These life-cycle moves are generally local moves. More than 10 percent (10.3 percent) of Americans moved locally during 1990-91, accounting for 61 percent of all moves in the United States during this time period.[2] Less prevalent are long-distance moves, which are generally undertaken for economic reasons. More than 6 percent of the population (6.1 percent) undertook long-distance moves in 1990-91.[3] Many long-distance moves are the result of corporate and military transfers, new jobs, or a desire to work elsewhere.

When considering whether to move for economic reasons, workers weigh a variety of factors, including their preferences for relocation and the relative costs and benefits of moving. There are many noneconomic reasons, such as family and community ties, why workers often prefer not to move. When considering whether the benefits (the relative wage gain or loss associated with a move) outweigh the costs, workers must consider not only the actual relocation costs but also the noneconomic costs.

The economic benefits associated with moving can be thought of in terms of relative wage gains. These gains vary depending on the age of the worker and the type of separation from his/her previous employment. Younger workers tend to experience larger wage gains, as do those who quit their jobs (compared with those who are laid off). For example, between 1970 and 1981, the average wage gain among individuals who voluntarily left their jobs and did not receive government relocation assistance was 5.3 percent for younger workers and 4.2 percent for older workers. The average wage gains for laid-off workers were 1.6 percent and 0.9 percent for younger and older workers, respectively.[4] Because only 15 percent of the households included in this study also moved to new residences, one might expect wage gains for those who voluntarily relocated to be even higher.

The costs associated with moving to a new residence can be substantial. In addition to the costs of moving household goods, those relocating to a new residence may also incur indirect moving costs, such as family transportation, meals, and lodging. Homeowners also may incur costs when selling their old homes and purchasing new ones. Finally, those with working spouses must also consider the costs associated with a second job search.

Certain groups are more likely to move than others. Of long-distance movers in 1990-91, men were more likely to move than women (6.4 percent of men moved compared with 5.8 percent of women). Whites were more likely to move long distances than blacks (6.2 percent of whites compared with 5.4 percent of blacks), although blacks were more likely to move at all. Of those in their twenties, 12 percent to 13 percent moved long distances, a rate substantially higher than those in all other age groups. Finally, those with higher education levels tend to be more mobile than those with lower levels.[5]

The Role of Geographic Mobility in the Labor Market

Economists generally divide unemployment into three types: frictional, cyclical, and structural. Such a classification system is useful when considering the appropriate policy responses for reducing unemployment. Frictional unemployment is unemployment that is caused by the normal functioning of the market and is usually addressed with the provision of job search assistance through an employment service. Cyclical unemployment is unemployment associated with reduced demand and is addressed through the provision of UI and job search assistance. Structural unemployment refers to the unemployment caused by mismatches between the characteristics of workers and the needs of the marketplace. One group of structurally unemployed are older workers whose skills may no longer be in demand because of changes in technology, demand, or comparative advantage: these are the displaced or dislocated workers. Policies directed toward the reemployment of dislocated workers often attempt to modify the supply characteristics of workers through retraining, modifications in their expected wages, or geographic relocation.

Geographic mobility helps clear the labor market and thus reduces all types of unemployment by allowing workers to move from areas of low labor demand to areas of high labor demand. In the case of structural unemployment, geographic mobility can reduce unemployment by moving workers from areas where the demand for certain skills is low to areas where the demand is higher. Because the costs of moving are often higher for dislocated workers, government-funded relocation assistance can reduce unemployment by both expanding the labor market for workers who traditionally conduct job searches only in local labor markets and by reducing the costs associated with moving.

Government-Funded Relocation Projects

This report reviews the experience of two sets of programs that provided relocation assistance to individuals. The first group consists of 37 relocation programs conducted in the late 1960s under the Manpower Development and Training Act (MDTA). The second group consists of six recent projects aimed at reemploying dislocated workers:

> Downriver Community Conference Economic Readjustment Program in the Detroit, Michigan, metropolitan area

- Four dislocated worker demonstration programs funded by the Comprehensive Employment and Training Act (CETA), which provided relocation assistance to dislocated workers in Buffalo, New York; Lehigh Valley, Pennsylvania; Mid-Willamette Valley, Oregon; and Yakima, Washington

- New Jersey Unemployment Insurance Reemployment Demonstration Project

Table 1 summarizes the main characteristics of these programs. As the table demonstrates, the programs vary along several dimensions, including the populations targeted by each program, the size of each program, relocation and other types of services provided, and how participants were selected.

Target Populations

One of the most important ways that the projects differ is in their focus. In the MDTA relocation programs, assistance was provided to a variety of groups, including, but not limited to, dislocated workers. In fact, the majority of workers (72 percent) who received relocation assistance were classified as the general unemployed, whereas only 20 percent were classified as trained workers and 8 percent as unemployed as a result of mass layoffs.[6] The relocation services provided by the other programs, however, were designed specifically to assist dislocated workers in becoming reemployed.

The characteristics of the local labor markets varied considerably among projects. It appears that most of the MDTA programs were targeted at specific counties or areas that had relatively high unemployment rates or recent mass layoffs. All of the CETA-funded demonstrations targeted workers in areas where mass layoffs had occurred. These areas had high local unemployment rates, ranging from 10.3 percent in Mid-Willamette Valley to 17 percent in Yakima. Each program reserved some portion of its services for workers recently laid off. The New Jersey demonstration also targeted services toward "experienced workers likely to be displaced from their jobs." However, these workers were selected using eligibility screens other than regional area screens. Workers who were over 25, had been employed by their previous employer for at least 3 years, who did not have a specific recall date, who were not hired through a union hall, and who had received one UI payment were eligible to participate in the New Jersey demonstration.

The relocation programs and the reemployment programs differed in the number of people they attempted to serve. The smallest MDTA relocation project relocated 2 people, whereas the largest relocated 981 people. Of the reemployment demonstrations, the smallest project reviewed was in Yakima. It served only 243 workers. The largest project, on the other hand, offered services to more than 3,800 workers.

Services Provided

Relocation assistance was the principle service provided during the MDTA-sponsored projects. In the other programs, relocation assistance was merely one of several reemployment services provided. Workers in these programs typically received intensive job search assistance, classroom training, or on-the-job training to assist them with becoming reemployed. Not all participants, therefore, utilized the relocation assistance services provided. In fact, few did.

Table 1
Characteristics of Selected Programs Offering Relocation Assistance

Project	Target Population	Program Dates	Scope of Program	How Participants Were Selected	Maximum Assistance	Participant Eligible to Receive:			Other Services Provided*
						$ for Out-of-Town Job Search	Flat Fee	Reimbursement Up to a Set Maximum	
MDTA Demonstrations	Various, including but not limited to dislocated workers	1965-68	38,793 workers in 37 projects in 28 States	Varied by project	$460	x	x	x	1
Downriver Community Conference	Dislocated workers in automotive-related industries	7/80-9/83	2,100 workers from 3 auto plants	Random assignment of laid-off workers	$600 for interviews; unspecified cap for relocation	x	x	x	1,2,3
Buffalo, NY	Dislocated workers in steel and auto industry	10/82-9/83	798 workers; 64% from 9 plants		$600 for interviews; $600 for relocation	x		x	1,2,3
Lehigh Valley, PA	Dislocated workers in steel and auto industry	10/82-9/83	1,028 workers; 62% from 12 plants		$200 for interviews; $500 for relocation	x		x	1,2,3
Mid-Willamette Valley, OR	Dislocated workers in pulp and paper industry	10/82-9/83	305 workers; 100% from 9 plants		$500 for interviews; $500 for relocation	x		x	1,2,3
Yakima, WA	Unskilled construction workers laid off by nuclear plants	10/82-9/83	243 workers; 22% from 6 plants		$200 for interviews; $600 for relocation	x	x		1,2,3
New Jersey UI Demonstration	Experienced workers likely to be displaced from jobs	7/86-fall '87	3,810 workers throughout State	Random selection	$400 for interviews; $300 to $1,000 for relocation	x	x		1,2,3

*Key:
1 - Job Search Assistance
2 - Classroom Training
3 - On-the-Job Training

Sources: MDTA Demos: Fairchild (1970) and Schnitzer (1970). Downriver: Kulik (1984). Buffalo: Jerrett et al. (1983) and Wright (1984a). Lehigh Valley: Jerrett et al. (1983) and Wichita (1984a). Mid-Willamette: Jerrett et al. (1983) and Wright (1984b). Yakima: Jerrett et al. (1983) and Wichita (1984b). New Jersey: Corson et al. (1989).

The types of relocation services provided to unemployed workers varied by project. Most projects reimbursed workers for out-of-area job interviews. Usually interviews had to be at least 50 miles away, and workers were allowed to claim reimbursement for multiple trips up to a set maximum reimbursement level. Reimbursement procedures for actual moving expenses were handled in different ways for different projects. Some provided reimbursement of moving expenses up to a set fee, whereas others provided lump sum bonuses to help offset expenses. For projects that offered reimbursement, there were major differences in the types of moving expenses that were covered. The Downriver program provided substantial reimbursement: a lump sum for moving, a transportation allowance for relocating families, and reimbursement for moving household goods. Some projects, like the Downriver program, offered both a lump sum payment to those choosing to move and reimbursement for certain activities.

Outcomes of Government-Funded Relocation Programs

Key outcomes, including program utilization rates, relocation rates, the percent of relocatees who subsequently moved, and the associated costs from the relocation and reemployment demonstration programs are summarized in Table 2.

Percent Receiving Any Related Assistance (Out-of-Area Job Search or Relocation Assistance)

The percent of eligible workers who used relocation services was considerably higher in the relocation programs than in the reemployment programs. In the relocation programs, 37 percent of eligible workers used relocation assistance to help them find jobs in other areas or to help with moving costs.[7] In the reemployment programs, utilization rates varied from 21 percent in Yakima, to 1 percent in Lehigh Valley, to less than 1 percent in New Jersey.

There seems to be several factors that explain this disparity in program utilization rates. First, the relocation programs tended to serve those who were more likely to move. Thus, workers in the MDTA relocation demonstrations were more likely to move even without government funds to offset some of the costs. Second, the local economies in Lehigh Valley and Yakima were more severely depressed than those in areas of New Jersey. During the reemployment demonstration period, Yakima and Lehigh Valley had unemployment rates of 11 percent[8] and 17 percent,[9] respectively, whereas during the New Jersey program, the State unemployment rate was only 5 percent, with unemployment rates in local program areas varying from 3.9 percent to 8 percent.[10] Finally, the different participation rates may be a function of how participants were selected. Workers participating in the New Jersey and Buffalo programs were selected at random from among a group of eligible participants; only those selected for the program were eligible to receive benefits. In other programs, workers received services on a walk-in basis. Even though this is not a valid comparison, it might indicate that many of the latter group of workers would not have moved without assistance.

Table 2
Outcomes of Selected Programs Offering Relocation Assistance

Project	Number/ Percent Receiving Relocation Assistance	Number/ Percent Relocated	Average Cost Per Relocated Participant	Local Unemployment Rate	Percent Who Moved After Relocation	Overall Project Placement Rate	Overall Hourly Wage at Layoff	Overall Post-Placement Wage	Overall Wage Replacement Rate
MDTA Demonstrations	37%	32%	$867	varied	20% moved back; 20% changed jobs	37%	n.a.	n.a.	n.a.
Downriver Community Conference	n.a.	8%	$944	12% to 18%	20%	49% to 73%	$10.00	n.a.	n.a.
Buffalo, NY	14	5 1%	n.a.	12.6%	n.a.	65%	$10.81	$6.87	58%
Lehigh Valley, PA	10 1%	2 0.2%	n.a.	11%	n.a.	32%	$9.45	$6.83	74%
Mid-Willamette Valley, OR	n.a.	18 6%	$555	10.3%	n.a.	61%	$10.29	$7.32	69%
Yakima, WA	51 21%	22 11%	$567	17%	n.a.	80%	$9.98	$8.11	81%
New Jersey UI Demonstration	0.5% to 0.8%	2 0.2%	n.a.	5%	n.a.	74%	$9.45	$9.21	97%

n.a.—not available

Sources: MDTA Demos: Fairchild (1970) and Schnitzer (1970). Downriver: Kulik (1984). Buffalo: Jerrett et al. (1983) and Wright (1984a). Lehigh Valley: Jerrett et al. (1983) and Wichita (1984a). Mid-Willamette: Jerrett et al. (1983) and Wright (1984b). Yakima: Jerrett et al. (1983) and Wichita (1984b). New Jersey: Corson et al. (1989).

Percent Relocating With Government Assistance

Relocation rates may differ from the utilization rates if even a few participants who interview for jobs outside their local areas actually receive job offers and decide to accept these offers. As in the case with program utilization rates, there is considerable disparity between the relocation rates of the MDTA-sponsored relocation projects and those of the later reemployment programs. Relocation rates for the MDTA demonstration programs averaged approximately 32 percent.

Relocation rates for workers participating in the reemployment programs were very low, although these rates differed considerably among projects. The percent of participants who used government funds to move is shown in Table 2. Mid-Willamette Valley, Yakima, and Downriver had relocation rates of 6, 8, and 11 percent, respectively.[11] Buffalo, Lehigh Valley, and New Jersey all relocated 1 percent or less of their participants.

There are several reasons why relocation rates of workers in the relocation programs were low. First, voluntary mobility plays only a small part in labor market adjustment; therefore, workers may adjust to layoffs by seeking lower paying jobs in their immediate area. Second, the populations targeted do not tend to be highly mobile. Third, because relocation assistance benefits may not adequately offset the costs associated with moving, relocation may not appear as an attractive alternative. To illustrate this point, companies that relocated their employees domestically during 1988 paid an average of $4,835 per move for moving-service packing and transportation,[12] far above the several-hundred-dollar reimbursements or benefits that were offered in the reemployment programs.

Characteristics of Those Using Relocation Assistance

Data on the gender and age of those using relocation assistance are available primarily for the earlier relocation projects. The MDTA-sponsored projects provided assistance primarily to male workers (more than 90 percent of those receiving services were male).[13] Only the New Jersey program reemployment program provided descriptive statistics on the workers actually receiving relocation assistance. All individuals who received relocation assistance under this program were men, despite the fact that the New Jersey program had much higher percentages of eligible women than any of the other reemployment programs. Because recent reemployment projects have been targeted toward assisting dislocated or displaced workers, most recent projects have mostly served male workers.

Reflecting the different objectives between the relocation programs and the reemployment programs, the work experience of the workers served by the different programs varied considerably. The majority of workers in the relocation programs were under 25 years of age.[14] Because those between the ages of 22 and 24 tend to be the most mobile, this result is not surprising. Nearly all workers served in the reemployment programs, however, had substantial work experience. Even though data on the characteristics of those using relocation assistance are sparse, it has been determined that each program's eligibility requirements were such that the eligible populations largely consisted of experienced workers.

Jobs Before and After Relocation

Again, data on the types of jobs workers had before and after relocation are generally unavailable. Half of the workers in the MDTA demonstration projects had been in unskilled positions before becoming unemployed or were new entrants into the labor market; 26 percent had worked in industrial and craft occupations. After relocating, 50 percent of these workers were employed in industrial and craft occupations, whereas 36 percent worked in unskilled jobs.[15] Those using relocation assistance in the New Jersey program tended to be from white-collar occupations with above-average wages and substantial work experience (7 out of the 10 workers were 45 to 54 years old).[16]

Durability of Moves

Several programs contacted relocatees after their moves to follow up on whether they stayed in the area. In the relocation programs, relocated workers were usually contacted either 2 or 4 months after they had moved. Twenty percent of those who had changed jobs had moved back; another 20 percent had switched to new jobs.[17] The reasons workers returned home were often economically motivated. Many felt that housing and the overall cost of living was too high in the new area; therefore, even if they had experienced a wage gain, in relative terms they were worse off than before. Because many of the MDTA relocatees moved from rural areas to urban areas, this accounts for the substantial increases in cost of living experienced. Similarly, 20 percent of movers in the Downriver program moved after being relocated.[18]

Certain characteristics were associated with whether an individual was likely to remain in their new job. Although single individuals were more likely to relocate, they were also more likely to move back. Workers who were over 45 and the long-term unemployed also were likely to move back.

Relocation Costs

Costs associated with relocation vary, depending on the permitted types of reimbursements, the number of out-of-town interviews that workers have, and the distances traveled by workers who relocate. The average cost for relocating workers in the MDTA relocation programs was $867.[19] Relocation costs in the Downriver demonstration averaged $944 per participant.[20] In the Mid-Willamette Valley and Yakima demonstrations, relocation costs were approximately $560.[21]

Compared with the costs associated with other reemployment schemes, such as worker retraining and intensive job search assistance, relocation is not too costly a method for assisting unemployed individuals with finding new jobs. However, because so few workers are interested in relocating, it does not reduce the duration of unemployment for many workers.

Endnotes

1. DeAre (October 1992, p. VIII).

2. DeAre (October 1992, p. XI). Local moves are defined as moves within the same county and long-distance moves as those between counties.

3. DeAre (October 1992, p. XI).

4. Mincer et al. (1985, pp. 1-35).

5. DeAre (October 1992, p. XV).

6. Fairchild (1970, p. 58).

7. Fairchild (1970, pp. 147-148).

8. Wichita (1984, p. 12).

9. Wichita (1984a, p. 9).

10. Corson et al. (1989, p. 39).

11. Kulik et al. (1984, p. 40) and Wichita (1984b, p. 60).

12. Atlas Van Lines (1989, p. 7).

13. Fairchild (1983, p. 111).

14. Fairchild (1970, p. 111).

15. Schnitzer (1970, p. 181).

16. Corson et al. (1989, p. 119).

17. Manheimer et al. (1986, p. 31).

18. Kulik et al. (1984, p. 40).

19. Fairchild (1970, p. 105).

20. Kulik et al. (1984, p. 40).

21. Wright (1984b, p. 34) and Wichita (1984b, p. 60).

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Wage-Rate Subsidies for Dislocated Workers

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An array of innovative policies has been suggested to address more effectively the needs of dislocated workers. In this paper, we model and simulate the impacts of a wage-rate subsidy (or salary supplement) program in which a dislocated worker who becomes reemployed would receive a payment equal to one-half the difference between the wage previously earned and the wage currently earned. The simulations are based on a search model that is institutionally rich and that provides estimates of the impacts of a wage subsidy by incorporating empirical results from the reemployment bonus experiments that were conducted in the mid- to late-1980s. The model includes several groups of workers other than dislocated workers and therefore provides estimates of the degree to which these other workers might be crowded out of jobs by the wage subsidy program.

The results suggest that a wage-rate subsidy paid for two years after reemployment would shorten the unemployment spells of dislocated workers by nearly 2 weeks, and would increase employment of dislocated workers by about 900 to 1000 per 100,000 in the labor force. But the simulations also raise the possibility that the gains for dislocated workers could come at the expense of other groups of workers; that is, other groups of workers could experience small increases in unemployment duration, and decreases in employment levels that almost fully offset the gains for dislocated workers. Three factors may mitigate these crowding-out results -- crowding out is widely dispersed over various groups of non-dislocated workers, the structural changes that result in dislocation of some workers (and drive the need for a policy like a wage subsidy) benefit non-dislocated workers, and the crowding-out results are quite sensitive to one of our assumptions. We also compare the wage-rate subsidy program with a reemployment bonus, and show that the two can be structured so as to give identical results.

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I. Introduction

Since the recession of the mid-1970s, there has been growing concern about dislocated workers and interest in policies that might assist them. Dislocated workers are workers who have lost a job as a result of a plant closing or mass layoff that resulted in turn from some form of economic restructuring, such as technological change, changes in product demand, or changing patterns of international trade. Such workers usually earned high wages and had considerable seniority in their former job. Most had accumulated much firm- and occupation-specific human capital. After dislocation, they face bleak prospects -- low-wage jobs, long spells of unemployment, and difficulty gaining reemployment. As a result of dislocation, they suffer large loses of human capital and lifetime income.[1]

The magnitude of the losses suffered by dislocated workers has been clarified recently by Jacobson, LaLonde, and Sullivan (1993a,b,c), who assembled a very large data base of dislocated workers from administrative wage records. Their findings suggest that the average dislocated worker suffers lifetime earnings losses totalling $80,000. Moreover, they find that nearly three-quarters of the losses occur because earnings after reemployment are permanently lower for these workers.

The losses suffered by dislocated workers pose a major challenge for public policy. Existing policies to assist dislocated workers form a patchwork that does not come close to compensating dislocated workers for their losses (Jacobson, LaLonde, and Sullivan 1993a, chapter 7).[2] Unemployment Insurance (UI) -- the largest and most important program providing

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[1]The literature on dislocated workers has grown dramatically in the past decade. Hamermesh (1989) and Seitchik (1991) discuss the difficulties in defining dislocated workers. Hamermesh (1989) and Jacobson, LaLonde, and Sullivan (1993a, chapter 2) review past evidence on the costs of worker dislocation.

[2]These policies include income replacement from Unemployment Insurance (UI) and (for some) Trade Adjustment Assistance (TAA), job search assistance from the Employment Service, and

assistance to dislocated workers -- has served as an effective "first line of defense" against the hardship suffered by workers experiencing short spells of unemployment or temporary layoff. But UI has been criticized by some for providing benefits that are too stingy to compensate dislocated workers for their losses and too brief to provided dislocated workers the opportunity to complete education and training programs that would prepare them for good jobs (U.S. Department of Labor 1993; Advisory Council on Unemployment Compensation 1994, chapters 1 and 2). Others have noted that UI creates especially strong disincentives for dislocated workers to find reemployment, since they face low-wage jobs but usually receive the maximum UI benefit amount (Jacobson, LaLonde, and Sullivan 1993a, pp. 150-152).

An array of innovative policies has been suggested to address more effectively the needs of dislocated workers. Some have been or are in the process of being implemented -- extended UI benefits are provided to certain workers enrolled in approved training under the Trade Adjustment Assistance program, and intensive job search assistance is being implemented through the UI system in the form of worker profiling. Others are under serious consideration -- self-employment incentives and the reemployment bonus are included in the Clinton Administration's Reemployment Act.

The wage-rate subsidy -- or salary supplement -- for dislocated workers is a promising possibility that has received relatively little attention.[3] In this paper, we model a wage subsidy program in which a dislocated worker who becomes reemployed would receive a payment equal to one-half the difference between the wage previously earned and the wage currently

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subsidized training programs (for example, Economic Dislocation and Worker Adjustment Assistance, or EDWAA).

[3]Greenwood (1994) reports on the design of a wage-rate subsidy experiment that is underway in Canada. Jacobson (1994) examines the impact of the wage-rate subsidy that is part of the Trade Adjustment Assistance program and finds that TAA's wage-rate subsidy was very lightly used.

earned.[4] We model both a wage subsidy that is paid in perpetuity, and one that is limited to the 2 years following reemployment. This policy, which could also be thought of as a salary supplement or earnings insurance, has been suggested and discussed recently by several researchers, including Jacobson, LaLonde, and Sullivan, 1993a, pp. 160-169), Baily, Burtless, and Litan (1993, pp. 194-197), and Parsons (1994).

The wage-rate subsidy we consider has much to commend it. It would induce dislocated workers to search harder for jobs and accept employment that they might otherwise refuse; hence, it would shorten their duration of unemployment and increase their employment. It would redistribute income toward dislocated workers, who have suffered losses through no fault of their own (and possibly through government action such as trade liberalization). Private and social benefits would derive from the policy because output would increase, workers' general skills would be maintained, and new skills would be acquired on-the-job. The costs of a wage subsidy would be (at least partially) offset by reduced spending on public income support and training programs, which would otherwise provide income and services to the subsidized workers. Jacobson, LaLonde, and Sullivan point out that the wage subsidy, by supplementing earnings after reemployment, attacks the source of nearly three-quarters of the wage losses suffered by dislocated workers. They, and Baily, Burtless and Litan, stress the reemployment incentive effects of the wage subsidy and its potential to reduce resistance to structural change, such as trade liberalization.[5]

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[4]Jacobson, LaLonde, and Sullivan (1993) specify a somewhat more complicated subsidy formula. Baily, Burtless, and Litan (1993) would limit the subsidy payments to the two years following dislocation, and would link the size of the subsidy to a worker's age and previous job tenure.

[5]An alternative to the wage-rate subsidy is the wage-bill subsidy, which would pay a subsidy to an employer who hires a dislocated worker. Existing evidence on wage-bill subsidies suggests that they suffer from very low participation rates (see, for example, the evidence on the Employer Bonus in Woodbury and Spiegelman 1987).

But the benefits of a wage subsidy would not come without a potential cost. The direct effect of the wage subsidy is to increase the search intensity of dislocated workers and thereby increase their employment. But the increased search intensity of dislocated workers also has an indirect crowding-out effect -- if dislocated workers search harder for jobs, then dislocated workers may beat other (non-dislocated) workers to job vacancies. As a result, job vacancies that would normally be available to other workers are filled by dislocated workers, and the other workers don't get jobs that they would otherwise obtain. This crowding-out effect of a wage subsidy to dislocated workers, if large, could be an important drawback of the policy.[6]

Little is known about the potential effectiveness of a wage subsidy that is offered to dislocated workers.[7] In this paper, we model and simulate both the direct and indirect impacts of such a program. In the next section, we develop a search model that is institutionally rich and that provides estimates of the impacts of a wage subsidy by incorporating empirical results from the reemployment bonus experiments that were conducted in the mid- to late-1980s. The model includes several groups of workers other than dislocated workers and therefore provides estimates of the degree to which these other workers might be crowded out by the wage subsidy program. The results, which are presented in section III, suggest that the wage subsidy program would indeed provide gains to dislocated workers, but also raise the possibility that these gains would come at the expense of other groups of workers.

[6]Effects of a similar nature have been considered by Levine (1993), who examined the spill-over of UI on UI-ineligibles. Our earlier work considered the crowding-out effects of a reemployment bonus on workers not offered a bonus (Davidson and Woodbury 1993).

[7]Jacobson (1994) appears to be the sole exception. In contrast, there is much evidence on the effectiveness of the wage subsidy as an anti-poverty program targeted on disadvantaged workers. See, among others, Bishop (1977), Hurd and Pencavel (1981), Betson and Bishop (1982), Lerman (1982), and Haveman (1988, pp. pp. 165-168).

Section III also compares the wage subsidy program with a reemployment bonus, and shows that the two can be structured so as to give identical results. Although a reemployment bonus would yield a given impact at lower cost than a wage subsidy, bonuses have the disadvantage that worker must act within 6 to 12 weeks of claiming UI benefits.

II. The Model

To investigate the impact of wage-rate subsidies on the reemployment of dislocated workers, we use a partial equilibrium search model in the spirit of work by Diamond (1982), Mortensen (1982) and Pissarides (1990). The model can be thought of as one in which workers flow through various labor market states, with rates of transition between states depending in part on the search behavior of workers. We assume that unemployed workers search randomly across firms for a job vacancy, and that firms with vacancies randomly select workers from the pool of applications they receive. Each unemployed worker chooses search intensity -- the number of firms contacted -- in an effort to maximize expected lifetime utility. Increasing search intensity raises the probability of reemployment but is also costly. A steady-state equilibrium is generated in such a model by equating the flows into and out of each labor market state.

Worker dislocation is considered in the model by assuming that there are two employment sectors -- high-wage and low-wage -- and that the economy experiences a one-time shock that causes part of the high-wage sector to shut down. Dislocated workers in our model are former employees of the high-wage sector who must now search for a low-wage job. In contrast, high-wage workers who experience a regular layoff search for (and eventually find) a high-wage job.

A. The Unemployed

Since we are interested in the crowding-out effects of the wage subsidy program, it is necessary to model the behavior of all unemployed workers, not just the dislocated workers directly affected by the program. Figure 1 summarizes the categories of unemployed workers we examine in the model. We begin by dividing the unemployed into two classes -- unemployed workers who are eligible for UI benefits and those who are not. We refer to workers in the latter class as "UI-ineligibles" and use U_i to denote the total number of such workers in steady-state equilibrium. This class consists mainly of new entrants and reentrants into the labor force, as well as workers with a weak attachment to the labor force, and typically accounts for approximately 60% of unemployed workers (Blank and Card 1991, table 1). We denote by q the fraction of unemployed workers who are UI-ineligibles.

Next, we divide the class of UI-eligible workers into two sub-classes -- those who claim UI and those who do not. We refer to workers in the latter subclass as "UI-eligible non-claimants" and use U_k to denote the total number of such workers in the steady-state equilibrium. Why these workers fail to claim benefits has concerned policy-makers and puzzled researchers (Burtless and Saks 1984, Corson and Nicholson 1988, Vroman 1991, Blank and Card 1991, Anderson and Meyer 1993). Among the various explanations for the failure of these workers to collect benefits for which they are eligible, the most likely is that they expect to find reemployment fairly rapidly. Hence, the costs of filing for and obtaining UI exceed the expected benefits. We use k to denote the UI take-up rate -- that is, the fraction of unemployed UI-eligible workers who choose to claim their benefits. Based on the work of Blank and Card (1991), we set k = .75.[8]

[8]Blank and Card report a range of roughly .65 to .75 for the UI take-up rate. The results reported below are essentially invariant to changes in k in the range of .65 to .75.

Finally, we divide the sub-class of UI-eligible claimants into three categories -- high-wage, low-wage, and dislocated workers. The total number of high-wage UI-eligible claimants in the t^{th} period of search is represented by $U_{h,t}$ while $U_{\ell,t}$ and $U_{d,t}$ play the same roles for low-wage and dislocated workers, respectively. In line with the discussion of dislocated workers in the introduction, we define dislocated workers as workers who earned a wage premium in their former job, but who can gain reemployment only in a low-wage job.[9] The wage premiums earned before dislocation could result from collective bargaining agreements, firm-specific human capital, a good job match, and/or efficiency wage considerations. We do not model the source of the premium, but rather take it as given.

For our purposes, then, a dislocated worker is a victim of a shrinking high-wage sector -- that is, a worker who earned a high wage in his previous job and, after separation, has no alternative but (eventually) to accept a low-wage job.[10] In our model, the only difference between low-wage and dislocated workers (once they are unemployed) is that the dislocated workers receive wage subsidies from the government after they are reemployed, while low-wage workers do not. We use h to denote the fraction of UI-eligible claimants who earn high wages, and d denotes the fraction of low wage workers who have been dislocated from the high-wage sector.

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[9]In the model (as in fact), only a fraction of high-wage workers who become unemployed are dislocated -- namely, those whose unemployment stems from a plant closing or similar restructuring. Most high-wage workers experience short spells of unemployment and are recalled or find reemployment at a high wage.

[10]In fact, dropping out of the labor force is another option for dislocated workers. However, the model does not explicitly treat flows into and out of the labor force, which implies an assumption that those flows are constant and that the stock of individuals not in the labor force is in steady state.

B. The Equations

Figure 2 depicts the model we use, which is based on a model we developed in earlier work (Davidson and Woodbury 1993). The model characterizes flows through the labor market by specifying stocks of workers in various states of employment and unemployment, and then quantifying the transition rates between those states. We measure time in two-week intervals (since UI claimants are typically certified for 2 weeks of benefits at a time) and assume that all UI-eligible claimants exhaust their benefits after 27 weeks (i.e., 14 periods) of insured unemployment.[11] Essentially, the model follows workers as they flow through the possible states of employment and unemployment, and uses steady-state conditions to characterize an equilibrium.

There are two differences between the model used in our earlier work and the one used here. First, in the earlier model, the UI take-up rate was assumed to equal 100%. In this model, we relax that assumption and allow the UI take-up rate to be less than 100% (i.e., $k < 1$). Second, in the earlier model, workers and jobs were homogeneous. In this, model, we allow for heterogeneity of jobs and workers by dividing UI-eligible claimants into high-wage, low-wage, and dislocated workers.

The model consists of five sets of equations. The first set consists of three accounting identities. We let T denote the total number of jobs available, use J to represent the total number of jobs that are filled, and use V to represent the number of job vacancies in the steady-state equilibrium. Since all jobs are either filled or vacant, the first identity is $T = V + J$.

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[11]That is, we assume that UI-eligible claimants can receive 26 weeks of benefits after a waiting week. We also assume that all UI recipients experience a single spell of unemployment during their benefit year, and that they do not accept part-time employment that would result in partial benefit payments. These assumptions fit the majority of UI recipients.

The second identity states that all workers must be either employed or unemployed. If we let L denote the total number of workers in the labor force and use U to represent total unemployment, then our second identity is $L = J + U$.

The third identity sums unemployed workers over the five categories shown in Figure 1 and over the time periods in which they are unemployed. If we use $U_{h,e}$ to denote the number of high-wage UI-eligible claimants who have exhausted their UI benefits (that is, have been unemployed for more than 14 periods) and define $U_{\ell,e}$ and $U_{d,e}$ analogously for low-wage and dislocated workers, then this identity can be written as $U = U_i + U_k + \sum_{t=1,14} (U_{h,t} + U_{\ell,t} + U_{d,t}) + U_{h,e} + U_{\ell,e} + U_{d,e}$.

The second set of equations, the <u>steady-state conditions</u>, equate the flows into and out of each state of employment or unemployment. If these equations are satisfied, then total unemployment and its composition remain constant over time. Consider, for example, the flow of workers out of employment and into UI-ineligible unemployment. We use s to denote the separation rate, or fraction of jobs that turn-over in each period. Thus, sJ workers lose their job in a given period. If we let q denote the fraction of unemployed workers who are UI-ineligible (as noted above, we set $q = .6$), then qsJ UI-ineligible workers lose their jobs.[12] It follows that the flow <u>into</u> state U_i is qsJ. To calculate the flow out of this state, let m_i denote the reemployment probability (or job match probability) for any UI-ineligible worker. Then m_iU_i unemployed UI-ineligible workers find jobs in any given period, and this represents the flow <u>out of</u> state U_i. These flows are shown in the northwest quadrant of Figure 2. In a steady-state equilibrium, U_i must remain constant over time. Therefore, we must have $qsJ = m_iU_i$. There is an analogous steady-state equation for each possible state of unemployment. Figure 2

[12]The remaining (1-q)sJ newly unemployed workers are UI-eligible.

shows the flows into and out of each state and, for completeness, all of the steady-state conditions are written out in the Appendix.

We refer to the third set of equations as the reemployment probability equations. They define the probability of reemployment for any given unemployed worker as a function of the search effort of all workers and the number of vacancies.[13] Let $p_{j,t}$ denote the search effort of an unemployed type j UI-eligible claimant in the t^{th} period of search, where the subscript j can take one of three values -- h for high-wage workers, ℓ for low-wage workers, or d for dislocated workers. The terms p_i and p_k refer to the search effort of unemployed UI-ineligible and UI-eligible non-claimants, respectively. Search effort corresponds to the probability of contacting a firm (alternatively, the number of firms contacted) in any given period by the worker seeking employment. Assuming that workers apply to firms at random, the probability that any given firm has a vacancy is V/T. If we let λ denote the average number of job applications received by each firm, then the probability that a worker gets a job conditional on applying at a firm with a vacancy is $(1-e^{-\lambda})/\lambda$ (see Davidson and Woodbury 1993). Thus, the probability that an unemployed type j UI-eligible claimant in the t^{th} period of search finds a job is given by $m_{j,t}=p_{j,t}(V/T)[(1-e^{-\lambda})/\lambda]$. As shown in equations (11)-(14) in the Appendix, there is a reemployment probability equation for each state of unemployment (see also Figure 2).

As noted above, the probability of reemployment increases with search effort. But increasing search effort is costly. We assume that the cost of search effort is given by cp^z, with $z>1$ denoting the elasticity of search costs with respect to search effort. We assume that c differs between UI-eligible and UI-ineligible workers, but that z is the same for all workers.

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[13]Note that in the reemployment probability equations in the Appendix [(11)-(14)], each group's own search intensity (p_j) enters directly, and the search effort of all other groups of workers enters indirectly through λ.

Our fourth and fifth sets of equations are used to calculate the optimal search effort of unemployed workers. In the fourth set, we calculate the expected lifetime income of workers in each possible state of unemployment and employment. Then, in the fifth set, we calculate the level of search effort that maximizes these expected lifetime incomes.

Expected lifetime income is calculated by considering both the current and future prospects faced by the worker. For example, let $V_{j,t}$ denote the expected lifetime income of an unemployed type j UI-eligible worker in the t^{th} period of search, $V_{j,w}$ the expected lifetime income for an employed type j UI-eligible worker, w_j the type j wage, and x biweekly UI benefits. (As above, the subscript j can take on one of three values -- h for high-wage workers, ℓ for low wage workers, or d for dislocated workers). Then, an unemployed type j UI-eligible claimant in the t^{th} period of search receives current net income equal to UI benefits less the cost of search, or $x-c(p_{j,t})^z$. With probability $m_{j,t}$ this worker finds a job yielding net future income of $V_{j,w}$. With the remaining probability, $1-m_{j,t}$, the worker remains unemployed and can expect net future income of $V_{j,t+1}$. Therefore,

$$V_{j,t} = x - c(p_{j,t})^z + [m_{j,t}V_{j,w} + (1 - m_{j,t})V_{j,t+1}]/(1 + r).$$

Note that future income is discounted, with r denoting the interest rate. In the Appendix, equations (16)-(19) state the conditions describing the expected lifetime income for workers in each state of unemployment.

To calculate $V_{j,w}$, the expected lifetime income for an <u>employed</u> type j UI-eligible claimant, we follow the same procedure. Current income is equal to the worker's wage, w_j. In addition, with probability (1-s) this worker keeps his job for another period and continues to earn $V_{j,w}$. With probability s the worker loses his job and has to search for new employment, resulting in a future income of $V_{j,1}$. Therefore,

$$V_{j,w} = w_j + [sV_{j,1} + (1-s)V_{j,w}]/(1 + r).$$

Again, the Appendix shows the conditions describing expected lifetime income for workers in each state of employment -- see equations (20)-(21).

Finally, for each unemployed worker, search effort is chosen to maximize expected lifetime income. Therefore, there is an equation defining optimal search effort for each possible state of unemployment -- see equations (22)-(24) in the Appendix -- with one exception. The exception is made for UI-eligible non-claimants. Presumably, these workers do not claim UI benefits because they do not expect to be unemployed for a significant length of time -- that is, they expect to be able to find jobs with relatively little effort. Therefore, we treat these workers differently by assigning them a high reemployment probability and ignoring their search decision. Provided that their reemployment probability is set high enough (so that their expected duration of unemployment is roughly half the expected duration of high-wage UI-eligible claimants), our results are not sensitive to this treatment.

To investigate the impact of wage subsidies paid to dislocated workers, we solve the model first assuming that there are no wage subsidies. In the absence of wage subsidies, low-wage workers and dislocated workers face the same wage: that is, $w_\ell = w_d$. We then introduce a wage subsidy paid to dislocated workers that equals half the difference between the wage earned before dislocation (w_h) and the market opportunity wage now facing the worker (w_ℓ). This implies a subsidy paid to dislocated worker of $(w_h - w_\ell)/2$, so that, w_d increases to $(w_h + w_\ell)/2$. With this change, we resolve the model, and compare the results.

Intuitively, the wage subsidy increases the opportunity cost of unemployment for dislocated workers, resulting in an increase in search effort by these workers. The increased search effort lowers their duration of unemployment, increases their steady-state employment level, and may decrease the employment of other workers. By solving the model for different wage subsidy programs, we can gauge the magnitude of these different impacts. However, to do so, we must first set the values of the parameters of the model.

C. The Parameters

The key endogenous variables in the model are employment (J), the number of unemployed workers in the different states of unemployment (the U measures), the reemployment (or job match) probabilities for unemployed workers in different states of unemployment (the m terms), and search effort (or employer contact probability) for unemployed workers in different states of unemployment (the p terms). The key parameters are the fraction of unemployed workers who are ineligible for UI benefits (q), the UI take-up rate (k), the job separation or turnover rate (s), the interest rate (r), total available jobs (T), the size of the labor force (or the total number of workers, L), biweekly UI benefits (x), the biweekly wages (w_h, w_ℓ, w_d and w_i), the search-cost parameters (c, c_i, and z), the fraction of UI-eligible claimants who earn high wages (h), and the fraction of low-wage UI-eligible claimants who are dislocated workers (d).

In specifying the parameters, we follow the approach adopted in our earlier work (Davidson and Woodbury 1993). It is useful to specify a set of parameters that can be taken as a reference case, although it is important to test the sensitivity of our results to variation in the parameters, since the existing research suggests a range of values for each of the parameters. We begin by obtaining values of parameters that are available in existing research. For example, as noted above, we set q, the proportion of the unemployed who are UI-ineligible, equal to .6, and k, the proportion of UI-eligibles who claim their benefits, equal to .75.

For s, the separation rate, we turn to research by Ehrenberg (1980), Clark and Summers (1982), and Murphy and Topel (1987). Their results suggest a biweekly value for s that falls somewhere in the range of .006-.014 (the mean appears to be .01 with a standard error of about .004). For r, the interest rate, we consider biweekly values in the range .002-.02, which translates into annual discount rates that range in value from 5% to 67%.

Consider next T and L. We first note that the model is homogeneous of degree zero in T and L so that we may set L=100 without loss of generality. We then note that as T varies with L held fixed, the model generates different values for U and V. Research by Abraham (1983) suggests that U tends to be close to 2V, although it varies over the business cycle. Although the actual value depends on the other parameters, our model predicts that U=2V when T is approximately 96.25 and that for values of T ranging from 95 to 97.5, U ranges from 1.5V to 3V.

The considerations to this point suggest specifying a reference case in which s=.01, r=.008, and T=96.25. As we show below, the results are remarkably insensitive to variation in s, r, and T within the ranges described above.

For the remainder of the observable parameters, we turn to data collected to analyze the Illinois Reemployment Bonus Experiment.[14] In the Illinois experiment, the average biweekly UI-benefit was $245. We set the biweekly wage earned by "high-wage" workers equal to $846, and the biweekly wage earned by "low-wage" workers equal to $538. Therefore, $x = \$245$, $w_h = \$846$, and $w_\ell = \$538$.[15]

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[14]In the Illinois experiment, a randomly assigned group of new UI claimants were offered a $500 cash bonus if they found a new job within 11 weeks and held the job for 4 months. Their behavior was compared with that of a randomly assigned control group. The design and results of the experiment are described in Woodbury and Spiegelman (1987).

[15]It is impossible to distinguish dislocated workers from other workers in the Illinois experimental data, so we turn to the Washington Reemployment Bonus experiment (Spiegelman, O'Leary, and

This leaves the unobservable parameters associated with the cost function (c, c_i, and z), h, and d. For c, c_i, and z we use the approach taken in our earlier work (Davidson and Woodbury 1993), with some modifications. In the earlier work, we found values of c and z that made the model's predictions match the results observed in the Illinois experiment.[16] Specifically, we found values of the search cost parameters such that (a) the duration of unemployment predicted by the model in the absence of a reemployment bonus matched the observed duration of unemployment in the Illinois control group, and (b) the change in unemployment duration due to the bonus predicted by the model matched the actual change observed in the Illinois experiment. To find c_i, we used estimates by Katz and Meyer (1990) and Woodbury (1991) of the increase in the expected duration of unemployment brought about by a 1-week extension of UI benefits.[17] Using these estimates and the expected duration of

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Kline 1992), in which data on worker dislocation were gathered. In the Washington experiment, 15% of enrollees were dislocated by the standard BLS criteria (that is, employed by the same employer for at least 3 years prior to job loss), and the base period earnings of these dislocated workers were 57% higher than the base period earnings of other workers. Hence, $w_h = 1.57w_\ell$. In the Illinois experiment, the average base period earnings of all workers in the control group was \$584. This allows us to write $\$584 = .15(1.57w_\ell) + .85(w_\ell)$. Solving yields $w_\ell = \$538$ and $w_h = \$846$.

[16]The \$500 Illinois bonus reduced the duration of insured unemployment by .714 week in the case of workers who were eligible for 26 weeks of state regular benefits. The bonus impact in the case of workers eligible for an additional 12 weeks of Federal Supplemental Compensation (FSC) appears to have been much greater. The impact of .714 week is smaller than that reported by Woodbury and Spiegelman (1987), which is 1.13 weeks for state-regular eligibles and FSC-eligibles combined. The .714-week estimate, however, is appropriate to our model (which allows for 26 weeks of UI benefits), and is similar to estimates of bonus impacts obtained in similar trials in Pennsylvania and Washington State (Decker and O'Leary 1992). Evidence on impacts of the Illinois bonus under different potential benefit durations is developed elsewhere (Davidson and Woodbury 1991).

Note that we assume the impact of a bonus on dislocated workers is the same as on workers generally. This accords with the evidence obtained in the Washington Reemployment Bonus Experiment, where dislocated workers' response to the bonus offer did not differ from the response of other workers. See Spiegelman, O'Leary, and Kline (1991).

[17]Katz and Meyer estimate that eligibility for 1 additional week of UI benefits increases the expected duration of unemployment by .16 to .2 week; Woodbury estimates a larger increase -- .4 week. Our results turn out to be insensitive to which estimate we use.

unemployment for the control group in Illinois, we could infer the expected duration of unemployment for UI-ineligibles and then choose c_i such that the model's prediction matched that inferred value.

As noted above, the model used here differs from the model in our earlier work in two respects -- here we assume that the UI-take up rate is less than 100% and that there are high- and low-wage workers. We therefore extended the previous model and followed the same approach to obtain estimates of the search cost parameters. For the reference case, we find that $z = 1.381$, $c = 157.8$, and $c_i = 102.8$.

The last two parameters are h, the proportion of UI-eligible claimants who are high-wage workers, and d, the proportion of low-wage UI-eligible claimants who are dislocated workers. In treating these parameters, we follow two approaches, each reflecting an extreme assumption. At one extreme, we assume that all unemployed workers (both high- and low-wage) compete for the same jobs so that, effectively, there is a single labor market. In other words, workers are heterogeneous but jobs are homogeneous.[18]

In the case of a single labor market, once we know h we can infer d. To see how, let U_{HO} denote the total number of high-wage UI-eligible claimants <u>before</u> the high-wage sector shrinks (leading to worker dislocation), and let U_H denote the total number of high-wage UI-eligible claimants after the dislocated workers lose their jobs. Dislocated workers are defined to be workers who previously earned high wages and then, after losing their job, can only find reemployment at the low wage. Suppose that 15% of the UI-eligible claimants who initially

[18]This appears to violate the law of one price -- high-wage workers get a high wage even though jobs are all identical. As mentioned earlier, the high wages paid to high-wage workers may stem from any of several non-competitive forces such as collective bargaining, firm-specific human capital, or efficiency wage payments.

earned high wages fit this profile (that is, $h=.15$).[19] Then, the number of dislocated workers will be $.15U_{HO}$ and the number of high-wage UI-eligible claimants ex-post will be $U_H = .85U_{HO}$. Finally, since d is defined as the fraction of low-wage UI-eligibles who are dislocated workers, we have $d = .15U_{HO}/U_L$, where U_L denotes the total number of low-wage UI-eligible claimants after the dislocated workers have lost their jobs. Substituting from above for U_{HO} yields $d = (.15/.85)U_H/U_L$. This expression can be simplified further by using the fact that $U_H/U_L = h/(1-h)$. Substitution then yields $d = 3h[17(1-h)]$. For each wage subsidy program that we consider, we report results for values of h ranging from .5 to .1 (and, therefore, d ranges from .176 to .002). As we show below, our results are similar for all such values.

The other extreme is to assume that high- and low-wage workers compete in different sectors of the labor market. That is, we assume the existence of a dual labor market in which there are two kinds of workers and two kinds of jobs. If high-wage unemployed workers compete only for high-wage jobs and low-wage unemployed workers compete only for low-wage jobs, then crowding-out will be confined to the low-wage sector -- the dislocated workers (formerly high-wage workers now forced to look for low-wage jobs) are offered a wage subsidy and search in the low-wage sector. The notion of a dual labor market can be captured by setting $h=0$, which essentially splits off the high-wage sector (the northeast quadrant of Figure 2) and implies that dislocated and low-wage workers compete only for low-wage jobs.

[19]The 15% figure is probably an upper bound on the percentage of previously high-wage workers who become dislocated. Data on worker dislocation that were gathered in evaluating the Washington Reemployment Bonus Experiment suggest that, using the BLS definition of job loss after 3 or more years working for the same employer, 15% of new UI recipients were dislocated. Washington State's criteria, which include considerations such as the industry in which a worker was employed and whether the UI claim resulted from a plant closing, suggest a figure closer to 5%. Using the Displaced Worker Surveys over the period 1979-1986, Seitchik (1991) finds that about 10% of all unemployed workers were dislocated (that is, lost their job due to plant closing, slack work, or job abolition).

In modelling the dual labor market, we also need to make an assumption about the sector in which UI-eligible non-claimants seek jobs. One extreme possibility is that they compete for jobs only in the high-wage sector, which we can model by setting $k=1$. Setting $k=1$ implies that all low-wage workers who are eligible for UI benefits claim those benefits; there are no UI-eligible non-claimants in the low-wage sector, and any UI-eligible non-claimants in the economy are seeking high-wage jobs. The alternative possibility is that UI-eligible non-claimants compete for jobs in the low-wage sector, which we can model by setting $k=.75$ as before.

To examine the impact of wage subsidies in the dual labor market model, we allow d to vary between .15 and .05. These are (approximately) the values of d that correspond with values of h in the range of .5 to .1 (see the right-most columns of Table 1A). In all cases, we compare the steady-state equilibria with and without a wage subsidy in order to gauge the impact of the wage subsidy.

By considering these two extreme cases -- a single labor market and a dual labor market -- we should obtain upper and lower bounds on the impacts of wage subsidies. Cases in which high- and low-wage workers compete for some, but not all, of the same jobs should fall between our two extreme cases. In addition, cases in which UI-eligible non-claimants compete for jobs in both the high- and low-wage sectors should fall between the two sets of estimates we obtain using the alternative dual labor market models.

D. Summary of the Model

The basic set up of the model can be visualized by referring to Figures 1 and 2. Figure 1 shows the groups of workers we consider -- high-wage UI-eligible claimants, low-wage UI-eligible claimants, dislocated workers, UI-eligible non-claimants, and UI-ineligibles. Several key parameters are defined in Figure 1: k is the UI take-up rate (set at .75); h is the proportion of UI-eligibles who are high-wage workers (which we allow to vary between .1 and .5); d is the proportion of low-wage UI-eligibles who are dislocated (which we allow to vary between .02 and .176); and q is the proportion of all unemployed workers who are UI-ineligible (set at .6). Much of the model's complexity stems from the number of sub-groups of workers we consider and from the number of states of unemployment through which each of these groups can flow. It is important to consider these various groups of workers so that crowding-out can be gauged, and equally important to consider multiple states of unemployment, since incentives facing a worker can change during a jobless spell.

Figure 2 shows the various labor market states and the flows through the labor market that are specified by the model. The flows from state to state are quantified by transition rates, which depend on reemployment probabilities (or match probabilities, m). These reemployment probabilities depend in turn on search behavior and the search technology, and are the outcome of an optimizing choice. Three sets of equations -- for reemployment probability, expected lifetime income, and optimal search effort -- specify this optimization. Steady-state equilibrium in the model is obtained by equating the flows into and out of each labor market state. The complete structure of the model is set out in the Appendix.

We model worker dislocation by assuming that there are two employment sectors -- high-wage and low-wage -- and that the economy experiences a one-time shock that shuts down part of the high-wage sector. Dislocated workers are formerly high-wage workers who must now search for a low-wage job.

In the model, the total number of jobs available (T) is fixed, but employment (J, or the number of jobs that are filled) varies with job turnover (the separation rate, s) and the effectiveness of job search and matching (m). For example, if the rate at which workers separate from jobs (s) increases, then fewer jobs will be filled and unemployment will rise. If the search intensity of unemployed workers (p, the probability that a workers contacts a firm) is high, then more jobs will be filled and unemployment will be lower.

III. Results

We consider the impacts of two different wage-rate subsidy programs. In each, the government pays a subsidy to each dislocated worker who gains reemployment equal to half the difference between the high wage received before dislocation (w_h) and the low wage after reemployment (w_l). As a result, the net wage (including the subsidy) received by a dislocated worker who finds reemployment is $w_d = (w_h + w_l)/2$. In the first program, which we call the "temporary" program, the worker receives the subsidy for two years after gaining reemployment. In the second program -- a "permanent" program -- the worker receives the wage subsidy for as long as employment continues.

In both programs, the wage subsidy increases the opportunity cost of unemployment for dislocated workers and results in increased search effort on their part. For example, in the reference case of the permanent program when half of all UI-eligible claimants are high-wage workers (that is, $h=.5$), search effort increases by approximately 30% for all dislocated workers. This increase in search effort of dislocated workers has the following implications:

- o *There is an increase in overall steady-state employment. That is, more of the total available jobs are filled as dislocated workers are induced to search harder for and accept jobs that would otherwise have remained vacant. This increase in total employment is small, since the wage-rate subsidy is offered to a small portion of the labor force (dislocated workers).*

- *Reemployment probabilities and employment levels rise for dislocated workers and fall for all other workers, who are beaten to vacancies and crowded out of the labor market by the more aggressive dislocated workers. (The larger the increase in overall steady-state employment, the less crowding out occurs.)*
- *As the reemployment probabilities of non-dislocated workers change, their optimal search effort changes. That is, as it becomes more difficult for the non-dislocated workers to find jobs, their search effort adjusts.*

These three impacts of the wage subsidy can be thought of respectively as a gross employment effect, a crowding-out effect, and a rivalry effect. Note that the gross employment effect is an increase in total employment that is driven by the increase in search effort of dislocated workers. The increase in employment of dislocated workers is offset at least partially by decreases in employment of some other groups of workers. This offset is the crowding-out effect. The rivalry effect is the most subtle of the three effects ---it implies that the increased search intensity of dislocated workers, who are now offered a wage subsidy, is taken into account by other workers when they choose their optimal search intensity. Since dislocated workers make up only a small fraction of the total labor force (and of job seekers), the rivalry effect turns out to be extremely small. For example, in the reference case of the permanent program with h=.5, no non-dislocated worker alters search effort by more than .5% as a result of the wage subsidy.[20] Accordingly, we focus on the gross employment and crowding-out effects of the wage subsidy from here on.

[20]It is not surprising that the rivalry effect is so small in the case of a wage subsidy to dislocated workers. In our work on the displacement (or crowding-out) effects of a reemployment bonus, we also found small rivalry effects, and the program we were modelling was offered to a far larger proportion of unemployed workers (Davidson and Woodbury 1993).

A. Impacts on Dislocated Workers

Tables 1A and 1B show the simulated impacts of the temporary wage subsidy -- that is, one that is paid during the first 2 years after reemployment -- in the reference case as h (the fraction of UI-eligibles who are high-wage workers) and d (the fraction of low-wage UI-eligibles who are dislocated) vary. The results suggest that a wage subsidy lasting 2 years would reduce a dislocated worker's expected duration of unemployment by nearly 2 weeks, and would increase employment of dislocated workers by about 900 to 1,000 per 100,000 labor force participants (see the "Dislocated workers" columns in Tables 1A and 1B).

As the tables indicate, the simulated impacts of the temporary wage subsidy are robust to changes in h and d. Also, there is little difference between the impacts simulated using a single labor market model and those simulated using a dual labor market model.

Whether these impacts on expected duration of unemployment and employment are large or small is a question that can only be answered in a relative sense. In section III.D below, we compare the impacts of the wage subsidy with impacts of a $500 reemployment bonus offered to workers who gain reemployment within 12 weeks. The results suggest that, compared with such a reemployment bonus, the temporary wage subsidy would have roughly twice the impact on the duration of unemployment and level of employment of dislocated workers. We discuss the significance of these relative impacts further in section III.D.

Tables 2A and 2B show the impacts of a wage subsidy that is paid in perpetuity to a dislocated worker who gains reemployment. The results suggest that a permanent program would reduce a dislocated worker's expected duration of unemployment by nearly 5 weeks, and would increase employment of dislocated workers by about 2,200 to 2,400 per 100,000 in the labor force. These impacts are roughly two and a half times the impacts estimated for the temporary wage subsidy. In results not reported in the tables, we have examined the sensitivity of the difference between the temporary and permanent programs to different discount rates (r). Not surprisingly, we find that the differences between the temporary and permanent programs decrease at higher discount rates, but only slightly.[21] That is, when distant wage subsidy payments are discounted more heavily, the impact of the permanent program is slightly closer to the impact of the temporary program. It is unclear whether the discount rate we are using in the reference case -- .008, or about 20% annually -- should be considered particularly high. Given the nature of social programs, participants could well discount future benefits promised by a permanent wage subsidy program at even higher rates.

Tables 2A and 2B indicate that, as with the temporary wage subsidy, the simulated impacts of the permanent wage subsidy are robust to changes in h and d. Also, it makes little difference whether we assume that there is a single labor market or a dual labor market model.

B. Crowding-Out Effects

Although the wage subsidy considered here is provided only to dislocated workers, it has the potential to affect the unemployment duration and employment prospects of other workers. The reason is that if the wage subsidy does increase the search intensity of dislocated workers, then job vacancies will be filled more quickly than otherwise by dislocated workers, and vacancies that otherwise would have been available to non-dislocated workers will vanish. In effect, the wage subsidy will motivate dislocated workers to beat other workers to the vacancies, lengthening the unemployment duration and reducing employment of other workers. If the improved well-being of dislocated workers did come at the expense of other

[21]This is true when we apply the values of the search cost parameters (c, c_i, and z) used in the reference case to models that use different interest rates.

workers, then the wage subsidy would be a far less attractive policy than if such costs were not imposed on other workers.

Tables 1A and 1B show impacts of the wage subsidy on the other groups of workers considered in the model -- high-wage UI-eligible claimants, low-wage UI-eligible claimants, UI-eligible non-claimants, and UI-ineligibles. The extent to which these groups of workers are crowded out by the wage subsidy to dislocated workers can be considered in two ways -- first by looking at impacts on the duration of unemployment, and second by looking at impacts on steady-state employment.

By the first criterion -- impact on unemployment duration -- the model suggests that the crowding-out effects of the wage subsidy to dislocated workers are rather small. The results shown in Table 1A suggest that the impacts on unemployment duration are largest for low-wage UI-eligible claimants, but even these low-wage UI-eligibles would suffer at worst an additional half day (.0806 to .0965 week) of unemployment per spell as a result of the wage subsidy to dislocated workers. In other words, any crowding-out effects of the wage subsidy are dispersed so that they do not fall heavily on any particular group of workers.

But by the second criterion -- impact on employment of workers other than dislocated workers -- the simulations suggest that crowding out is virtually complete (see Table 1B). That is, nearly all of the employment gains experienced by dislocated workers come at the expense of non-dislocated workers. For example, in the reference case for the single labor market and $h=.5$, the employment of dislocated workers rises by 989 per 100,000 workers in the labor force, but employment of other workers combined <u>falls</u> by 982 per 100,000. Similar results hold for the other cases displayed in Table 1B.

Crowding out is nearly complete in this model because the employment gains that result from offering a wage subsidy to a small percentage of unemployed workers (in this case, dislocated workers) are correspondingly small. Employment gains occur in this model through increases in the search intensity of workers who are offered an inducement (such as a wage subsidy) to search harder. When search intensity increases, vacancies disappear and more of the total available jobs in the economy are filled. If only a few workers are offered such an inducement, employment rises only modestly. Recall that the wage subsidy can increase employment even though the total number of available jobs (T) is fixed in the model. Since T=V+J (total available jobs equal the sum of vacancies and jobs that are filled), inducements to search harder cause V to fall and J to rise.

To illustrate the dependence of the crowding out results on the fact that the wage subsidy is offered only to a few workers, we have run a simulation in which a temporary (two-year) wage subsidy is offered to all low-wage UI-eligible claimants, rather than just to dislocated workers. We find that employment of low-wage UI-eligible claimants increases by 868 per 100,000 in the labor force, and that the total decrease in employment of other groups (high-wage UI eligible claimants, UI-eligible non-claimants, and UI-ineligibles) is only 314. In other words, when the wage subsidy is offered to 12.4% of the unemployed (all low-wage UI-eligible claimants), only .36 job is crowded out by each job gained. When the wage subsidy is offered to just 2.6% of the unemployed (dislocated workers only), nearly 1 job is crowded out by each job gained.

Although nearly all the gains accruing to dislocated come at the expense of other workers, it is nevertheless clear that no particular group of workers is especially burdened by the crowding-out effects of the wage subsidy. That is, as was clear in Table 1A, the crowding-out effects of the wage subsidy are dispersed widely over the various groups of non-dislocated workers.

Tables 2A and 2B show the crowding-out effects of a permanent wage subsidy. Table 2A suggests that the permanent wage subsidy would increase the unemployment duration of non-dislocated workers by more than twice as much as the temporary wage subsidy. For example, the most affected group -- low-wage UI-eligible claimants -- would experience up to an additional day or so (.2024 to .2431 week) of unemployment per spell as a result of the permanent wage subsidy. Again, however, the crowding-out effects of the permanent wage subsidy are dispersed across the various groups of non-dislocated workers. This cost of the permanent wage subsidy is offset by a proportionate benefit to dislocated workers.[22]

To summarize, the simulations suggest that the crowding-out effects of the wage subsidy are virtually complete for all cases we consider. In our reference case, 99% of the increased employment of dislocated workers comes at the expense of other workers. Crowding out is nearly complete because the wage subsidy is offered to only a small segment of the unemployed, and hence generates only small employment gains. (A wage subsidy offered to all low-wage workers is accompanied by much less crowding out, in contrast to a wage subsidy offered to dislocated workers only.) Although crowding out is nearly complete, the crowding out of non-dislocated workers by dislocated workers is spread quite evenly over the various groups of non-dislocated workers, so that no single group bears the brunt of the wage subsidy's crowding-out effect. That is, each group of non-dislocated workers experiences a slight increase in unemployment duration and a slight decrease in employment level.

[22]We would need to introduce an explicit social welfare function if we wanted to draw conclusions about whether the temporary or permanent program has greater social benefits. We are currently pursuing work along these lines.

C. Sensitivity Analysis

The results to this point have all focussed on the reference case in which r = .008, s = .010, and T = 96.25. We now explore the sensitivity of the results to changes in the separation rate s and the total number of jobs available T. We focus on variations in these two parameters because doing so may give insight into how the effects of a wage subsidy would vary over the business cycle. Slack labor markets are associated with fewer total available jobs (T) and a lower job separation rate (s). So by examining how the impacts of the wage subsidy behave as T and s fall, we can learn how a recession might alter the outcomes that could be expected from a wage subsidy.

Tables 3 and 4 show the simulated effects of a wage subsidy with a higher and a lower separation rate s. In Tables 3A and 3B, we have set s equal to .006, whereas in Tables 4A and 4B, s=.014. Together Tables 3 and 4 show that when separation rates are higher, the wage subsidy causes smaller reductions in the unemployment duration of dislocated workers, but causes larger increases in their employment. This ambiguity occurs because there are two effects of a higher separations rate, s. First, when s is higher, the duration of jobs is shorter. This implies that the wage subsidy is worth less to a worker who receives it (because jobs are less enduring), so workers who could receive the subsidy respond less strongly -- that is, their search effort increases by less. It follows that a higher s implies smaller reductions in unemployment duration of dislocated workers. Second, when s is higher, there are more job vacancies. Hence, even though workers' search effort increases by less (as just noted), even the moderated increase in search effort generates more employment. So the additional vacancies implied by a higher s mean that the wage subsidy leads to larger employments gains for dislocated workers.

The wage subsidy's impact on unemployment duration of dislocated workers is not especially sensitive to changes in s in the range of .006 to .014 -- the variation is on the order of 25 to 33%. (See, for example, the top rows of Tables 3A and 4A: for s=.006, the wage subsidy's impact on unemployment duration is -2.378 weeks, whereas for s=.014, the impact is -1.778 week.) But the wage subsidy's impact on employment of dislocated workers varies greatly with changes in s in the range of .006 to .014 -- on the order of 75 to 90%. (See the top rows of Tables 3B and 4B: for s=.006, the wage subsidy increases employment by 713 per 100,000, whereas for s=.014, the impact is 1243 per 100,000.) In either case, however, crowding-out of non-dislocated workers is nearly complete (over 99%), and most of the employment gains of dislocated workers come at the expense of other workers.

Tables 5 and 6 show the simulated effects of a wage subsidy when total available jobs are higher and lower than the reference case. In Tables 5A and 5B, T is set at 95, whereas in Tables 6A and 6B, T=97.5. Tables 5 and 6 show that when there are more jobs available, the wage subsidy causes both smaller reductions in the unemployment duration of dislocated workers and smaller gains in employment.[23] However, the changes in T we consider (95 to 97.5) lead to relatively small changes in the wage subsidy's impact on both unemployment duration and employment of dislocated workers -- on the order of 7 to 11%. (Regarding unemployment duration, for example, the top rows of Tables 5A and 6A show that for T=95, the wage subsidy's impact on unemployment duration is -2.077 weeks, whereas for T=97.5, the impact is -1.877 weeks. Regarding employment levels, the top rows of Tables 5B and 6B show that for T=95, the wage subsidy increases employment by 1038 per 100,000, whereas

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[23]The wage subsidy's impacts are smaller when T is larger because when T is high, jobs are available and unemployment is low. As a result, workers are able to find jobs quickly even without searching hard. In effect, search effort is less important to job search outcomes when jobs are abundant. Since the wage subsidy increases search effort, it has a smaller impact when T is high.

for T=97.5, the impact is 937 per 100,000.) It follow that the crowding-out effect is basically invariant to changes in the total number of jobs available.

We conclude that the basic results described for the reference case do not change appreciably when the separation rate (s) and total jobs available (T) vary over a fairly significant range. The direct impacts of the wage subsidy on dislocated workers change ambiguously with variation in s, but unambiguously increase with increases in T. Crowding out is virtually unaffected by changes in either s or T, and is virtually complete in all cases.

D. Comparisons with a Reemployment Bonus

The Clinton Administration's Reemployment Act (REA) would enable state employment security agencies to offer a reemployment bonus to dislocated workers.[24] Such a reemployment bonus program would offer a lump-sum cash payment to dislocated workers who find reemployment within about 3 months of losing their job, and who hold that new job for at least 4 months.

The similarities between a reemployment bonus and a wage rate subsidy invite comparison. Both attempt to encourage the reemployment of dislocated workers by offering a financial inducement to dislocated workers to seek and accept a new job. The main difference between the two is in the way the financial inducement is structured. The reemployment bonus is a relatively large one-time payment provided about 7 months after job

[24]Actually, the bonus would be offered to workers who meet the state's "profiling" criteria -- that is, to workers who are predicted to have a high probability of exhausting their UI benefits. Conceptually, the correspondence between workers who meet the profiling criteria and dislocated workers is incomplete. But in our model, dislocated workers are those who have lost high-wage jobs and have little expectation of returning to such jobs. Hence, they have long expected durations of unemployment and would meet most conceivable profiling criteria. In other words, the correspondence between the model's dislocated workers and profiled workers is reasonably good.

loss, whereas the wage subsidy is a smaller but continuing payment provided over a longer period of time. Also, the wage subsidy does not require workers to find reemployment within as short a time as 3 months.

Two obvious questions arise about reemployment bonuses and wage subsidies. First, how do their direct and indirect impacts differ? Second, can a reemployment bonus and a wage subsidy be structured so as to have identical incentives and impacts?

Tables 7A and 7B display the impacts of a $500 cash bonus offered to dislocated workers who find a new job within 12 weeks. The flat $500 bonus is a natural one to examine because it was tested in the Illinois experiment, the results of which we have used to calibrate our model. Also, the $500 bonus represents 3 to 4 times the average weekly benefit amount received by UI recipients in the Illinois experiment, and the reemployment bonus programs enabled by the proposed Reemployment Act call for a bonus of similar size. To obtain the results shown in Tables 7A and 7B, we used the model, method of calibration, and solution algorithm described in section II. Also, the parameters used are the same as those used in the standard case underlying Tables 1A and 1B, so differences between the wage subsidy and the reemployment bonus can be understood by comparing Tables 1 and 7.

The figures displayed in Tables 7A and 7B are remarkable because they suggest that the direct impacts of the $500 bonus are uniformly about one-half the impacts of a wage subsidy. For example, the bonus-induced reduction in the unemployment duration of dislocated workers -- about .9 weeks -- is about half the reduction induced by the wage subsidy (see Table 1A). The bonus-induced increase in employment of dislocated workers -- about 450 to 470 per 100,000 -- is about half the increase induced by the wage subsidy (see Table 1B). With both the reemployment bonus and the wage subsidy, the employment gains of dislocated workers are offset nearly one for one by employment losses for other workers.

(For example, in the first row of Table 7B, dislocated workers' employment gain of 447 is offset by a loss of 443 for other workers. In the first row of Table 1B, the gain of 989 is offset by a loss of 982. So in both case, relative crowding-out is virtually complete.)

We conclude that the direct impacts of a reemployment bonus are qualitatively the same as those of a wage subsidy -- the differences are a just matter of scaling. It follows that it should be possible to structure a reemployment bonus and a wage subsidy so that they have identical incentives and impacts. Using our model, it is straightforward to find the bonus amount that would have the same impact as the wage subsidy for our reference case. For the single labor market model with $h=.5$, we find that a bonus of $1,104 would have direct and indirect impacts identical to the temporary wage subsidy.

We concluded above that the temporary wage subsidy has an impact that is about twice that of the $500 reemployment bonus. On the other hand, the wage subsidy's impact comes at considerably more that twice the expense. Since the wage subsidy amounts to $154 biweekly [($846-$538)/2 = $154], the amount paid in wage subsidies to a worker would exceed the $500 bonus payment after only two months.[25]

This added expense of the wage subsidy needs to considered in light of the loses experienced by dislocated workers. Jacobson, LaLonde, and Sullivan (1993) have argued that the case for a wage subsidy is not so much improved efficiency as greater equity. That is, the purpose of a wage subsidy is to redistribute income to workers who have lost their jobs due to economic restructuring, in addition to getting dislocated workers back to work.

The wage subsidy could be viewed as a better method of transferring income to dislocated workers than the reemployment bonus because the bonus is paid only to workers

[25]These figures take account of the break-up of jobs in calculating the total expense of a wage subsidy.

who are fortunate enough to find reemployment within about 3 months. The wage subsidy, on the other hand, would be paid to a dislocated worker regardless of when he or she gained reemployment.

E. Extensions: Firm Behavior

The model we have used makes no attempt to model firm behavior. In fact, our firms are quite passive -- when they have a vacancy they randomly choose from the pool of applicants and pay a wage that is determined outside of the model. Moreover, the total number of jobs available (T) is fixed, so that we do not allow the demand for labor to change as the result of the wage subsidy. (The number of steady-state vacancies is endogenous and falls with the implementation of the wage subsidy. That is why crowding out of non-dislocated workers is not quite complete.)

We begin this section by indicating the reasonableness of the assumptions that the wage and T are exogenous. However, we also describe how our model could be extended to make the wages and T endogenous, and discuss the sensitivity of our results to these assumptions.

There are several reasons to expect that wage rates would not change after the implementation of the wage subsidy program. First, the wage subsidy is offered to a small fraction of the unemployed and it is not likely that a change in the behavior of a fraction of the unemployed could have significant aggregate wage effects. This is exactly why so few jobs are created by the wage subsidy in the first place -- even though all dislocated workers search harder, there are too few of them to have a large aggregate impact. Second, experimental evidence suggests strongly that a reemployment bonus program induces no change in wage rates (Woodbury and Spiegelman 1987; Decker and O'Leary 1992). Since the reemployment bonus and wage subsidy programs have similar behavioral impacts, one would expect them to have similar wage effects. Third, in an early version of our work on the crowding-out (or displacement) effects of reemployment bonuses, we developed a model similar in flavor to the one used here, except that it allowed for endogenous wage rates (see Davidson and Woodbury 1990). We did so by introducing two profit functions for firms--one that calculates the expected lifetime profit for a firm with a filled job and one that calculates the expected lifetime profit for a firm with a vacancy. We then assumed that wages are negotiated once the worker and firm make contact and that the resulting wage divides evenly the surplus created by the job. Our model predicted that the reemployment bonus would have almost no impact on wages, which is exactly what happened in the reemployment bonus experiments. It follows that if we extended the model presented in this paper in a similar manner, we would again obtain the prediction that wage subsidies to dislocated workers should produce no significant changes in wage rates. The cost of extending the model in such a manner is rather high, however, in that the model would more than double in size.

It is also possible to extend our model so that T is endogenously determined. To do so, we could follow Pissarides (1990) and assume that firms create vacancies until the expected profit from doing so equals the cost of creating the vacancy.[26] Since the wage subsidy increases search effort, it reduces the expected duration of a vacancy, thereby making it more profitable for firms to create vacancies. Thus, if T does change as the result of the wage subsidy, it should increase, which would reduce the amount of crowding-out suggested by our simulations. We would expect, however, that any change in T would be quite small since the wage subsidy changes the behavior of so few workers.

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[26]Although this extension of the model is straightforward theoretically, calibration would be difficult. Given the limited amount of vacancy data available, it would be difficult to pin down the cost of creating a vacancy.

To test the sensitivity of our results to the assumption that T (total available jobs) is exogenous, we have calculated the increase in T that would be necessary to completely reverse our crowding-out results. That is, we calculate the change in T that would need to result from the wage subsidy to dislocated workers so that employment would be unchanged for other workers. Although the exact value varies with the parameters, we find that T would have to increase somewhere between .025 and .03 percent for there to be no crowding out. This is quite a small increase -- in the neighborhood of 30,000 to 40,000 jobs for the U.S. labor market. Whether this is plausible is an open question. The apparent sensitivity of crowding out to changes in total jobs available weakens our crowding-out results considerably, and suggests the potential importance of extending the model to make T endogenous. This is the focus of our work in progress.

IV. Discussion and Conclusions

This examination of a wage bill subsidy paid to dislocated workers has focussed on the subsidy's impacts on the duration of unemployment and levels of employment of dislocated and other workers. Our main results can be summarized as follows. The temporary wage subsidy program, which provides a subsidy for two years after gaining reemployment, has large direct impacts on the unemployment duration and employment level of dislocated workers. The results of our reference case suggest that the two-year subsidy would shorten the unemployment spells of dislocated workers by nearly 2 weeks, and would increase employment of dislocated workers by about 900 to 1000 per 100,000 in the labor force. (These findings are summarized in Tables 1A and 1B.) A wage subsidy paid in perpetuity would have impacts on dislocated workers that are about two and a half times those estimated for the temporary wage subsidy (see Tables 2A and 2B).

Wage subsidies to dislocated workers could also have indirect impacts on workers other than dislocated workers. Specifically, our results suggest that the wage subsidy leads to small increases in the unemployment duration, and decreases in employment, of non-dislocated workers. These decreases are relatively evenly dispersed across the various groups of non-dislocated workers we examine -- high- and low-wage UI eligibles, UI-eligible non-claimants, and UI-ineligibles. But on net, virtually all of the employment gains experienced by dislocated workers as a result of the wage subsidy come at the expense of other workers (see Tables 1B and 2B).

These main results appear to be quite robust to changes in the parameters that must be supplied in order to obtain our simulations (as shown in Tables 3, 4, 5, and 6). The crowding-out results, however, are very sensitive to the assumption that the total number of available jobs (T) is fixed and exogenous. As we report in section III.E, if employers responded to a wage subsidy for dislocated workers by increasing labor demand by just .025 to .03 percent, there would be no crowding out of non-dislocated workers.

We find that a reemployment bonus can be structured so as to obtain impacts identical to the wage subsidy. Specifically, in our reference case, a reemployment bonus of about $1,100 offered to workers who gain reemployment within 12 weeks of losing their job would have direct and indirect impacts that are identical to a temporary (two-year) wage subsidy.

If the crowding-out effects of the wage subsidy are as large as our main results suggest, then a wage subsidy to dislocated workers fails the Pareto criterion because some workers could be hurt by the program. But there may be three mitigating factors. First, the structural changes that lead to worker dislocation presumably improve the lot of the majority at the expense of dislocated workers. It is the burden of structural change, which itself fails the Pareto criterion, that the wage subsidy is intended to redress. Second, the crowding-out impacts of the wage subsidy that we find in our main results are widely dispersed over various groups of non-dislocated workers. Third, as just mentioned, our crowding-out results are quite sensitive to the assumption that the total number of available jobs is exogenous, so they should be treated as provisional.

We have not attempted to treat the administrative or funding issues that would need to be addressed if a wage subsidy for dislocated workers were adopted. It is clear, however, that the UI system provides a natural administrative vehicle for a wage subsidy program, specifically through continued payment of benefits (or some portion of benefits) to dislocated workers after reemployment. This in turn suggests a method of funding based on a dislocated worker's maximum UI benefit entitlement. For example, in the simulations reported above, a biweekly subsidy of $154 was paid to dislocated workers who were eligible for biweekly UI benefits of $245. Hence, in this example, a worker's UI benefit entitlement could fund up to about 41 weeks of a wage subsidy. Funding a wage subsidy that lasted longer or that began after some weeks of UI benefits had already been paid would require additional funding sources, and it is unclear where such funding could be found given the severe budget restrictions currently facing the federal government.

In any event, these administrative and funding issues would be moot if the direct impacts of a wage subsidy paid to dislocated workers were small, and they may yet be moot given the possibility that the wage subsidy may have harmful indirect impacts on workers other than dislocated workers. Mitigating the crowding-out effects of the wage subsidy are three factors -- that the crowding-out effects are widely dispersed, that the structural changes that result in dislocation of some workers (and hence a need for a policy like the wage subsidy) benefit most other workers, and that our crowding-out results are quite fragile. Our main purpose has been to appraise the direct and indirect impacts of the wage subsidy, and we believe that the findings suggest that a wage subsidy for dislocated workers is well worth further consideration.

Appendix: Complete Statement of the Model

A. Identities

(1) $T = J + V$

(2) $L = J + U$

(3) $U = U_i + U_k + \sum_{t=1,14} (U_{h,t} + U_{\ell,t} + U_{d,t}) + U_{h,e} + U_{\ell,e} + U_{d,e}$

B. Steady-State Conditions

In each equation below, the left-hand-side represents the flow into a state and the right-hand-side represents the flow out of that state. The labor market state for each equation is listed to the right in parentheses.

(4) $qsJ = m_iU_i$ (state U_i)

(5) $(1-q)(1-k)sJ = m_kU_k$ (state U_k)

(6) $(1-q)khsJ = U_{h,1}$ (state $U_{h,1}$)

(7) $(1-q)k(1-h)(1-d)sJ = U_{\ell,1}$ (state $U_{\ell,1}$)

(8) $(1-q)k(1-h)dsJ = U_{d,1}$ (state $U_{d,1}$)

(9) $(1-m_{j,t-1})U_{j,t-1} = U_{j,t}$ (state $U_{j,t}$ for $2 \le t \le 14$, $j = h, \ell, d$)

(10) $(1-m_{j,14})U_{j,14} = m_{j,e}U_{j,e}$ (state $U_{j,e}$ for $j = h, \ell, d$)

C. Reemployment Probabilities

(11) $m_i = p_i(V/T)[(1 - e^{-\lambda})/\lambda]$

(12) $m_k = p_k(V/T)[(1 - e^{-\lambda})/\lambda]$

(13) $m_{j,t} = p_{j,t}(V/T)[(1 - e^{-\lambda})/\lambda]$ for $1 \le t \le 14$, $j = h, \ell, d$

(14) $m_{j,e} = p_{j,e}(V/T)[(1 - e^{-\lambda})/\lambda]$ for $j = h, \ell, d$

(15) $\lambda = (1/T)[p_iU_i + p_kU_k + \sum_{t=1,14}\sum_j p_{j,t}U_{j,t} + \sum_j p_{j,e}U_{j,e}]$

D. Expected Lifetime Income

(16) $V_i = -c_i(p_i)^z + [miV_{i,w} + (1-m_i)V_i]/(1+r)$

(17) $V_{j,t} = x - c(p_{j,t})^z + [m_{j,t}V_{j,w} + (1-m_{j,t})V_{j,t+1}]/(1+r)$

for $1 \leq t \leq 13$, $j = h,\ell,d$

(18) $V_{j,14} = x - c(p_{j,14})^z + [m_{j,14}V_{j,w} + (1-m_{j,14})V_{j,e}]/(1+r)$

for $j = h,\ell,d$

(19) $V_{j,e} = -c(p_{j,e})^z + [m_{j,e}V_{j,w} + (1-m_{j,e})V_{j,e}]/(1+r)$

for $j = h,\ell,d$

(20) $V_{i,w} = w_i + [sV_i + (1-s)V_{i,w}]/(1+r)$

(21) $V_{j,w} = w_j + [sV_{j,1} + (1-s)V_{j,w}]/(1+r)$ for $j = h,\ell,d$

E. Optimal Search Effort

(22) $p_i = \arg\max V_i$

(23) $p_{j,t} = \arg\max V_{j,t}$ for $1 \leq t \leq 14$, $j = h,\ell,d$

(24) $p_{j,e} = \arg\max V_{j,e}$ for $j = h,\ell,d$

(25) $p_k = p_{h,e}$

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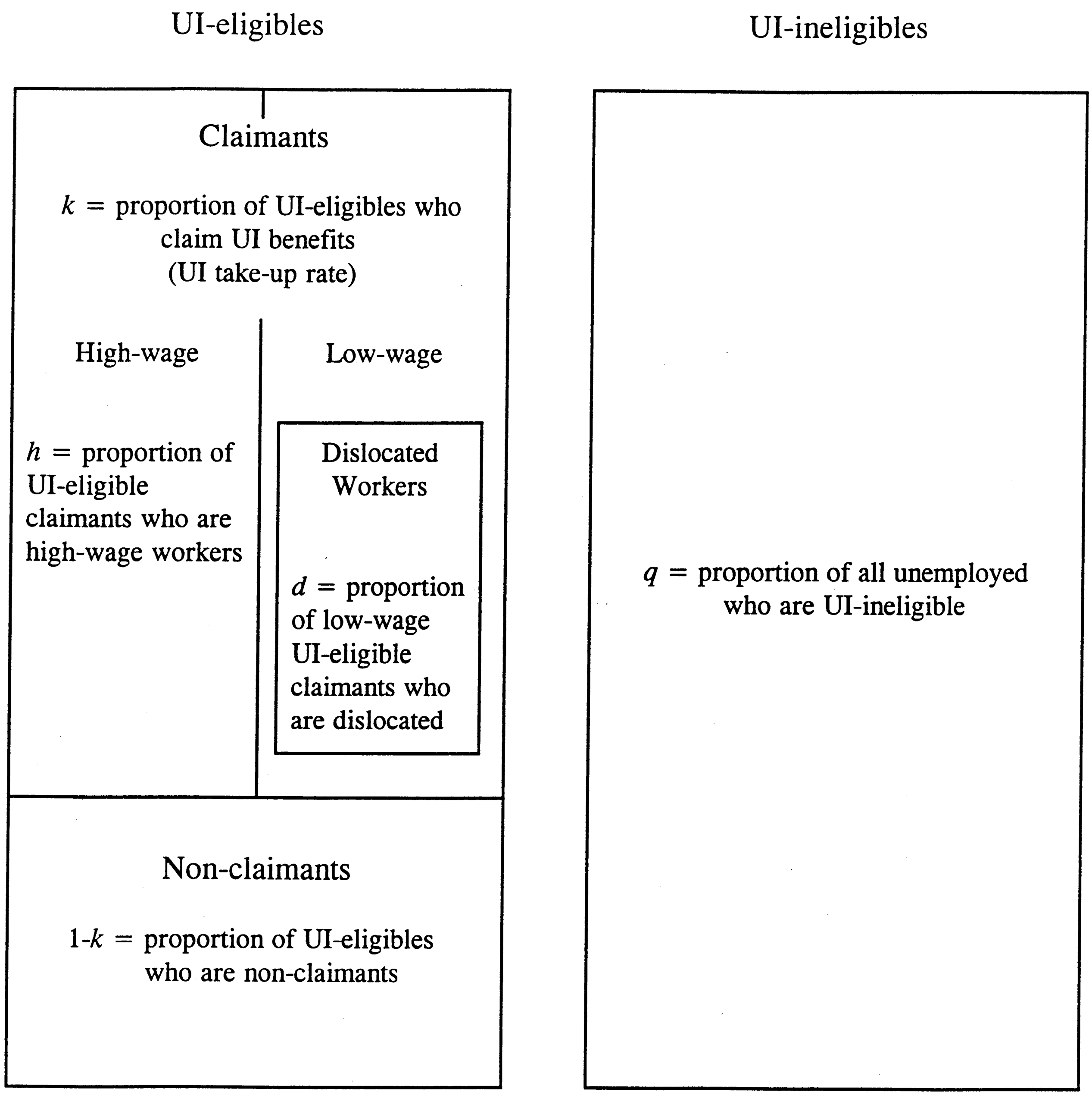

Figure 1
The Unemployed

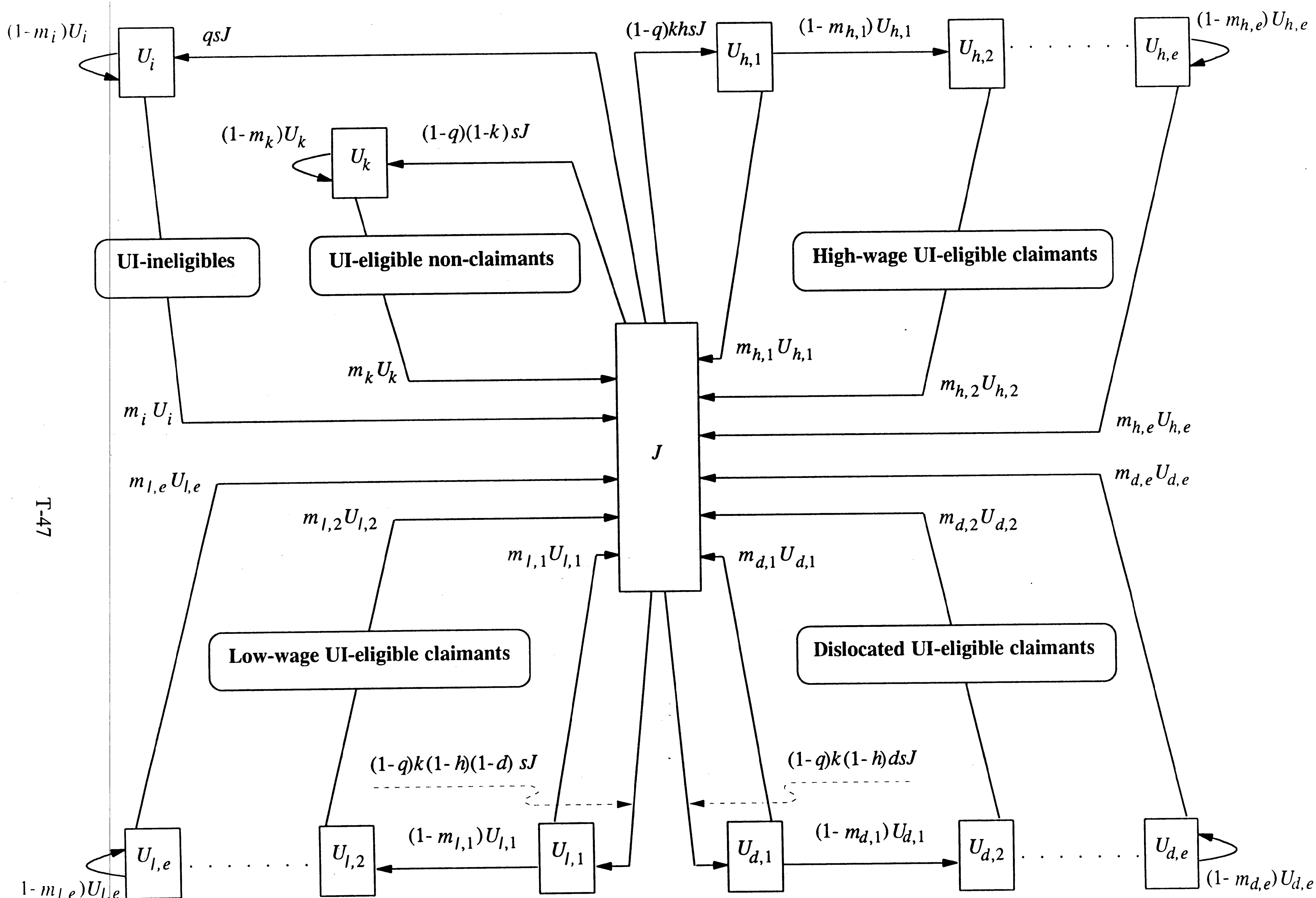

Figure 2
Labor Market Flows

Table 1A

Effects of a Temporary Wage Subsidy on Expected Duration of Unemployment, Reference Case

Model and Assumptions	Dislocated workers	High-wage UI-eligible claimants	Low-wage UI-eligible claimants	UI-eligible non-claimants	UI-ineligibles	h	k	d
Single Labor Market	-1.979	.0348	.0544	.0164	.0384	.5	.75	.176
	-1.966	.0272	.0425	.0127	.0300	.4	.75	.118
	-1.954	.0199	.0310	.0095	.0219	.3	.75	.076
	-1.942	.0128	.0199	.0064	.0140	.2	.75	.044
	-1.931	.0064	.0099	.0031	.0070	.1	.75	.020
Dual Labor Market, with UI-eligible non-claimants in *low-wage* sector	-1.846	NA	.0806	.0245	.0569	0	.75	.15
	-1.871	NA	.0536	.0163	.0379	0	.75	.10
	-1.895	NA	.0268	.0081	.0189	0	.75	.05
Dual Labor Market, with UI-eligible non-claimants in *high-wage* sector	-1.748	NA	.0965	NA	.0682	0	1	.15
	-1.778	NA	.0642	NA	.0453	0	1	.10
	-1.807	NA	.0320	NA	.0226	0	1	.05

Notes: The wage subsidy modelled is one in which the wage paid to a reemployed dislocated worker (w_d) is half the difference between the wage earned before dislocation (w_h) and the market opportunity wage currently facing the worker (w_l). Hence, $w_d=(w_h+w_l)/2$. The subsidy is limited to two years.

Results shown are for the reference case in which the biweekly separation rate $s=.010$, total jobs available $T=96.25$, and the biweekly interest rate $r=.008$.

h denotes the fraction of UI-eligible claimants who are high-wage workers; k denotes the UI take-up rate; and d denotes the fraction of low-wage UI-eligible claimants who are dislocated workers. In the single labor market model, $d=3h/[17(1-h)]$, as shown in the text. In the dual labor market models, d and k are independent.

Table 1B

Change per 100,000 in Employment under a Temporary Wage Subsidy, Reference Case

Model and Assumptions	Dislocated workers	Non-Dislocated Workers					h	k	d
		High-wage UI-eligible claimants	Low-wage UI-eligible claimants	UI-eligible non-claimants	UI-ineligibles	Total			
Single Labor Market	989	-213	-331	-201	-237	-982	.5	.75	.176
	982	-214	-336	-198	-229	-977	.4	.75	.118
	977	-207	-331	-207	-228	-973	.3	.75	.076
	971	-226	-323	-194	-226	-969	.2	.75	.044
	965	-193	-321	-193	-257	-964	.1	.75	.020
Dual Labor Market, with UI-eligible non-claimants in *low-wage* sector	922	NA	-399	-237	-275	-911	0	.75	.15
	935	NA	-406	-246	-275	-927	0	.75	.10
	947	NA	-413	-295	-236	-944	0	.75	.05
Dual Labor Market, with UI-eligible non-claimants in *high-wage* sector	873	NA	-505	NA	-354	-859	0	1	.15
	888	NA	-518	NA	-361	-879	0	1	.10
	903	NA	-532	NA	-366	-898	0	1	.05

Notes: See Table 1A.

Table 2A

Effects of a Permanent Wage Subsidy on Expected Duration of Unemployment, Reference Case

Model and Assumptions	Dislocated workers	High-wage UI-eligible claimants	Low-wage UI-eligible claimants	UI-eligible non-claimants	UI-ineligibles	h	k	d
Single Labor Market	-4.941	.0870	.1361	.0822	.0959	.5	.75	.176
	-4.908	.0680	.1061	.0644	.0748	.4	.75	.118
	-4.876	.0498	.0774	.0474	.0546	.3	.75	.076
	-4.846	.0321	.0497	.0306	.0351	.2	.75	.044
	-4.815	.0160	.0247	.0153	.0174	.1	.75	.020
Dual Labor Market, with UI-eligible non-claimants in *low-wage* sector	-4.627	NA	.2024	.1232	.1429	0	.75	.15
	-4.680	NA	.1344	.0818	.0949	0	.75	.10
	-4.733	NA	.0669	.0408	.0473	0	.75	.05
Dual Labor Market, with UI-eligible non-claimants in *high-wage* sector	-4.393	NA	.2431	NA	.1717	0	1	.15
	-4.457	NA	.1612	NA	.1139	0	1	.10
	-4.521	NA	.0802	NA	.0567	0	1	.05

Notes: The wage subsidy modelled is one in which the wage paid to a reemployed dislocated worker (w_d) is half the difference between the wage earned before dislocation (w_h) and the market opportunity wage now facing the worker (w_ℓ). Hence, $w_d=(w_h+w_\ell)/2$. The subsidy is paid in perpetuity.

Results shown are for the reference case in which the biweekly separation rate (s)=.010, total jobs available (T)=96.25, and the biweekly interest rate (r)=.008.

h denotes the fraction of UI-eligible claimants who are high-wage workers; k denotes the UI take-up rate; and d denotes the fraction of low-wage UI-eligible claimants who are dislocated workers. In the single labor market model, $d=3h/[17(1-h)]$, as shown in the text. In the dual labor market models, d and k are independent.

Table 2B

Change per 100,000 in Employment under a Permanent Wage Subsidy, Reference Case

Model and Assumptions	Dislocated workers	Non-Dislocated Workers					h	k	d
		High-wage UI-eligible claimants	Low-wage UI-eligible claimants	UI-eligible non-claimants	UI-ineligibles	Total			
Single Labor Market	2469	-536	-833	-500	-583	-2452	.5	.75	.176
	2453	-530	-834	-500	-576	-2440	.4	.75	.118
	2437	-526	-809	-485	-607	-2427	.3	.75	.076
	2422	-509	-826	-509	-572	-2416	.2	.75	.044
	2407	-520	-780	-520	-584	-2404	.1	.75	.020
Dual Labor Market, with UI-eligible non-claimants in *low-wage* sector	2311	NA	-990	-600	-695	-2285	0	.75	.15
	2339	NA	-1001	-609	-711	-2321	0	.75	.10
	2366	NA	-1014	-627	-716	-2357	0	.75	.05
Dual Labor Market, with UI-eligible non-claimants in *high-wage* sector	2193	NA	-1266	NA	-891	-2157	0	1	.15
	2227	NA	-1297	NA	-906	-2203	0	1	.10
	2260	NA	-1317	NA	-931	-2248	0	1	.05

Notes: See Table 2A.

Table 3A

Effects of a Temporary Wage Subsidy on Expected Duration of Unemployment,
Sensitivity Analysis with $s=.006$

Model and Assumptions	Dislocatedw orkers	High-wage UI-eligible claimants	Low-wage UI-eligible claimants	UI-eligible non-claimants	UI-ineligibles	*h*	*k*	*d*
Single Labor Market	-2.378	.0486	.0844	.0447	.0614	.5	.75	.176
	-2.351	.0378	.0653	.0349	.0476	.4	.75	.118
	-2.325	.0276	.0472	.0257	.0344	.3	.75	.076
	-2.300	.0177	.0301	.0166	.0220	.2	.75	.044
	-2.275	.0879	.0149	.0080	.0109	.1	.75	.020
Dual Labor Market, with UI-eligible non-claimants in *low-wage* sector	-2.145	NA	.1206	.0651	.0880	0	.75	.15
	-2.181	NA	.0802	.0433	.0585	0	.75	.10
	-2.216	NA	.0400	.0216	.0292	0	.75	.05
Dual Labor Market, with UI-eligible non-claimants in *high-wage* sector	-1.980	NA	.1412	NA	.1031	0	1	.15
	-2.022	NA	.0939	NA	.0686	0	1	.10
	-2.064	NA	.0468	NA	.0342	0	1	.05

Notes: See Table 1A.

Table 3B

Change per 100,000 in Employment under a Temporary Wage Subsidy,
Sensitivity Analysis with $s=.006$

Model and Assumptions	Dislocated workers	Non-Dislocated Workers					h	k	d
		High-wage UI-eligible claimants	Low-wage UI-eligible claimants	UI-eligible non-claimants	UI-ineligibles	Total			
Single Labor Market	713	-148	-247	-138	-178	-711	.5	.75	.176
	705	-138	-251	-138	-176	-703	.4	.75	.118
	697	-139	-244	-139	-174	-696	.3	.75	.076
	690	-133	-238	-133	-185	-689	.2	.75	.044
	683	-171	-228	-113	-171	-683	.1	.75	.020
Dual Labor Market, with UI-eligible non-claimants in *low-wage* sector	643	NA	-282	-156	-203	-641	0	.75	.15
	654	NA	-285	-154	-214	-653	0	.75	.10
	665	NA	-285	-166	-213	-664	0	.75	.05
Dual Labor Market, with UI-eligible non-claimants in *high-wage* sector	594	NA	-344	NA	-248	-592	0	1	.15
	607	NA	-346	NA	-259	-605	0	1	.10
	619	NA	-361	NA	-257	-618	0	1	.05

Notes: See Table 1A.

Table 4A

Effects of a Temporary Wage Subsidy on Expected Duration of Unemployment,
Sensitivity Analysis with $s = .014$

Model and Assumptions	Dislocated workers	High-wage UI-eligible claimants	Low-wage UI-eligible claimants	UI-eligible non-claimants	UI-ineligibles	h	k	d
Single Labor Market	-1.778	.0299	.0441	.0291	.0303	.5	.75	.176
	-1.770	.0235	.0346	.0218	.0237	.4	.75	.118
	-1.762	.0172	.0253	.0164	.0174	.3	.75	.076
	-1.755	.0111	.0163	.0105	.0112	.2	.75	.044
	-1.747	.0056	.0081	.0055	.0056	.1	.75	.020
Dual Labor Market, with UI-eligible non-claimants in *low-wage* sector	-1.679	NA	.0665	.0426	.0458	0	.75	.15
	-1.699	NA	.0443	.0286	.0305	0	.75	.10
	-1.719	NA	.0221	.0139	.0152	0	.75	.05
Dual Labor Market, with UI-eligible non-claimants in *high-wage* sector	-1.604	NA	.0806	NA	.0555	0	1	.15
	-1.629	NA	.0537	NA	.0369	0	1	.10
	-1.654	NA	.0268	NA	.0184	0	1	.05

Notes: See Table 1A.

Table 4B

Change per 100,000 in Employment under a Temporary Wage Subsidy, Sensitivity Analysis with $s = .014$

Model and Assumptions	Dislocated workers	Non-Dislocated Workers					h	k	d
		High-wage UI-eligible claimants	Low-wage UI-eligible claimants	UI-eligible non-claimants	UI-ineligibles	Total			
Single Labor Market	1243	-277	-415	-264	-277	-1233	.5	.75	.176
	1238	-275	-421	-259	-275	-1230	.4	.75	.118
	1232	-280	-409	-258	-280	-1227	.3	.75	.076
	1228	-272	-408	-272	-272	-1224	.2	.75	.044
	1223	-271	-407	-271	-271	-1220	.1	.75	.020
Dual Labor Market, with UI-eligible non-claimants in *low-wage* sector	1172	NA	-492	-321	-341	-1154	0	.75	.15
	1188	NA	-511	-325	-341	-1177	0	.75	.10
	1203	NA	-504	-347	-347	-1198	0	.75	.05
Dual Labor Market, with UI-eligible non-claimants in *high-wage* sector	1120	NA	-665	NA	-432	-1097	0	1	.15
	1138	NA	-681	NA	-442	-1123	0	1	.10
	1156	NA	-696	NA	-452	-1148	0	1	.05

Notes: See Table 1A.

Table 5A

Effects of a Temporary Wage Subsidy on Expected Duration of Unemployment, Sensitivity Analysis with $T=95$

Model and Assumptions	Dislocated workers	High-wage UI-eligible claimants	Low-wage UI-eligible claimants	UI-eligible non-claimants	UI-ineligibles	h	k	d
Single Labor Market	-2.077	.0404	.0647	.0380	.0461	.5	.75	.176
	-2.061	.0315	.0504	.0297	.0359	.4	.75	.118
	-2.046	.0231	.0366	.0218	.0262	.3	.75	.076
	-2.031	.0149	.0235	.0137	.0168	.2	.75	.044
	-2.015	.0074	.0116	.0067	.0083	.1	.75	.020
Dual Labor Market, with UI-eligible non-claimants in *low-wage* sector	-1.915	NA	.0948	.0559	.0677	0	.75	.15
	-1.944	NA	.0631	.0370	.0451	0	.75	.10
	-1.977	NA	.0315	.0181	.0225	0	.75	.05
Dual Labor Market, with UI-eligible non-claimants in *high-wage* sector	-1.797	NA	.1127	NA	.0805	0	1	.15
	-1.832	NA	.0749	NA	.0536	0	1	.10
	-1.866	NA	.0372	NA	.0269	0	1	.05

Notes: See Table 1A.

Table 5B

Change per 100,000 in Employment under a Temporary Wage Subsidy, Sensitivity Analysis with $T=95$

Model and Assumptions	Dislocated workers	Non-Dislocated Workers					h	k	d
		High-wage UI-eligible claimants	Low-wage UI-eligible claimants	UI-eligible non-claimants	UI-ineligibles	Total			
Single Labor Market	1038	-218	-359	-207	-250	-1034	.5	.75	.176
	1030	-219	-356	-205	-246	-1026	.4	.75	.118
	1023	-227	-359	-208	-246	-1040	.3	.75	.076
	1015	-225	-338	-197	-253	-1013	.2	.75	.044
	1008	-224	-336	-224	-224	-1008	.1	.75	.020
Dual Labor Market, with UI-eligible non-claimants in *low-wage* sector	957	NA	-414	-242	-294	-950	0	.75	.15
	971	NA	-418	-248	-300	-966	0	.75	.10
	986	NA	-437	-246	-301	-984	0	.75	.05
Dual Labor Market, with UI-eligible non-claimants in *high-wage* sector	898	NA	-517	NA	-372	-889	0	1	.15
	915	NA	-531	NA	-377	-908	0	1	.10
	933	NA	-536	NA	-395	-931	0	1	.05

Notes: See Table 1A.

Table 6A

Effects of a Temporary Wage Subsidy on Expected Duration of Unemployment, Sensitivity Analysis with T=97.5

Model and Assumptions	Dislocated workers	High-wage UI-eligible claimants	Low-wage UI-eligible claimants	UI-eligible non-claimants	UI-ineligibles	*h*	*k*	*d*
Single Labor Market	-1.877	.0302	.0459	.0287	.0319	.5	.75	.176
	-1.867	.0237	.0360	.0226	.0250	.4	.75	.118
	-1.858	.0174	.0263	.0168	.0183	.3	.75	.076
	-1.849	.0112	.0169	.0105	.0118	.2	.75	.044
	-1.840	.0056	.0084	.0059	.0059	.1	.75	.020
Dual Labor Market, with UI-eligible non-claimants in *low-wage* sector	-1.769	NA	.0689	.0433	.0480	0	.75	.15
	-1.790	NA	.0458	.0288	.0319	0	.75	.10
	-1.811	NA	.0229	.0144	.0159	0	.75	.05
Dual Labor Market, with UI-eligible non-claimants in *high-wage* sector	-1.688	NA	.0832	NA	.0580	0	1	.15
	-1.713	NA	.0553	NA	.0386	0	1	.10
	-1.739	NA	.0276	NA	.0193	0	1	.05

Notes: See Table 1A.

Table 6B

Change per 100,000 in Employment under a Temporary Wage Subsidy, Sensitivity Analysis with T=97.5

		Non-Dislocated Workers							
Model and Assumptions	Dislocated workers	High-wage UI-eligible claimants	Low-wage UI-eligible claimants	UI-eligible non-claimants	UI-ineligibles	Total	h	k	d
Single Labor Market	937	-206	-310	-193	-219	-928	.5	.75	.176
	933	-199	-314	-199	-215	-927	.4	.75	.118
	928	-203	-315	-203	-203	-924	.3	.75	.076
	924	-205	-307	-205	-205	-922	.2	.75	.044
	920	-212	-283	-212	-212	-919	.1	.75	.020
Dual Labor Market, with UI-eligible non-claimants in *low-wage* sector	883	NA	-373	-238	-259	-870	0	.75	.15
	894	NA	-379	-237	-269	-885	0	.75	.10
	905	NA	-400	-233	-267	-900	0	.75	.05
Dual Labor Market, with UI-eligible non-claimants in *high-wage* sector	842	NA	-486	NA	-339	-825	0	1	.15
	855	NA	-499	NA	-344	-843	0	1	.10
	869	NA	-503	NA	-360	-863	0	1	.05

Notes: See Table 1A.

Table 7A

Effects of a $500 Reemployment Bonus on Expected Duration of Unemployment, Reference Case

Model and Assumptions	Dislocated Workers	High-wage UI-eligible claimants	Low-wage UI-eligible claimants	UI-eligible non-claimants	UI-ineligibles	h	k	d
Single Labor Market	-0.894	.0157	.0248	.0145	.0174	.5	.75	.176
	-0.903	.0125	.0199	.0119	.0138	.4	.75	.118
	-0.913	.0093	.0145	.0087	.0102	.3	.75	.076
	-0.921	.0061	.0094	.0064	.0067	.2	.75	.044
	-0.930	.0031	.0048	.0028	.0034	.1	.75	.020
Dual Labor Market, with UI-eligible non-claimants in *low-wage* sector	-0.898	NA	.0392	.0238	.0277	0	.75	.15
	-0.912	NA	.0261	.0156	.0185	0	.75	.10
	-0.925	NA	.0131	.0075	.0923	0	.75	.05
Dual Labor Market, with UI-eligible non-claimants in *high-wage* sector	-0.903	NA	.0498	NA	.0327	0	1	.15
	-0.920	NA	.0332	NA	.0235	0	1	.10
	-0.937	NA	.0166	NA	.0117	0	1	.05

Notes: The reemployment bonus modelled is one in which a lump sum of $500 is paid to a dislocated worker who finds a new job within 12 weeks and holds the job for 4 months.

Results shown are for the reference case in which the biweekly separation rate $s=.010$, total jobs available $T=96.25$, and the biweekly interest rate $r=.008$. Hence, the results in this table should be compared with the wage subsidy impacts shown in Table 1A.

h denotes the fraction of UI-eligible claimants who are high-wage workers; k denotes the UI take-up rate; and d denotes the fraction of low-wage UI-eligible claimants who are dislocated workers. In the single labor market model, $d=3h/[17(1-h)]$, as shown in the text. In the dual labor market models, d and k are independent.

Table 7B

Change per 100,000 in Employment under a $500 Reemployment Bonus, Reference Case

Model and Assumptions	Dislocated Workers	Non-Dislocated Workers					h	k	d
		High-wage UI-eligible claimants	Low-wage UI-eligible claimants	UI-eligible non-claimants	UI-ineligibles	Total			
Single Labor Market	447	-93	-152	-93	-105	-443	.5	.75	.176
	452	-93	-155	-93	-109	-450	.4	.75	.118
	456	-103	-145	-103	-103	-454	.3	.75	.076
	461	-99	-164	-99	-99	-461	.2	.75	.044
	465	-133	-133	-133	-66	-465	.1	.75	.020
Dual Labor Market, with UI-eligible non-claimants in *low-wage* sector	449	NA	-193	-116	-135	-444	0	.75	.15
	455	NA	-196	-120	-135	-451	0	.75	.10
	462	NA	-201	-115	-144	-460	0	.75	.05
Dual Labor Market, with UI-eligible non-claimants in *high-wage* sector	451	NA	-262	NA	-181	-443	0	1	.15
	460	NA	-267	NA	-188	-455	0	1	.10
	468	NA	-279	NA	-186	-465	0	1	.05

Notes: See Table 7A. The results in this table should be compared with the wage subsidy results in Table 1B.

The U.S. Employment Service: A Review of Evidence Concerning its Operations and Effectiveness

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PREFACE

The U.S. Employment Service (ES) is unique among public institutions in that it offers assistance, both free of charge and without restriction, to jobseekers and to firms seeking qualified workers to fill openings. In the past two decades, however, questions have been raised concerning the Service's performance and continued usefulness in a changing labor market. To address such questions this paper offers a critical review of the evidence concerning ES effectiveness, and identifies gaps in our current knowledge concerning program operations and impacts. The paper unfolds as follows.

To establish a context for the review, Section One traces the evolution of the ES program mission and functions over time, and the contemporaneous changes in agency workload and resources. Two salient conclusions emerge. First, the ES mission has enlarged considerably over the years to encompass a variety of functions, while its role vis-a-vis other employment and training institutions has become less clear and consistent, and more subject to states' interpretation. Second, the funding and workload implications of these changes are not transparent. While it has been argued that ES funding has not kept pace with changes in mission or responsibilities and that real dollar resources for ES activities have declined, a lack of historical data and changes in program funding arrangements, render it almost impossible to evaluate these claims or to assess over time trends in services (outputs) provided and their associated costs.

This institutional review guides the formulation of a behavioral model that describes the various tasks and functions assigned to the ES, including their interrelationships, outputs and factors which condition them. While standard benefit cost analysis provides an appropriate calculus for measuring ES performance, such an assessment should also recognize that the ES is a complex operation with multiple outputs whose benefits and costs warrant measurement. Our review of existing empirical research can then be laid against this behavioral model to isolate gaps in current knowledge. Also, since most of the studies reviewed here are point-in-time examinations of a narrow range of ES activities, we are able to place them in their appropriate historical context and note potential areas in which subsequent program changes may affect the external validity of results. These conceptual and measurement issues are discussed at length in Section Two.

Section Three presents the empirical evidence concerning effects of ES services on workers' reemployment and earnings; and on unemployment duration and benefit receipt. Both labor exchange (job finding) services and work test administration are considered.

Section Four concludes the paper with a summary of findings and sketches the broad outlines of an agenda for future research.

SECTION ONE

AN INSTITUTIONAL HISTORY OF THE EMPLOYMENT SERVICE: EVOLUTION OF THE PROGRAM'S MISSION AND RESOURCES

The Employment Service (ES) was established in 1933 to assist unemployed workers in finding public or private sector employment, and to assist firms in filling job vacancies. Since inception it has operated as a Federal-state partnership, with the Federal government responsible for collection of employer payroll taxes which partially finance services provided; states, in turn, are responsible for day-to-day program operation.

The original mandate of the Employment Service was to facilitate the matching of unemployed workers to job vacancies by obtaining information on their skills and qualifications and then referring them to appropriate job openings submitted by employers. During the past 60 years, however, the mission of the ES has enlarged beyond this labor exchange mandate to encompass a variety of additional compliance, regulatory, and information gathering functions. A variety of population groups -- youth, older workers, veterans, the disadvantaged, and lately, dislocated workers -- have been at times singled out for priority services. And over time the ES has gone from the sole institutional supplier of employment assistance, to one of many players.

To establish a context for assessing ES performance and effectiveness, Section 1.1 describes the evolution of the ES program mission and functions during the past six decades, highlighting over time changes in the agency's responsibilities and in its role vis a vis the larger institutional environment of publicly funded employment and training agencies. This section draws heavily on the work of Stevens (1978; 1989); McNeil (1988); and Ainsworth (1991), who have each contributed review articles on the institutional history of the ES; and on Johnson, et. al. (1983). Section 1.2 then considers contemporaneous -- though not necessarily concomitant -- changes in ES program resources and outputs. Section 1.3 summarizes the major conclusions suggested by these historical data.

1.1 The Evolution of Employment Service Mission and Functions

Established in 1933 by the Wagner-Peyser Act, the Employment Service's initial mission was to register the millions of Depression-era job seekers and refer them to newly created job opportunities in government public works or work relief projects. The Service was also responsible for matching private employer job orders with rosters of available workers as the economy began to recover. As set forth in the legislation, the agency's essential function was, therefore, as a public *labor exchange:* gathering information on labor supply and demand, and assisting in the screening and referral of applicants to job openings.

Two years later, the Social Security Act created the Unemployment Insurance (UI) program. Under the Act, states were required to pay UI benefits only to individuals registered with the ES. Though UI staff were primarily responsible for enforcement of claimants' compliance with these requirements, ES staff were required to *register UI claimants for work, refer qualified candidates to suitable job openings, and notify UI of noncompliance with registration requirements.* These activities -- commonly referred to as the "work test" -- represented an added responsibility for ES, as well as a potential shift in priorities. Whereas ES had previously served jobseekers who had applied for assistance voluntarily, starting in 1935 the agency was faced with individuals who were *required* to register to receive their UI benefits[1]. Additionally, the same ES staff might be responsible for compliance monitoring as well as making referrals to job openings.

Both ES and the UI program were operated as Federal-state partnerships. The Federal government was responsible for collecting payroll taxes which financed the programs; for establishing broad program regulations and performance expectations; and for allocating operating budgets to the states. States, in turn, were responsible for day to day program operations.

During the Second World War, as priorities focused on attaining national civilian manpower requirements, the Federal government assumed full operational control over the ES. In 1942 the agency was placed under the authority of the Federal War Manpower

1. In 1971, the ES was also given the responsibility for performing a similar function for certain recipients of food stamps and Aid to Families with Dependent Children (AFDC). See Ainsworth (1991).

Commission which was charged with assisting employers in filling vacancies caused by the draft; in transferring workers from less essential industries to wartime production; in directing the flow of migratory workers; and in recruiting new entrants to the labor force. A prototype Interstate Clearance System, which facilitated the identification and movement of skilled labor to areas of the country with heavy demands, was developed during this period.

Following the war, the ES reverted back to joint Federal-state administration. The agency's chief responsibilities at this time were providing readjustment services to returning veterans, and smoothing the transition to a postwar, civilian economy. During this postwar period, *provision of special services to veterans* became an explicit responsibility. The Serviceman's Readjustment Act, commonly known as the GI Bill (Title IV of PL-346), offered special, preferential counseling and placement opportunities for veterans through the ES, including apprenticeship and on the job training. The Service also helped to channel workers, dislocated as a result of cutbacks in war production, into civilian employment.

During the 1950s, the ES was assigned two additional missions. The first concerned another compliance issue: the *certification of alien labor.* For foreign workers to enter the U.S., the ES was required to demonstrate that the supply of U.S. workers was inadequate to meet demands and that they would not suffer adverse consequences, including employment and wage displacement, if foreign workers were admitted. The second mission concerned *special services to additional population subgroups*. Previously, the ES had focused its attention on the average jobseeker, with special emphasis only on returning veterans and individuals with special skills in high demand. Beginning in the mid-1950s, older workers, the disabled, and youth were all targeted for special assistance.

During the early 1960s, *labor market information gathering*, conducted up to this point as part of the basic labor exchange activity, became a mission unto itself. The Area Redevelopment Act of 1961 charged the ES with gathering information on unemployment levels by labor market area, to determine whether disadvantaged areas qualified for Federal assistance. The ES was also required to cooperate with state and local educational officials by providing information on occupational trends in labor markets that could be used in curriculum development and counseling.

With the passage of the Manpower Development and Training Act (MDTA) in 1962, and the Economic Opportunity Act of 1964, ES took on additional human resource development activities above and beyond those performed as part of its labor exchange mission. These included screening and referral of applicants to training institutions and to Great Society programs; surveys to assess placement rates in various occupational training areas; job readiness activities for new labor force entrants; and marketing services to employers. No longer was the agency's "assistive" (non-enforcement) mission construed to encompass *only* labor exchange; rather, the ES played a much broader role in the design and delivery of job training and human resource development programs.

Much of the expansion in ES' role and mission during this timeframe appears to have arisen because it was the only public institution in existence with experience in providing reemployment assistance. The legislation enacted during the 1960s either did not provide for development of new institutions, or did so with the expectation that they would coordinate with, and rely on, the established capabilities of the ES.

With the passage of the Comprehensive Employment and Training Act (CETA), in 1973, the ES was no longer the only institutional resource, and the broad role it had played vis-a-vis human resource development began to narrow. CETA created additional institutional infrastructure for local design and delivery of training and employment programs: Local Prime Sponsors, overseen by local elected officials, were designated in each state and given the broad mission of designing and administering skills training programs for the area. The ES was "refocused" on basic labor exchange functions, defined more narrowly in this context as referral to job openings and job placement. ES was expected to provide assessment and placement support to the CETA Prime Sponsors, yet many observers argued that conflicts over bounds of authority and responsibility -- "turf issues" -- inhibited integration of service delivery (Levitan and Taggart, 1976).

In 1982, the Job Training Partnership Act (JTPA) succeeded CETA, introducing a major shift in national employment and training policy and philosophy. JTPA targeted the bulk of program resources on training and allocated resources to states based on their performance in placing trainees and increasing their earnings. As part of the Act, the original 1933 Wagner-Peyser Amendments were amended for the first time. The intent of these amendments was to refocus the mission of the ES on its basic labor exchange role and to enhance linkages between the ES and JTPA programs.

The amendments themselves encompassed four principal elements:

- *The more complete transfer of ES program management from the Federal level to the states.* While the ES had always operated under substantial state control due to the nature of the Federal-state partnership, the amendments transferred much more authority to state partners for developing program regulations and performance expectations.

- *The development of improved linkages between ES, JTPA, and other employment and training agents.* The amendments sought improved linkages between ES and the other institutional players in the employment and training arena, including JTPA, vocational education, economic development agencies, and other state and local groups. Local joint planning involving ES and JTPA was mandated.

- *A reemphasis of basic labor exchange functions.* The amendments did not explicitly alter the list of tasks and functions assigned to the ES, but they *did* change the manner in which activities outside the basic labor exchange mission were funded. Specifically, the amendments allowed for the negotiation of separate reimbursable grant agreements between states and Federal agencies for such functions as enforcement and compliance, and special services to certain groups (such as veterans), reserving the basic ES grant for labor exchange services.

- *Changes in the interstate funds allocation formula.* The amendments altered the funds allocation formula to target resources to states based on need -- measured by unemployment indicators -- rather than placement performance and workload, as had been the case in the past. For the first time, states also received their allocation in dollars, rather than staff year equivalents.

A review of the implementation of the Wagner-Peyser amendments, carried out during Program Years 1983 and 1984 (Richardson, et. al., 1985), suggested that the Federal role in providing oversight and technical assistance was indeed sharply diminished as had been envisioned, and most states embraced the additional authority and responsibility they were given for program management. Thus, one important goal of the amendments -- devolution of responsibility for ES operations from Federal to state partners -- appears to have been accomplished.

An important complement to this devolution was the reduction of Federal reporting requirements, which reduced the amount of data available on program services, outputs and costs. States were only required to report counts of applicants by sex, veteran status, and economic disadvantage; the number receiving various services and placed; and counts of job openings received from employers and the proportion filled. Responsibility for setting performance expectations and measuring performance was also fully delegated to the states. The ceding to states of control over goal-setting, over establishment of rewards and recognitions, and over information flows concerning program operations, represented a very significant change in the nature of the Federal-state partnership.

The Wagner-Peyser amendments appeared to have only a modest influence on coordination of ES and JTPA activities, at least during the early implementation period (Richardson, et. al., 1985). Local joint planning requirements in and of themselves did not appear to foster real integration of service delivery; "turf" issues still remained. Rather, coordination was more evident in states where administrative responsibility for both programs was assigned to one agency, or where "cabinet clusters" composed of representatives from the various human resources departments were formed (Richardson, et. al., 1985; Brady, 1987). States which experienced a decline in program resources under the amendments also evidenced more coordination, most likely as they tried to leverage resources to maintain services.

The funds allocation formula had a substantial effect in redistributing resources across the states, with some states gaining as much as a 20 percent increase in program resources (Richardson, et. al., 1985). The reimbursable grant provision, on the other hand, did not appear to immediately influence the overall level of ES program resources. During the period covered by the study, Richardson (1985) found that once reimbursements from other Federal agencies were taken into account, the ES (Title III) program budget was adjusted to maintain a roughly constant level of resources.

In 1988, the Economic Dislocation and Worker Adjustment Assistance Act (EDWAA) amended JTPA to alter and improve services provided to dislocated workers. EDWAA stipulated an expanded role for the ES in these activities, but again permitted states quite substantial discretion in how this was to be accomplished. Early implementation of EDWAA was examined by Dickinson, et. al. (1992), and by the National

Commission for Employment Policy in its Congressionally mandated review of ES (Romero, et. al., 1991a). Both found that ES was indeed more involved, at both state and local levels, in services to dislocated workers; and that much of this involvement was stimulated by restructuring administrative responsibilities for job training and human resources at the state level.

Specifically, sixty five percent of states created an umbrella agency headed by a single administrator to oversee ES, UI, JTPA, and Trade Adjustment Assistance. In 40 percent of these states, ES was designated as the lead agency responsible for administering all employment and training programs under EDWAA. In other umbrella states ES was designated a role comparable in scope to the JTPA agency. In states without umbrella agencies, arrangements for involving ES in EDWAA activities were more complex but still evident (Romero, et. al., (1991a).

Most recently, a number of states have broadened the ES' role still further, to serve as the overall coordinator of employment and training services. Some of these changes have indeed been stimulated by the organizational arrangements effected under EDWAA, while in other cases the impetus has been declining resources and the need to leverage services through a more efficient delivery system. In still other cases, states have taken a "customer perspective" to the organization of their human resource systems and have attempted to streamline access.

Several states have initiated "one stop" shopping for coordinated service delivery at local level[2]. This concept entails bringing together representatives from the various employment and training programs in a single location, to streamline referral of applicants to the appropriate program. In such systems, one agency, often ES, acts as a "broker" or "gatekeeper," collecting initial data on applicants and performing initial assessment.

In New Jersey, for example, the Jobs New Jersey initiative links ES and JTPA service delivery systems at both state and local levels. UI offices offer an "Early Intercept" program which targets walk in dislocated workers and attempts to refer them to services quickly. In

2. States with such systems include Delaware, Idaho, Indiana, Pennsylvania, New Jersey, New York, North and South Dakota and Vermont.

Pennsylvania, Job Centers are networks of one stop locations operated out of ES offices, and providing an array of employment and training services.

A related notion is that of a "single point of contact," or single point of entry into the human resources system. UI and ES staff play the gatekeeper function in some of these systems, including those in Kansas, Connecticut, and Kentucky. Other states have instituted more innovative arrangements. In Georgia, for example, a local joint planning committee comprised of ES and JTPA representatives plans services for the area. To avoid competition, they have developed a shared placement system: JTPA staff refer job ready applicants to ES for placement, while the ES refers those in need of training to the SDA for JTPA training. Incentive funds from both ES and JTPA systems are available to reward collaboration.

1.2 Trends in Program Resource Levels and Composition

The principal source of ES funding has been, since the program's inception, the payroll tax levied against employers pursuant to the Federal Unemployment Tax Act (FUTA). These taxes are deposited in an Employment Security Administration Account Trust Fund used to finance administrative costs of both the Employment Service and Unemployment Insurance programs.

Prior to the 1982 Wagner Peyser amendments, FUTA funds were used to finance all ES activities. Many had argued that, as the program's missions increased and expanded, the cost of meeting those missions was (unfairly) absorbed by operating budgets established for labor exchange activities. The 1982 amendments altered this practice by permitting the ES to charge various agencies for performing certain functions tangential to its main labor exchange mission. This change enables ES to "charge back" the cost of providing services to veterans to the Department of Veterans' Affairs, and to accept subcontracts from JTPA Service Delivery Areas to provide counseling or other services to participants in their programs. In the past several years, an increasing number of states have also begun to supplement ES funding with general revenues or special employer taxes.

Prior to the 1982 amendments, states also received their ES program budget in staff year equivalents, as opposed to dollars. Additionally, some staff were dedicated to specific functions, such as services to veterans, limiting the ability of state and local program administrators to shift resources in response to changing missions. The 1982 Wagner-Peyser amendments began to allocate resources in dollars, permitting both added flexibility in staffing and in the use of automation to leverage staff resources.

A 1991 General Accounting Office Study (GAO, 1991) indicated that roughly 62 percent of states' ES budgets were drawn from Federal Wagner-Peyser monies; 23 percent were drawn from other Federal sources via the reimbursable grant program; 13 percent from JTPA; and close to 3 percent from other state funding.

Exhibit 1 attempts to trace the actual change in ES program funding and the corresponding real dollar change since 1950[3]. This has proven a difficult task because of changes in reporting requirements attributable to the 1982 Wagner-Peyser amendments. The figures for 1950 through 1983 represent the entire ES program budget allotted to states, including funds directed at activities which later were covered under the reimbursable grant program. The figures for 1984 and beyond, conversely, represent just the ES Title III program budget; retrieving amounts contributed by other Federal agencies under reimbursable grants, or by states, has proven almost impossible.[4] Thus, comparing pre- and post-1984 figures would yield erroneous conclusions regarding changes in the ES program budget. Examination of each period's trend separately is probably more appropriate.

Exhibit 1 suggests that, in real dollars, ES funding grew most substantially in the mid-1960s, roughly corresponding to the Great Society period of expansion in human services programs. Funding then remained roughly constant in real dollar terms until the Reagan Administration's across the board cuts in Federal spending in 1982. The agency's budget was reduced by over 10 percent in real dollar terms between 1981-82.

Exhibit 1 further shows that the purchasing power of the ES has eroded considerably

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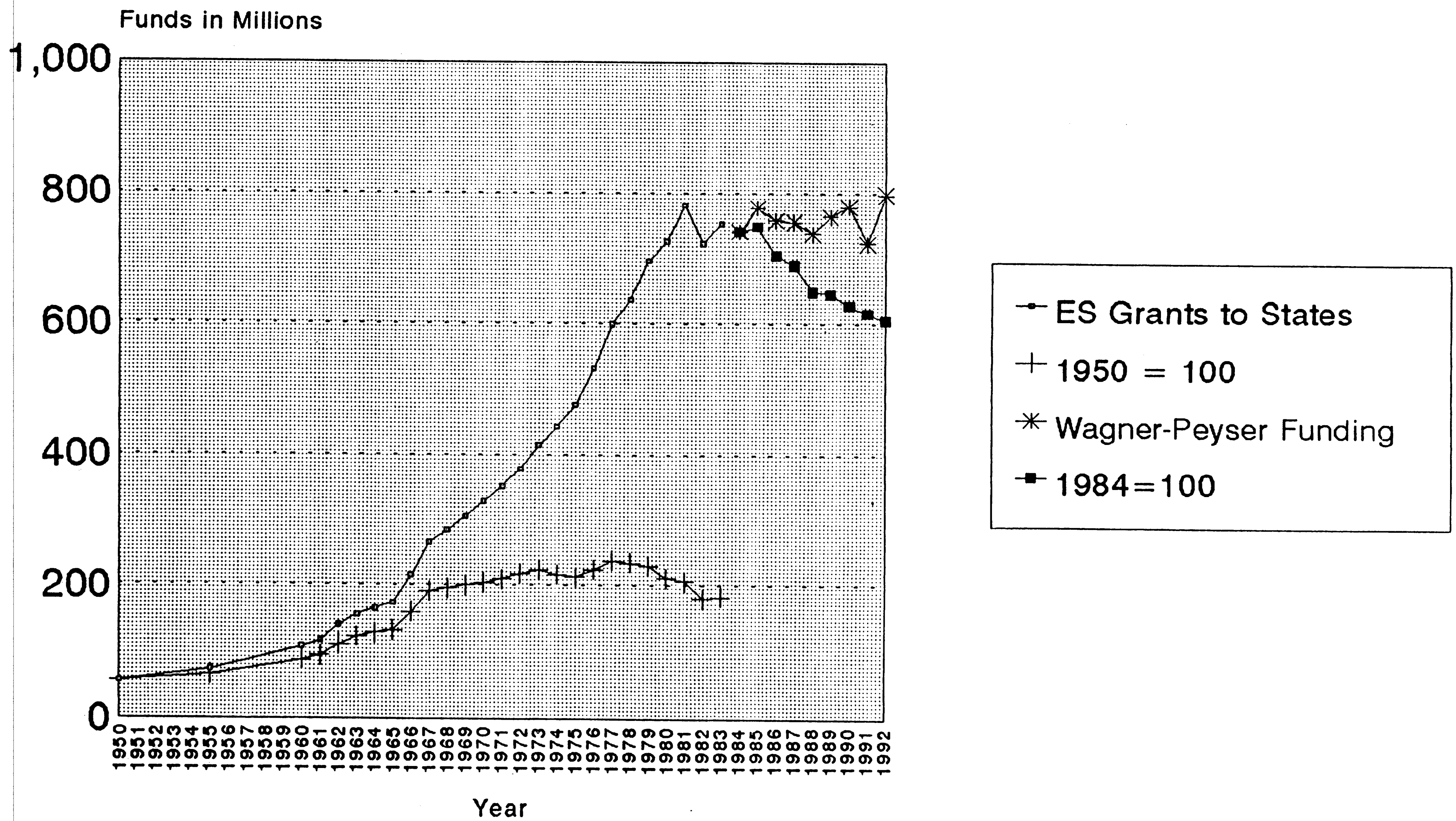
3. Data for this section are drawn from personal communications with U.S. Employment Service staff and with the Interstate Conference of Employment Security Agencies (ICESA).

4. The GAO obtained the information quoted in their report through a special survey of states. Indeed, because of changes in reporting requirements accompanying the Wagner-Peyser amendments, one would have to retrieve information on reimbursable grant funds, JTPA funds, or other state contributions to ES, directly from the states.

Exhibit 1

ES Funding by Year

Actual Funding and Real Dollar Increase



Source: U.S. Employment Service and Interstate Conference of Employment Security Agencies. Personal communication.

in real dollar terms -- by about 20 percent -- since 1984. This figure may be somewhat misleading, however, since the figures reported for 1984 and beyond represent only the ES Title III program budget. However, it does reveal declining funding for basic labor exchange services.

Largely due to budgetary constraints, the number of local ES offices has been reduced by 25 percent, from roughly 2,400 in 1981 to 1,800 in 1987. The number of staff positions has also been reduced by some 20 percent (GAO, 1991).

It has been more difficult to trace changes in ES workload during this period; data are generally available only back as far as the mid-1980s[5]. Additionally, the only measures available concern job finding services provided to registrants under the basic labor exchange function; figures are not available on other ES outputs, such as activities associated with work test administration. Nonetheless, as shown in Exhibit 2, the number of applicants for ES services has declined by about 10 percent since 1984. Figures on the number of applicants placed in jobs are more sketchy and suggest that placement rates have declined from roughly 20 percent of applicants in 1984 to about 17 percent in 1989.[6] Data are, unfortunately, not readily available on the number or fraction of applicants receiving job referrals.

1.3 A Summary of Lessons Drawn from the Review

This institutional review has highlighted a number of important changes in ES' role and function since its inception in 1933. These changes have encompassed four key areas, as summarized below.

Expansion of the number and scope of functions performed. The ES' original mission was straightforward: to assist in the matching of unemployed workers to job openings submitted by employers. Over the years, the ES has also become responsible for work test compliance, various labor certification activities, and information gathering. Some of these,

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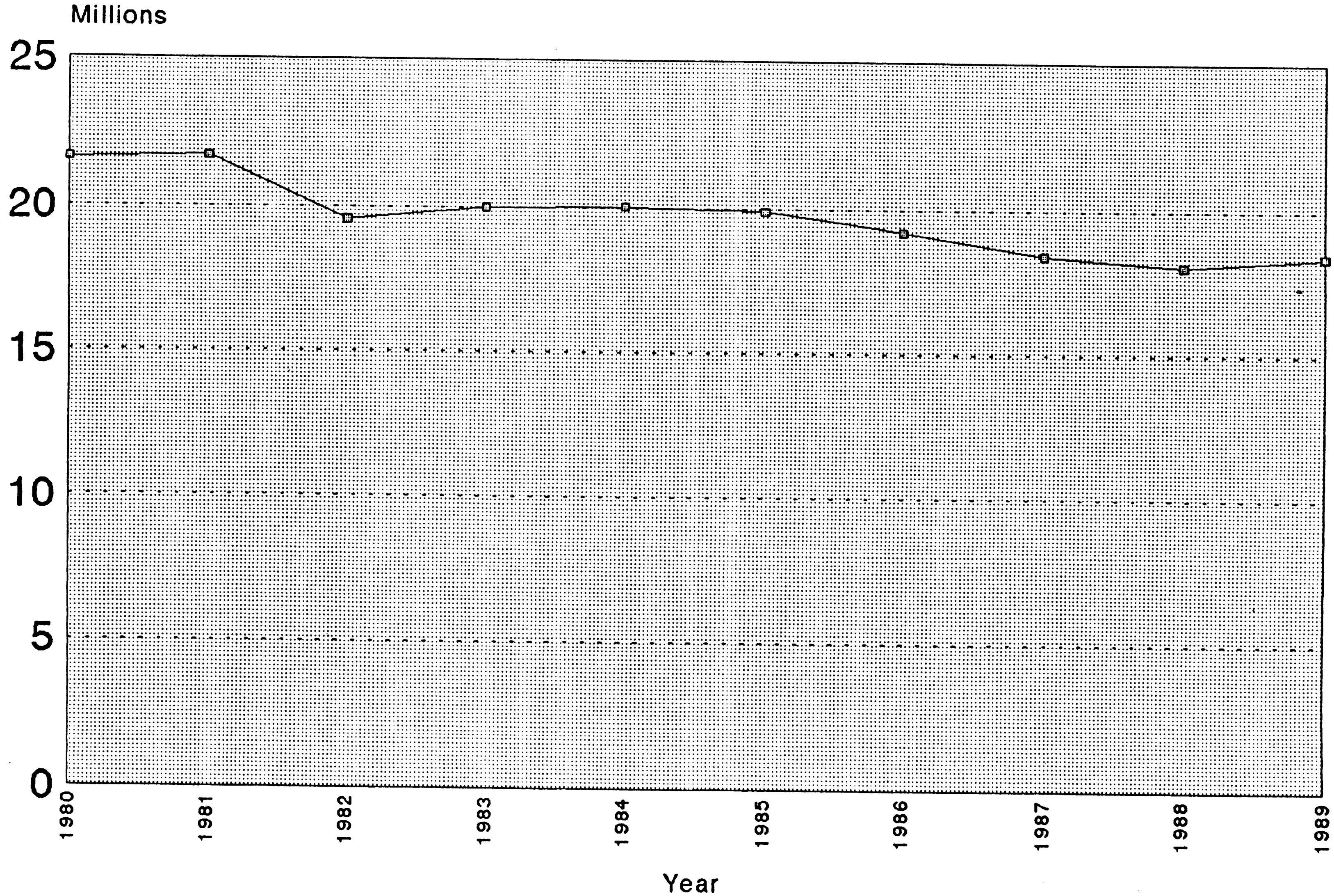
5. Data for these discussions are drawn entirely from the General Accounting Office (1989, 1991).

6. The 1984 figures are drawn from Richardson, et. al. (1985), while the 1989 figures come from the *Employment and Training Report of the Secretary of Labor* (1992).

Exhibit 2
ES Applicants by Year



Source: General Accounting Office (1989).
Personal communication.

like the collection of labor market information, are episodic and well delimited, while others, like administration of the work test, become more or less intensive as the unemployment rate rises or falls.

Changes in the individuals targeted for assistance. In its early years the ES simply served any unemployed jobseeker. Later, veterans were targeted for special assistance. In the 1960s, disadvantaged persons, youth, and older workers were emphasized. In 1988, the EDWAA amendments targeted dislocated workers for special attention. The impact of these changing emphases on the composition of the service population is not clear from available data, yet it is safe to assume that these groups have varying skills, past labor force experience, and potential for referral and placement.

Changes in the ES' role vis a vis the public employment and training environment. Until CETA was enacted in 1973, the ES was the only public employment agent. CETA supplanted that role, creating local Prime Sponsors which were responsible for a broad range of employment and training functions; ES was "refocused" on its basic mission of job referral and placement. Through JTPA and the subsequent movement to streamline human services delivery, many states have defined ES' role yet again: either as a broker/gatekeeper in a one stop shopping service delivery model; or as the agent that provides both front end screening/assessment, as well as placement services once clients are job ready.

Changes in the Federal-state partnership. ES has always operated as a Federal-state partnership. The passage of the 1982 Wagner-Peyser amendments, however, accorded state partners substantially more authority for operating the program than had been the case previously. States' authority has no doubt been reinforced by the additional financial contributions that many have made from general revenues, to operate the program.

An examination of ES program budget information suggests that there have been significant reductions in Federal resources directed at basic ES labor exchange activities since 1984: on the order of 20 percent. Over 30 states now provide additional support to the ES from general revenues (General Accounting Office, 1991), and monies drawn from other Federal sources including JTPA, constitute over one third of states' ES resources. In terms of workload the number of applicants, and the proportion placed in jobs, appear to have declined slightly since 1984.

Thus, the ES has witnessed both a great deal of change in its program mission, at the same time as there has been a declining Federal funding commitment for ES services. As Bendick (1989) notes, a sense of "unfulfilled potential" often pervades discussion of the current and future role of the ES as part of this country's human resource strategy. In the next section, we consider some of the themes and criticisms that have been raised and formulate a framework for a critical review of empirical research conducted on the Service's effectiveness.

SECTION TWO

A CONCEPTUAL FRAMEWORK FOR MEASURING THE EFFECTIVENESS OF THE EMPLOYMENT SERVICE

2.1 The Effectiveness of the Employment Service: Criticisms and Issues Affecting Improvement

In its early years the Employment Service was lauded for its role in facilitating economic recovery during the Depression, and for facilitating the smooth transition from a defense to a civilian economy in the post-World War II period. More recently, however, the agency's effectiveness has been called into question because the ES appears to play a relatively minor role in the operation of the labor market. Less than 2 percent of job openings are estimated to be filled through this means (Ainsworth, 1991). Other criticism has been prompted by reviews of the *outcomes* experienced by applicants and employers (General Accounting Office, 1982; 1989). More specifically:

- *Those who use the ES tend to be more disadvantaged than other job seekers, and turn to the ES when other job search methods fail.*

  ES applicants are typically younger than the average member of the labor force; have less prior work experience; and turn to the ES when other search methods have failed (General Accounting Office, 1982; Katz, 1978).

- *Less than half of those who register with the ES actually receive subsequent services, defined as counseling, testing, or referral to a job opening.*

  In Program Year 1989, the most recent year for which data are available, 3 percent of registrants received counseling, 2 percent were referred to training, and 41 percent received a job referral. These proportions have been relatively consistent during the past 15 years (General Accounting Office, 1982; *Employment and Training Report of the Secretary of Labor*, 1992).

- *An even smaller fraction of ES registrants actually find jobs.*

  While this proportion has varied over the years, it has rarely exceeded 25 percent. In PY 1989, 17 percent of registrants were placed. Placement rates for veterans and migrant/seasonal workers exceed the average, in part because of the ES' special emphasis on veterans services and in part because of the specialized labor market for migrant workers (*Employment and Training Report of the Secretary of Labor*, 1992).

Conversely, a lower than average percentage of UI claimants are placed. Some of the reasons advanced as explanation are that: claimants are overqualified or referred to jobs paying less than the ones they held; UI benefits discourage job finding; and that better educated claimants are more selective or have higher reservation wages (General Accounting Office, 1982).

- *The jobs that registrants obtain are often low-skill and low-wage.*

  It is frequently maintained that employers tend not to list their higher skill, higher paying positions with the ES either because they believe the Service does not attract individuals qualified to fill them, or because they fear that the candidates that are referred will not be screened appropriately (General Accounting Office, 1982). Thus, the jobs that registrants tend to fill are entry level positions in clerical and high turnover blue collar fields (Bendick, 1989; Stevens, 1978).

Others argue that, in spite of this outcome data, the ES appears to be effective in assisting a subsegment of the registrant population to become reemployed more quickly than would be the case in the absence of the program. Additionally, because the cost per individual served is so low, it is argued that ES services are cost-effective even though they appear to benefit only a small subgroup.

In response to a Congressional mandate regarding the role and functions of the Employment Service, the National Commission for Employment Policy conducted a comprehensive analysis of existing research on the agency; interviewed Federal, state and local officials; held public hearings; and conducted new empirical research on the Service's effectiveness in meeting its labor exchange mission[7]. Witnesses who testified before the Commission offered several views concerning issues which they believed were critical to ES performance. Among these were the following:

- The *proliferation of program missions* over the years, coupled with *real reductions in program funding,* have "spread the ES too thin." It is argued that

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7. See Ainsworth (1991); Romero, et. al. (1991a); and Romero, et. al. (1991b) for the Commission reports on this project.

the reductions in program funding have rendered the agency unable to address the most basic labor exchange functions -- let alone keep pace with newly added responsibilities.

- The *agency mandate to "serve all registrants" may be subordinated to the goal of meeting employers' needs for the best qualified workers.* To maintain credibility with employers, the ES must screen referrals carefully for correspondence with employer skill requirements. This may mean that not all applicants receive a referral. It may also mean that some jobs listed with the ES may go unfilled if the applicant population does not supply the required skills.

- Despite recent efforts, *coordination between ES and JTPA is still lagging in many areas, leading to duplication of effort or outright competition* and conflict between the agencies, resulting in a less than optimal use of resources.

- The *work registration mandate* for UI claimants results in a large number of individuals who are forced to register to remain eligible for benefits, but who have no interest in ES services or, in some cases, in returning to work. These individuals strain existing staff resources and, because a much smaller proportion find jobs than people who use the ES voluntarily, they make the performance of the agency look poor.

- *Employer certification requirements* also affect agency performance, because they place ES staff in conflicting positions with respect to employers. Certification requirements may place staff in the role of "enforcer" vis-a-vis the same employers that they may be soliciting for job openings.

How are we to assess the performance of the ES, and criticisms such as those outlined above? Below we outline a framework which relies heavily on the now-familiar benefit-cost analysis, but which is also sensitive to the multiple goals and outputs of the ES agency and their interrelationships.

2.2 Establishing Expectations: A Conceptual Framework for Measuring ES Effectiveness

The yardstick typically used to measure returns on investment of public funds is whether benefits obtained by society outweigh the costs incurred in providing the service. This is an appropriate question to ask of the ES as well. By the same token, the variety of functions performed by the ES, and their interaction, render a benefit-cost assessment quite complex. In view of this complexity, we begin with a behavioral model that describes the various agency functions and outputs and their interactions. We also discuss the benefits that are expected to accrue to individuals, employers, and society as a whole from these various ES activities; as well as their potential costs.

As identified in its original mandate, the principal, enduring ES function is to facilitate labor exchange via the collection of information on applicants' skills and past work experience, and referring them to suitable job openings that have been listed with the Service by employers. Applicants utilizing the ES may receive counseling and in some cases testing, and they may review job listings on their own, but they may not contact employers directly from the listings; ES staff must provide the referrals. By serving as a labor market intermediary, then, the ES is expected to influence the timeliness and quality of job matches.

This effort should yield benefits to applicants, to employers, and to society as a whole. Applicants are expected to obtain more information about job vacancies than they might obtain on their own, improving the efficiency of their job search and reducing its cost. A key indicator of such benefits is a reduction in time spent unemployed, or alternatively, a shorter time to reemployment. Individuals utilizing the ES are also expected to receive benefits indicative of the quality of the match: benefits which may be expressed in terms of employment stability and earnings. Employment stability need not be restricted to retention in the reemployment job: we also expect that ES users would spend more time employed as opposed to unemployed or out of the labor force, than nonusers. Similarly, earnings increases may arise not only from the reemployment wage itself but also from more time spent employed or a greater opportunity for wage growth.

Employers are expected to realize benefits from ES use as well. These include a reduction in the time to fill job openings; reduced costs associated with screening applicants, since the ES assumes a portion of this responsibility; reduced turnover; and possibly a reduction in training costs once the applicant is hired.

Society, in turn, obtains benefits through a reduction in Unemployment Compensation paid to the unemployed worker; through reduction in other public transfer payments; and through increased taxes paid by the workers once they are reemployed.

The chief costs of ES use for applicants are essentially the costs associated with foregone leisure, and if the reemployment wage is lower than UI benefits and/or transfers currently received, a reduction in income. The chief cost for employers is the payroll tax which supports the ES and UI programs. The government also incurs costs, in funding ES services not funded through employer payroll taxes.

As we observe from our historical review, however, a focus on the ES labor exchange activities tells only part of the story. ES also plays a role in enforcement of work test requirements, an activity which also produces benefits and incurs costs. Individuals receiving Unemployment Insurance or other income transfers must register with the ES as a condition of continued benefit receipt. They must also participate in assigned work search activities and accept suitable job referrals. Failure to do so results in disqualification for benefits, either for the duration of noncompliance or for a longer period. There is substantial interstate variation in both work registration requirements and sanctions for noncompliance (Corson, et. al., 1984).

Society realizes the bulk of the benefit from these ES activities, in the form of a reduction in Unemployment Compensation and other transfers paid. This could come about from individuals' more rapid return to work, through denial of claims for noncompliance, or through claimants voluntarily leaving the rolls because they perceive the cost of compliance as too high. Individuals, in turn, bear the costs of both compliance with requirements, as well as foregone benefits if they are disqualified.

From the perspective of individuals and society, there may be important interactions between work test enforcement and labor exchange activities. Individuals subject to these requirements trade off the costs of compliance against expected gains in income from returning to work, and against UI benefits currently received and the value of their leisure time. It is argued that the costs of compliance are sufficiently low that a large proportion of claimants put forth the minimum effort needed to satisfy requirements, and do not begin their job search until they approach benefit exhaustion. Society is the loser in this scenario, as measured by benefits paid and foregone output while workers remain unemployed.

In addition to the benefit-cost implications of the interaction between these activities, there are also efficiency implications. Because the ES does not dedicate its staff exclusively to work test or labor exchange functions, but rather deploys them based on workload and need, one might wonder whether resources are being assigned optimally to produce the greatest social benefit. This is a particularly salient question in periods of high unemployment.

Other ES functions can be expected to have much more indirect effects on workers, firms, and society. The collection of labor market information, for example, presumably benefits jobseekers and society as a whole, though the effects are likely more indirect than either labor exchange or work test functions. Labor certification activities also presumably benefit workers (by offering some protection against displacement by foreign workers) and impose costs on employers, but the effects of these activities are probably more episodic than other ES functions.

Thus, in order to assess ES effectiveness, we would like at a minimum to measure the benefits and costs of labor exchange activities and work test administration, on ES applicants, employers, and society as a whole. For individuals, appropriate measures of benefit include unemployment duration/time to reemployment; reemployment wages in relation to prior wages; job retention; and short- and long-term earnings gains. For employers, measures of benefit include reduction in time to fill job vacancies; a reduction in the cost of screening applicants; and reduced turnover. From society's point of view, benefits arise from a reduction in Unemployment Compensation and other transfers paid, and from increased taxes paid by workers once they are reemployed.

Finally, from an efficiency perspective, we also wish to consider the degree to which the current allocation of ES resources to labor exchange and work test activities, produces the greatest stream of benefits.

2.3 The Empirical Evidence: Studies Reviewed

Despite the numerous criticisms of ES performance, there are only a small number of well-done empirical studies, most concerned only with the effectiveness of labor exchange services for applicants. Because of the care with which these evaluations have been conducted, however, they provide useful evidence concerning various aspects of ES services.

Johnson, et. al. (1983) examined the impacts of ES services on individuals' time to reemployment and earnings within six months of application to the ES. For this study, ES services were defined as receipt of a referral. Outcomes for applicants receiving referrals were then compared with outcomes for applicants receiving other ES services or receiving no services at all, providing an estimate of the net impact of the referral. Benefits were assigned dollar values and then compared with the budgetary cost of providing services.

Two papers by Katz (1978; included in Romero, 1991b) also examined the effects of ES services on unemployment duration. Katz' primary interest is in understanding the timing of individuals' use of the ES in the context of their unemployment spell, and once this is accounted for, measuring the impact of the ES in reducing subsequent unemployment. The ES "treatment" in his work is defined somewhat more broadly, encompassing receipt of job search assistance, referral or placement; experiences of individuals receiving these services were then compared with those observed for applicants not receiving services. Neither of his papers provides information on the costs associated with services or of their cost-effectiveness.

The administration of work test requirements was reviewed by Corson, et. al. (1984), using agency data and qualitative information obtained through field visits. The chief focus of this study was on interstate variations in Unemployment Insurance nonmonetary determinations, though it also considered ES participation in work test activities and its effect on determinations and denials. This study points up the substantial interstate variation in work test requirements and their enforcement; it is more limited in that it is a process study and does not consider the actual impacts and costs of differing levels of requirements.

More reliable evidence of the interaction between work registration and job search activities is found in the evaluation of the Charleston, South Carolina claimant placement and work test demonstration, also completed by Corson et. al. (1985). This demonstration tested the impact and cost of enhanced work search requirements and job placement services, and as such comes closest to considering the ways in which they interact. Additionally, because it uses an experimental design, it eliminates concern about selection bias which pertain to the Johnson and Katz studies. ES applicants were randomly assigned to a control group representing existing (pre demonstration) ES services, or to one of three treatment groups, offering more rigorous work registration requirements; more rigorous requirements plus enhanced placement services; or both of the above plus job search workshops. This design enabled the measurement of net and differential effects of more stringent work test rules and expanded placement services, on sample members' time to reemployment, weeks of benefits collected, and earnings. This study also estimated the cost of each treatment activity and its cost-effectiveness.

These studies together provide useful information on the benefits and cost-effectiveness of ES labor exchange services to individuals, and to some degree, illuminate the relationship between work test and labor exchange activities. Missing from this equation, however, is information on benefits to employers of using the ES, compared with the portion of their payroll tax that funds a portion of service costs. Additionally, since all of the studies except the evaluation of the Charleston experiment consider budgeted costs rather than resource costs -- staff time and materials actually consumed in performance of a function -- the cost figures may not be an accurate reflection of effort entailed in producing outputs.

Finally, it would be useful for program targeting if the studies had provided more rigorous estimates of the differential effects of ES services for various subgroups, in particular, mandatory registrants compared to volunteers, and dislocated versus disadvantaged workers. While the Johnson and Katz studies do consider effects for subgroups, both implicitly assume that the processes and factors governing reemployment and earnings are the same across subgroups.

Before considering evidence from the studies themselves, two additional caveats need be mentioned concerning their external validity, or continued generalizability beyond the context and environment in which they were conducted. The first is that each of the empirical studies necessarily represents a point-in-time "snapshot" of the ES. Given the changes that have occurred in program emphasis and funding, the generalizability of results from these studies beyond their immediate context to the current environment, is a serious concern. When commenting on these studies, then, we will indicate known threats to their generalizability.

The second consideration is that the ES is, at the present time, a predominantly state-run program. With the JTPA amendments of the 1980s and the increasing predilection of states to use their own general revenues to support program activities, the concept of a nationwide, reasonably uniform ES system has diminished greatly. This has implications both for expected variations in program effectiveness, as well as for the range and nature of policy or program interventions which might be implemented.

SECTION THREE

EFFECTS OF ES SERVICES ON EMPLOYMENT, EARNINGS AND UI BENEFITS RECEIVED

This section considers the empirical evidence regarding ES impacts on workers' time to reemployment, earnings, and for those who are UI claimants, various measures of benefits paid. Effects of ES labor exchange services -- counseling, job search assistance, referral and placement -- are considered first. Then we examine effects of ES involvement in work test enforcement.

3.1 Effects of ES Labor Exchange Services

The *National Evaluation of the Impact of the United States Employment Service*, completed in 1983 by Johnson, et. al. (1983) is the only nationally representative study which considers effects of ES labor exchange services on recipients' employment, wages and earnings. The goals of this study were to estimate the short term benefits of ES services on registrants' employment, wages, work hours and earnings, within six months following application; and to compare these to associated costs to arrive at an overall assessment of program cost-effectiveness. The study's authors also sought to measure the influence of office and site attributes on ES performance, in an effort to understand how such administrative and environmental factors affected outcomes.

To assure that results were generalizable to the national population of new ES applicants, a sample of 8,000 individuals applying for ES assistance between July 1980 and May 1981, in one of 30 offices in 27 states, was selected. This was a self-weighting sample, with each individual's probability of selection proportional to the volume of new applicants in each office. Equal numbers of sample members were selected in each site, for optimal sample power.

These 8,000 sample members received a baseline survey interview once they had applied for ES assistance, but before they had had additional contact with ES counselors or placement interviewers. This interview covered two years' worth of prior labor market experiences, demographic characteristics and family status, and information on current job search activities, including expectations about use of the ES. Importantly, the interview also gathered information on motivation, work ethic, and work attitudes, as research had suggested that these attributes affected both an individual's willingness to seek assistance and their prospects for reemployment.

Those who were interviewed at baseline were interviewed again six to nine months following application. This followup interview gathered information about services received from the ES and other sources; and about individuals' job search and reemployment experiences.[8]

Data on receipt of ES services was also gathered from Employment Service Automated Reporting System (ESARS) records for sample members. Because records could not be retrieved for five of the 30 sites, analyses were restricted to the subset with complete data.[9]

The most reliable means of measuring program impacts -- an experimental design involving random assignment of applicants to service and no-service groups -- was not feasible for this study. The stipulation that the ES services had to be available to all who desired them, was said to preclude such an approach. Researchers were thus forced to rely on nonexperimental methods, involving a comparison group, to estimate impacts. The approach ultimately adopted involved comparing outcomes of those applicants who received ES referrals, with outcomes for a comparison group composed of similar applicants who did not receive a referral. Comparison group members could receive other ES services, or could have applied but failed to receive a service.

Extensive efforts were made throughout the study to assure the comparability of

8. The followup interview response rate was 68 percent, and it appears that this was driven by a higher than expected percentage (11 percent) of out of area movers who could not be tracked and interviewed. Various tests for attrition bias suggested that this was not a serious threat.

9. Researchers were quite concerned about the sensitivity of results to exclusion of these five sites. A variety of attrition analyses suggested that the five excluded sites had a higher proportion of mandatory registrants than the others, but were otherwise quite similar on measured characteristics. The researchers also found that, in comparing the 25 remaining sites to the national distribution of offices, they provided a better match than if the five with missing records had been included. See Johnson, et. al. (1983).

treatment and comparison groups, and to detect and address any unmeasured differences between them that might confound estimates of program impact.[10] Extensive data were gathered on individual and agency attributes that are typically unmeasured but which may affect service receipt and outcomes. These included collection of data on: sample members' motivation and work ethic; characteristics of job orders received by each office, to test the hypothesis that those who received referrals did so because of a match between their characteristics and job requirements; and information on agency practices in making referrals. Various specifications of impact models utilizing this information, as well as individuals' prior earnings histories, were estimated with varying degrees of success.

Before summarizing these results and discussing opportunities for their extension, it is useful to consider characteristics of the research sample[11]. One of the most striking observations is its youthfulness. While the average age is 29 years, 60 percent of this sample is under age 30 and 44 percent under age 24; the age distribution of male and female sample members is generally similar. Also, twelve percent of women and seven percent of men had never worked in the past. Among those who had worked, 30 percent of the women and 21 percent of the men had three or fewer years of work experience. Only 19 percent of those with prior work experience had been out of work less than one week before applying at the ES. Forty percent were mandatory registrants.

It is also useful to consider the proportion of the sample that received various services from the ES. Data from ESARS records for sample members suggest that roughly 30 percent received a job referral: the indicator of ES treatment for purposes of the study. Eleven percent received job development services, and fewer than 5 percent received

10. The concern, well known in the literature, is that individuals who receive services differ from their no-service counterparts in ways that affect both their receipt of services and their eventual labor market outcomes. If the groups differ only in terms of *measured* characteristics, including these attributes in an analysis-of-covariance regression model will yield unbiased estimates of program impacts. But this is rarely, if ever, the case. Whether an individual receives services is likely to be affected by both individual and agency decisions, and to reflect both measured characteristics as well as *unmeasured* ones (motivation, appearance, etc.). Failure to account for such unmeasured characteristics can confound estimates of program impact with the effects of these characteristics, resulting in biased estimates.

11. Information is presented separately for men and women throughout the study.

counseling, testing or other support services. The majority of those who received a referral received exactly one referral, with half receiving it within one week and two thirds within one month. Sixty percent received a referral in their primary occupational classification. Additional information from the study's followup survey suggested that 87 percent of those who received a referral followed up on it by contacting the employer or interviewing for the job; 40 percent were offered a job; and 31 percent of these accepted.

These analyses suggest the presence of a variety of formal and informal selection processes. The fact only 30 percent of applicants received a job referral may be indicative of a mismatch between the skills offered by applicants and those specified in job orders placed by employers; it may also be indicative of extensive screening by ES staff based on observable characteristics as well as unobservables, like motivation or attitude. Analyses confirmed that very few demographic or work history characteristics influenced referral receipt. Indeed, the strongest predictors of referral receipt were indicators of motivation as measured by hours spent per day in job search, reservation wage and work orientation; veteran status; and site or agency characteristics.

Importantly, mandatory registrants were also significantly less likely to obtain a referral than volunteers, net of other indicators of motivation and work orientation. This may mean that they are, indeed, perceived by ES staff as less interested in finding employment, or that they convey this impression explicitly.

Johnson et. al. (1983) then consider the effect of ES referral on time to reemployment, percent of time spent employed, hours worked, average wage rate, and earnings. Each was measured for a six month period following the date on which the individual applied at the ES. Because this approach yielded little followup data for those referred late in the six month period, the authors also estimated impacts during the post-referral period for individuals who received a referral within a month after application; and then again for those who had received a referral within three months of application. Again, to account for differences in labor force behavior of men and women, separate estimates were made for each group.

Impact results have been summarized in Exhibit 3. The table reports mean differences between the referred and not referred groups, separately for men and women.

Exhibit 3

Summary of Main Impact Findings from The National Evaluation of the Employment Service

Measure	OLS Estimates						Selection Models			
	Impact of Referral within Six Months of Application		Impact of Referral in First Month on Post Referral Outcomes		Impact of Referral in First Three Months on Post Referral Outcomes		Maddala-Lee		Heckman	
	Men	Women	Men	Women	Men	Women	Men	Women	Men	Women
Earnings in 6 months	-98	325***	242	638***	-14.45	219.6**	4595***	155	3947***	275
Weeks till reemployment	-.64	-2.79***	-1.98***	-4.61***			-10.4**	-7.62*	9.02**	-6.25*
Percent of 6 month period unemployed	1.36	1.14	-3.86*	-8.39***						
Percent of 6 month period out of labor force	-1.51	-7.13***	-1.67	-5.71***						
Percent of 6 month period employed	.15	8.27***	5.53***	4.11***						
Hours worked	-7.48	88.3***	50.9**	157.8***						
Average wage	-.12	-.18	-.16	-.17						

*** Significant at 1% level.
** Significant at 5% level.
* Significant at 10% level.

Source: Johnson, et. al. (1983).

The table also reports the basic ordinary least squares estimates along with estimates from two efforts to correct for potential selection bias.

The OLS results suggest sizeable and significant effects of ES services for women, and no effects for men. Women who received ES referrals reduced their time to reemployment by close to three weeks, and increased their earnings by roughly $325 in the six months following application, compared to the not-referred group. These earnings results arose because women spent more of the six month period working and less time out of the labor force; there was no effect an ES referral on their average wage rate. The effects for women also appear to persist over time. An examination of the proportions of treatment and comparison groups employed at the end of the six month period indicated that those who received ES services still held an advantage.

A comparison of the effects of a referral received in the first month after application[12] produced estimated impacts that were considerably more positive, especially for men. A corollary analysis which considered effects of referral in the first three months, on outcomes in the subsequent three months, yielded results consistent with the first OLS estimates, that is, positive effects for women and zero effects for men. These estimates are also summarized in Exhibit 3.

Because of persistent concerns about unmeasured differences between the referred and not referred groups, the researchers used two methods to adjust for potential selection bias. One involved the familiar Heckman correction and the other, following Maddala-Lee, included the predicted probability of receipt of a referral as a right hand side variable. Results from the two estimations are also shown in Exhibit 3.

For men, both selection models suggest that the OLS models substantially underestimate effects of ES referrals on both weeks till reemployment and earnings in the six month period after application. The magnitude of the estimates, particularly for earnings, are quite troublesome and not believable: the estimated effect is more than twice

12. It seems that the authors were unable to estimate impacts of the referral in the five month period following its receipt, given the manner in which their survey collected earnings information. It appears instead that they estimated impacts for the six month period following application, which as the authors point out, includes at most one pre-referral month. See Johnson, et. al. (1983).

the sample mean and exceeds mean earnings for the referred group. Nonetheless it appears the case that simple OLS models contain a negative bias.

For women, the opposite appears true, as both models yield smaller but still positive effects on earnings, none of which is statistically significant. For women OLS models appear to overestimate ES effects on earnings. Oddly, the selection models also estimate a larger reductions in weeks till reemployment -- a result that appears highly inconsistent with that obtained for earnings.

How are we to interpret these results? Are there areas in which they are particularly vulnerable?

A first such area concerns the conceptualization of the ES treatment as referral. While this seems quite appropriate in view of the ES' mission to facilitate the *matching* of workers to jobs, it ignores the potential effects of other ES job finding services. It is possible that these other services, particularly any job search assistance, may have led to self-placements, and by ignoring them we are potentially underestimating the effect of ES services on employment. The authors test this proposition by estimating a model in which these services are allowed to have independent effects, but the power of their sample is small and results inconclusive.

A second area concerns the conceptualization and measurement of outcome variables. Most measures are taken for the six month period following application -- a decidedly short term view. Is this appropriate? For indicators such as time till reemployment or percent of period employed, we might argue that it is: if ES' essential service is to match workers to job vacancies, then this is an activity in which timeliness of referral and followup by the applicant is of the essence lest the job be filled by some other individual. Hence, we should expect ES effects on employment to show up fairly quickly. By the same token, a referral cannot have an impact until it is made: we feel that it is more appropriate to measure effects during a true "post referral" period, as Johnson and his colleagues attempt.

One outcome measure is, surprisingly, missing from the list of those investigated: the proportion of sample members in each group who *ever* found employment in the post application period! The authors report that 11 percent of sample members were recorded in ESARS records as receiving an ES placement, but the fraction who found employment -

- whether via an ES placement or on their own -- is not reported. In interpreting ES effects on return to work and earnings, then, we do not know whether a substantially greater fraction of the referred group was reemployed, or whether the reported effects are really large effects for a small subgroup of those referred[13]. Additional analysis in this area would shed more light on the manner in which ES effects arise.

The most puzzling feature of these results is the attempt to correct for potential selectivity bias in the probability of receiving a referral. Both selection models indicate that OLS substantially underestimates ES effects on men's earnings, and there is strong evidence that selectivity bias is indeed present. Results from estimation of impacts in the post-referral period also suggest strong evidence of selectivity bias for men: males receiving referrals within a month following application had higher preapplication earnings than those receiving referrals within three months following application or at any time during the period.

For women, however, the authors conclude that there is no strong evidence of selectivity bias although the various corrections suggest that OLS overestimates impacts on earnings and underestimates impacts on time to reemployment. As we hinted earlier, the fact that the bias appears to move in different directions for these two outcome measures is odd. One explanation might be that ES hastens reemployment for women, but with no effect on job stability and hence, no effect on earnings. This hypothesis could be tested with available data, however, since the survey also measured the proportion of the followup period that was spent in employment, unemployment, or out of the labor force.

Also puzzling are the opposite biases in estimates for men and women. If the bias was driven by, say, failure to account for people who never were reemployed during the observation period, one might expect the bias to move in the same direction for men and women.

While the authors ultimately adopt a conservative stance in deeming the OLS results most reliable, one is left wishing additional analyses were conducted to shed further light on these issues. Such additional analyses would include: examination of the proportion of men

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13. This is suggested in a later analysis in which the group of referrals who also receive ES placements are shown to account for most of the employment and earnings effects. See Johnson, et. al. (1983).

and women ever employed during the followup period; additional investigation of timing of reemployment in the post-referral period; and more analysis of effects for different subgroups of men and women.

With respect to the latter issue, it would be useful to estimate separate models for mandatory versus voluntary registrants, for both women but especially men. In the various probit models estimating probability of referral, mandatory registrants are significantly less likely to receive one, even after controlling for a variety of other indicators of motivation. This may be because the underlying processes and factors governing job search behavior of mandatory versus voluntary registrants, indeed differ, just as the literature suggests. Before estimating a model in which these observations are pooled, this proposition can and should be tested. It may be that the ES does have effects for men who volunteer, but these are eclipsed by the lack of effect for mandatory registrants.

The national evaluation concludes with the suggestion that benefit-cost calculations for ES services are very favorable. The average cost per referral was quite low -- estimated at $81 using the highest cost assumptions -- while the most conservative estimate of program benefits, averaged across men and women, was $146, yielding an extremely favorable benefit-cost ratio of 1.8:1.

Two papers by Katz (1978; included in Romero, 1991b) shed some additional light on the processes by which ES assistance may affect users' reemployment. The first paper analyzes the timing of individuals' use of the ES, and its effects on individuals' time till reemployment. The second paper builds on this work, using longitudinal data on over 16,000 dislocated workers who lost their jobs at some point between 1978 and 1987. We discuss both studies in turn below.

Katz' first paper examined data from the 1972 Bureau of Labor Statistics Job Finders Survey (JFS), and a data set obtained by merging ESARS and Continuous Wage and Benefit History (CWBH) records for a sample of ES applicants in the state of Pennsylvania. The paper had two main goals: to understand the determinants of ES use, and to estimate the contribution of the ES to individuals' subsequent time till reemployment.

For the former analysis, Katz relied on the JFS, which provides a great deal of information on individuals' job search activities. The analysis sample was restricted to white males who were not required to register with the ES as a condition of benefit receipt. Approximately 27 percent of sample members actually registered with the ES, a much lower percentage than used other sources -- such as direct application, family and friends, or private agencies -- to find a job. Katz provides reasonable evidence that those who turn to the ES do so as a "backstop" strategy when other avenues are exhausted. He suggests that, as job search lengthens, searchers are increasingly more likely to turn to secondary sources of assistance. Of those who found jobs through secondary channels rather than primary ones, 44 percent were ES users. Conversely, a much smaller fraction of those who began searching while on the job, actually turned to the ES.

Based on this analysis Katz suggests that, because individuals turn to the ES as a "backstop" only after some period of unsuccessful job search, it appears that ES users search longer for jobs when in fact what we are measuring is the delay in ES utilization. He argues that the methods by which an individual finds work come to be jointly determined with length of search, inducing a bias in the estimates of ES effect. He further argues that the critical issue is whether ES increases the speed of becoming reemployment compared to non-users, *at comparable points in the search process.*

Katz is extremely sensitive to the fact that ES users tend to self-select into the "ES user" population. To address this he estimates a two stage maximum likelihood model which first predicts the point in the unmeployment spell at which an individual switches from a primary to secondary search strategy, based on the frequency with which applicants with the same demographic and work history characteristics are observed to use or not use the ES (Katz, 1978, p.23). An increase in the value of this "switch point" reduces the probability that job searchers, having searched for any given length of time, will make use of the ES. Results from the first stage estimation show that low wage, low skill individuals turn (as might be expected) more quickly to the ES, while on the job searchers and voluntary quits delayed longer in using the ES.

The second stage estimation considered mean search times till reemployment for individuals employing primary and secondary strategies. A simple comparison of these means would be biased, because individuals self-select into a secondary search strategy based on observable characteristics and search experiences using primary channels. Once adjustments are made for this self-selection, Katz finds that those who switch to an ES job finding strategy have significantly lower times to reemployment than people with comparable observable characteristics who stick with their primary strategy. In fact, he estimates that ES reduces search time by approximately 50 percent over what would have been expected in the absence of the program.

The second paper by Katz (included in Romero, 1991b) builds upon this work. It draws upon a 5 percent sample of UI claimants, residing in Pittsburgh or Philadelphia, who lost their jobs at some point between 1979 and 1987. The sample was further restricted to individuals who: received Unemployment Insurance benefits, who had more than three years of work experience prior to becoming unemployed, and who were not at work in the quarter immediately prior to applying for UI benefits. Sample members thus bear more resemblance to dislocated, rather than disadvantaged, workers.

Because the study's ultimate focus is on these individuals' time to reemployment, information on the first completed spell of continuous joblessness (measured in quarters) after layoff is gathered from Unemployment Insurance records for each sample member[14]. Also gathered is work history and earnings information, in some cases dating back to 1969, for the period prior to layoff; this information was obtained from Unemployment Insurance wage records. Data on ES use and services received prior to, and during, the layoff was gathered from the Employment Service Automated Reporting System (ESARS). The resulting data base represents a rich, longitudinal profile of sample members' work history, earnings, and unemployment experiences.

The study seeks to measure the impact of the ES on time to reemployment by comparing outcomes for dislocated workers who received ES assistance ("ES users") and those who do not. ES use, the treatment indicator, is defined as "receipt of a job referral, placement, or other job search assistance service," a slightly broader definition than the one used in the *National Evaluation*, which considered only ES *referrals*.

Despite this broader definition, however, a smaller fraction of Katz' sample -- 20

14. There were 16,420 such periods of joblessness for sample members. Ninety one percent of sample members had completed spells of joblessness after the layoff event. Sample members with uncompleted spells generally retired from the workforce and thus were dropped from the analysis.

percent, compared to 30 percent in the *National Evaluation* -- were ES users. Some of this may be due to the nature of the sample: experienced workers with a solid work history might be assumed, a priori, to be more apt to find jobs through their own networks, than more disadvantaged people. An alternative explanation is that there are no mandatory registrants in Katz' sample. Because Pennsylvania does not require work registration as a condition of UI receipt, use of the ES is strictly voluntary on the part of claimants.

Unlike the *National Evaluation*, where ES users and nonusers were generally similar on measured demographic and work history attributes, this study finds them quite different with respect to employment and unemployment history. Specifically ES users had lower pre-unemployment average quarterly earnings than non-users, and were unemployed longer during the spell in question: an average of 5.5 quarters versus 3.1 quarters for non-users. A much larger fraction of ES users also exhausted their UI benefits: 68 percent versus 32 percent of non-users. This suggests, if anything, that ES users were more disadvantaged than their counterparts.[15]

To assess factors associated with ES use, the study estimates OLS models of the probability of receipt of ES services as a function of characteristics of dislocated workers in the sample.[16] The most significant predictors are related to income support: "weeks drawing unemployment benefits," "weekly benefit amount," and "whether UI benefits were exhausted." An increase in weekly benefit amount decreased the probability of ES use, while increases in weeks drawing benefits and ultimate benefit exhaustion increase the probability of use. Based on these results, it seems apparent, as Katz suggests, that the probability of ES use increases gradually as benefits are drawn down. It appears that dislocated workers use the ES as a "backstop" source of employment assistance, turning to it as other search methods prove unsuccessful. Indeed, the average delay in turning to the ES was roughly two

15. These figures pertain to the full sample. Results are not systematically presented separately for men and women, a significant omission in view of differences in their labor market behavior, and in view of results from the *National Evaluation*, which suggest impacts for women but not for men.

16. As noted earlier, both men and women are included together in the estimation, with a dummy variable to capture the effect of being female. This assumes that the functional form of the predictive model is the same for both men and women -- an assumption which can, and should, be tested empirically before pooling the observations.

quarters after layoff.

Because ES users appear more disadvantaged than non-users based on their prior employment and earnings history, and because it seems apparent that users delay turning to the ES until (a) they are close to exhausting benefits or (b) their own job search efforts have proven unsuccessful, there seems to be strong evidence of self-selection. This raises the obvious question of the comparability of ES users and non-users for purposes of impact estimation. Unfortunately, the paper does not fully explore this issue, leaving open questions concerning the validity of the impact estimates presented.

To be fair, because it relies solely on administrative records, this study has fewer tools available than the *National Evaluation*, which gathered survey data on sample members' motivation, work ethic, and agency selection processes; these variables could then be used as controls in predictive models of ES utilization. On the other hand, this study did have access to a rich longitudinal data set containing extensive information on workers' prior employment and earnings history, information which could be employed to control for unobserved differences between users and non-users.

That said, Exhibit 4 summarizes the main results of Katz' analysis. Comparisons are made between ES users and non-users with respect to the number of weeks they remained

Exhibit 4
Mean Difference in Weeks Remaining Jobless: ES Users and Non-Users

	Difference in Weeks Remaining Jobless if Used ES After:		
	1 Quarter	**2 Quarters**	**3 Quarters**
Philadelphia	-2.6	-10.7**	-10.9*
Pittsburgh	-1.5	- 7.1**	-12.2**
Men Formerly Employed in Manufacturing and Exhausting Benefits	-4.8	- 9.0*	- 7.6

* Significant at 5% level.
** Significant at 1% level.
Source: Katz, included in Romero (1991b), p. 21.

unemployed[17]. As in his previous paper, Katz also controls for the delay in ES use. ES users and non-users are compared at a comparable period in their unemployment spell, and the analytic question thus becomes whether ES users experience fewer weeks of subsequent unemployment than non-users, conditional on having remained unemployed up to this point.

The exhibit suggests that the ES reduces the remaining unemployment duration for all users, with the reductions most dramatic for those who wait the longest before applying. Differences between ES users and non-users are on the order of 2 weeks for those who delay ES use for a quarter after unemployment. The difference increases to close to 10 weeks -- and becomes statistically significant -- for those who delay use until two or three quarters after unemployment. These results seem intuitively appealing since the logical counterfactual for those who delay seeking job search help until they have been unemployed for quite some time, is probably prolonged unemployment.

Taken together, both of these papers offer valuable insights concerning the process by which ES is expected to influence applicants' reemployment. The first paper finds that individuals tend to delay ES use, treating the agency as a "backstop" source of assistance. The second paper finds, in turn, that some of the more significant predictors of delay in use concern remaining duration of UI benefits and the benefit replacement rate (fraction of earnings replaced by UI); these results are fully consistent with past work which addresses disincentive effects of such social insurance programs. Both papers suggest that, once the delay in use is controlled for, ES users have significantly shorter waiting times till reemployment than comparable non-users.

The major concern with this work, especially with the second paper, is the adequacy of controls for unmeasured differences between ES users and non-users. The earlier paper appears to use a two stage, Heckman-style maximum likelihood estimation to address possible selection bias, while the second paper appears to control only for measured differences using a regression framework. Simply comparing times to reemployment for users and non-users, while accounting for delays in ES use and differences in measured

17. Again, results are not presented separately for men and women, limiting our interpretation of the findings.

characteristics, does not assure estimates that are free of bias if the groups still differ in terms of unmeasured characteristics. An informative extension of this work would thus entail corrections for possible selectivity bias.

Another important issue concerns estimation of effects for subgroups. Most obvious in this regard is the estimation of separate effects for men and women, especially in view of the findings from the *National Evaluation.* Also of interest would be separate estimates for mandatory versus voluntary applicants, as both papers essentially restrict analysis to voluntary applicants.

3.2 Effects of ES Involvement in Work Registration Activities, and their Relationship to Labor Exchange Services

As we have suggested earlier, ES involvement in the administration of work registration requirements also yields benefits to society while imposing costs on individuals, employers, and the government. There are also likely to be important interactions between ES involvement in work registration and labor exchange (job search) activities -- interactions which raise questions concerning the efficiency of investment in each. The bottom line is whether the current allocation of effort to work registration versus job search, yields the greatest benefits to society in view of their costs.

There are presently no empirical studies which tackle this important question directly. Two efforts address pieces of the puzzle, however. The first, conducted by Corson, et. al. (1984) considers interstate variation in nonmonetary eligibility determinations in the Unemployment Insurance program. In the course of this work the relationship of ES activities to benefit denials is considered, and so it is useful to review these findings.

A second study, also conducted by Corson et. al. (1985), considers the relationship between work registration and job search activities more directly. Importantly, this is the only study reviewed which uses an experimental design, eliminating worries about selection bias and the comparability of treatment and comparison groups. The study considers net and differential impacts of different combinations of work registration and job search activities, on ES applicants' unemployment duration, employment, earnings, and probability of being denied UI benefits during their claim.

We discuss results from each study in turn.

The first study considered (Corson, et. al., 1984) investigates the influence of state laws, regulations, and procedures on nonmonetary eligibility determinations in the Unemployment Insurance system. Such determinations are of two forms:

- *Initial nonmonetary eligibility for benefits,* codified in each state's law and regulations. These criteria concern aspects of an individual's separation from his or her job, which render him or her eligible or ineligible for benefits.

- *Continuing eligibility for benefits*, which depends on demonstration of ability to work and availability for work, as well as absence of refusal to accept available/suitable work. The first two lead to denial for the length of the noncomplying period. The third leads to denial for all or part of the benefit period.

The second element is pertinent to our review, since it encompasses the involvement of ES staff in registering claimants for work and in providing suitable job referrals.

Corson, et. al. were primarily interested in the manner in which both types of determinations were influenced by state laws and local agency practices. Cross section time series data covering all 50 states for the period 1964-1981 were analyzed to assess the relationship between denial rates and explanatory variables reflecting state UI laws, nonmonetary eligibility rules, maximum benefit levels, and descriptors of the economy. Then, qualitative data were gathered and analyzed for six states with extremely high or low variations from predicted denial rates using the regression model[18].

These analyses offer a number of important observations concerning nonmonetary eligibility determinations. Most salient are the following:

- The frequency with which determination issues were detected depended not only on stated eligibility criteria, "but on a wide range of administrative guidelines and procedures that may vary from office to office in their application, and that may be adhered to closely or loosely depending on available staff resources, the pressure of claimant traffic, and the level of administrative review/control."

18. Within each state one rural and one urban office were selected. Data were gathered from structured interviews held with state and local program administrators.

- Practices which aid in the detection of potential continuing eligibility issues included a formal requirement that claimants engage in work search and clear specification of evidence required to document compliance with work search requirements.

- More severe disqualification penalties seemed to affect the behavior of claimants and potential claimants; the denial of benefits for the duration of the unemployment spell, as opposed to a shorter period, had a deterrence effect for most issues. Additionally, the severity of the sanction also influenced the proportion of eligibles who actually applied -- weeding out those whose behavior would have likely resulted in denial.

The first suggests that variations in work registration requirements are not simply attributable to variations in laws "on the books," but also to variations in agency practices and behaviors. Thus, when one considers the effects of the "work registration requirement" one must keep this variability in mind and acknowledge that the resulting service population may be a function of agency selection processes that may well change in response to workload pressures. The second and third point out the deterrence effects of clear requirements and strong sanctions for noncompliance; not only do we expect effects in terms of benefit denials, but also in terms of foregone applications for those who would likely be found noncompliant.

More useful in examining interactions between work registration and job search activities is the evaluation of the Charleston, South Carolina Claimant Placement and Work Registration Demonstration, also completed by Corson, et. al. (1985). This study examined how increased enforcement of the work test, coupled with enhanced ES services, affected the reemployment of UI claimants. Of all of the studies discussed in this paper, this was the only one which used an experimental design involving random assignment to treatment status. Thus, concerns about selection bias, which pervade interpretation of the results of the Johnson, et. al. (1983) and Katz (1978; included in Romero, 1991b) studies, are not present.

The Charleston demonstration operated between February and December, 1983. All UI claimants receiving a first payment in this period, except for mass layoff claimants whose employers considered them subject to recall, were randomly assigned to one of four groups:

- Treatment Group 1, which received a strengthened work test, plus enhanced placement services and job search workshops;

- Treatment Group 2, which received the strengthened work test plus enhanced placement;

- Treatment Group 3, which received the strengthened work test alone; or

- the Control Group, which received no demonstration services. Additionally for control group members, existing work registration requirements were waived. Claimants could, however, use the ES voluntarily. Claimants were required to comply with existing UI work search requirements, however.

Approximately 1,500 claimants were assigned to each of the four groups.

The strengthened work test involved two components: changes in ES registration procedures, and procedural improvements in communication between ES and UI. With respect to registration, new claimants were given a definite date by which registration was to be accomplished, and this was monitored through computerized checks of ES and UI files. If claimants failed to register by this date a nonmonetary determination was issued and the claimant denied benefits for the duration of noncompliance.

Enhanced placement services entailed a placement interview, accompanied by a job referral or job development attempt. Claimants receiving these services were also taught to use job information listings and automated job matching services. If not reemployed nine weeks later, these claimants were subject to a second call-in for additional enhanced services.

Job search workshops entailed a three hour curriculum geared at improving claimants' job search skills. Individuals in the treatment group which received this service were called in during the fourth week of their claim to attend the workshop.

Given this design, a comparison of outcomes for treatment group member, and for treatment group members versus control group members, would yield estimates of the net and differential impacts of various combinations of services. Three categories of outcome measures were examined, including the proportion of claimants experiencing benefit determinations and denials; employment related outcomes, including individuals' employment status at three and six months after ES registration and their earnings; and the duration of their UI claim. These are summarized in Exhibits 5 through 8 below.

Exhibit 5 summarizes the proportion of each group who were subject to a determination or were denied benefits for noncompliance. The data show, as one might expect, that a larger proportion of those assigned to each of the three treatment groups experienced a determination, and that a larger fraction of claimants in each treatment group were in fact denied benefits for noncompliance. The percent of determinations that resulted in denial, however, was actually higher for control group members; Corson, et. al. (1985) speculate that this was because treatment group members who misunderstood requirements may have been given a "second chance" to comply.

Not surprisingly, most of the determinations were for failure to respond to the initial work registration requirement imposed by the strengthened work test, and most of these occurred within four weeks of the effective date of the claim. The requirements to appear at the job search workshop, or to receive the "second wave" of enhanced placement services, did not result in a significant number of determinations.

On the other hand, between 22 and 36 percent of treatment group members received a nonmonetary determination for reasons apparently unrelated to the demonstration. This could encompass such things as failure to comply with UI work search requirements, unavailability for work, or failure to accept suitable employment.

Exhibit 6 summarizes demonstration impacts on various indicators of employment and earnings. The data are presented in two panels. The first simply presents the unadjusted mean value of each outcome measure for treatment and control group members; in the case of binary outcome measures, the proportion of group members with each outcome is presented. The second panel provides regression coefficients capturing effects of the different treatments on various outcomes of interest; estimates are made separately for men and women[19].

19. These are not as facile to interpret as adjusted treatment-control mean differences. Control group means for the independent variables included in the regression model are given in the text, but not separately for men and women. See Corson, et. al. (1985) page 27.

Exhibit 5

Nonmonetary Determinations, Denials, and Source/Timing of Determination by Treatment Group

Outcome	Treatment Group 1	Treatment Group 2	Treatment Group 3	Control Group
Percent of Claimants with Nonmonetary Determination	18.4	16.7	13.4	5.8
Percent of Claimants with Denial	8.7	9.2	7.4	4.2
Percent of Determinations Resulting in Denial	47.1	54.9	55.3	71.7
Source of Determination				
First ES Call-in	50.6	59.0	64.3	0.0
JSW Call-in	16.6	0.0	0.0	0.0
Second ES Call-in	11.1	12.2	0.0	0.0
ES-UI Communication	0.4	0.4	0.0	0.0
Other	21.7	28.4	35.7	100.0
Date of Determination Relative to Claim's Effective Date				
0-2 Weeks	46.7	40.4	43.3	47.6
3-4 Weeks	35.4	42.3	38.7	31.4
5 or More Weeks	17.9	17.4	18.1	21.0

Source: Corson, et. al. (1985), Tables III.6 and III.7.

Exhibit 6
Demonstration Impacts on Employment and Earnings

Outcome	Treatment Group 1		Treatment Group 2		Treatment Group 3		Control Group
Percent of Claimants with Long Term Placement	9.8*		11.2*		7.5		6.5
Percent of Claimants with First Quarter Wages	53.5		53.9		55.0		52.0
Mean First Quarter Wage	$1,912		$2,077*		$1,951		$1,925
Percent of Claimants with Second Quarter Wages	65.8*		62.3		62.5		61.6
Mean Second Quarter Wage	$2,279		$2,342		$2,250		$2,221
Demonstration Impact on:							
	Men	**Women**	**Men**	**Women**	**Men**	**Women**	
Probability of Receipt of Long Term Placement	.041**	.018***	.066**	.005	.022***	-.015	
Probability of First Quarter Employment	-.008	.039	.017	.001	.002	.063**	
First Quarter Wages	.157**	-.175	.114	-.097	.114	-.036	
Probability of Second Quarter Employment	.018	.075***	-.007	.0117	-.022	.059***	
Second Quarter Wages	.071	.089	.132***	.131	.080	-.930	

* Treatment group mean significantly different from control group mean at .05 level of significance for a one tailed test.
** Coefficient statistically significant at the 95 percent confidence level for a two tailed test.
*** Coefficient statistically significant at the 90 percent confidence level for a two tailed test.
Source: Corson, et. al. (1985), Tables III.10 through III.14.

Exhibit 6 suggests that between 7 and 12 percent of claimants received a long-term job placement, that is, one expected to last at least 150 days. The percentage of claimants in the first two treatment groups who received such a placement was significantly higher than observed for the control group, though it is interesting that almost 7 percent of control group members obtained long term placements via voluntary use of the ES. Additionally, there was no difference between control group members and treatment group members exposed to strengthened work search requirements, in the proportion obtaining a long term placement.

The exhibit further suggests that there were no treatment-control differences in the proportion of claimants who reported employment in the first quarter after registration; this figure was just over 50 percent for all four groups. By the second quarter after registration, treatment group members who received the full complement of services appeared to gain an advantage. Sixty five percent of those in Treatment Group 1 reported employment compared to 62 percent of the control group: a statistically significant difference. This did not translate into higher quarterly earnings, however, as Exhibit 6 shows.

Regression estimates of treatment effects, controlling for demographic characteristics, are presented in the second panel of the table. As suggested, members of all three treatment groups maintain an advantage over control group members with respect to the probability of obtaining a long term placement, with most of these effects attributable to males. Consistent with the unadjusted results in the first panel, however, the three demonstration treatments did not show consistent effects on other employment and earnings indicators. Additionally, other explanatory variables in the regression models did not prove to be significant predictors of the various employment and earnings measures, save for potential benefit duration and the wage replacement ratio. For these, signs were in the expected direction, with negative effects on employment and positive effects on earnings measures. Thus, one must conclude that, with respect to employment and earnings, the enhanced labor exchange services provided through the demonstration did not appear to yield appreciable benefits to registrants.

Although the demonstration did not appear to affect employment or earnings, Exhibit 7 shows that there were tangible effects on UI benefit weeks collected. Average weeks collected were uniformly lower for all three treatment groups, by between .5 and .7 weeks; these differences were statistically significant. The distribution of weeks claimed also shows that a greater proportion of treatment group claimants collected only 1-2 weeks of benefits, relative to the control group, with these differences also statistically significant.

The second panel reports estimates of treatment effects on UI benefit weeks collected. Again, the regression coefficients rather than adjusted treatment-control group differences are shown. Importantly, these results suggest effects for men, but not for women -- mirroring the placement effects shown in Exhibit 6. Results for other explanatory variables in the model (see Corson, et. al., 1985, p.75) were not surprising. Older people and women showed longer unemployment durations, as did individuals with longer potential UI benefit durations and those with higher wage replacement ratios.

Corson, et. al. (1985) further attempted to trace the source of the differential demonstration effects for men and women, by examining impacts by prior industry and by recall expectations. As shown in Exhibit 7, much of the demonstration impact can be traced to men in the construction industry. A large proportion of men -- some 36 percent -- had worked in construction prior to their unemployment, and treatment effects were much larger for this group. Men who did not expect recall also showed substantial negative treatment effects on weeks collected, but these effects vanished once industry was controlled for. Thus, the authors conclude that, while the demonstration treatments each reduced UI benefit weeks collected, most of that effect was attributable to one subgroup: men who had formerly worked in construction.

Exhibit 7

Demonstration Impacts on Benefit Weeks Collected

Outcome	**Treatment Group 1**		**Treatment Group 2**		**Treatment Group 3**		**Control Group**
Weeks of UI Collected							
1-2	10.3*		10.5*		11.2*		7.6
3-4	7.4		7.4		6.5		7.9
5-7	12.9*		10.9*		11.9*		9.8
Mean Weeks Collected	14.8*		15.0*		15.0*		15.5
Demonstration Impact on:							
	Men	**Women**	**Men**	**Women**	**Men**	**Women**	
Weeks of UI Collected	-1.14*	-.015	-1.15*	.031	-.083*	-.020	
Weeks Collected - Construction Industry Workers	-2.06*	2.30	-1.98*	2.02	-2.07*	2.25	
Weeks Collected - All Other Industry Workers	-0.88**	-.14	-.83**	.23	.38	-.19	

* Treatment group mean significantly different from control group mean at the 90 percent confidence level for a one tailed test.
** Coefficient statistically significant at the 95 percent confidence level for a two tailed test.
*** Coefficient statistically significant at the 90 percent confidence level for a two tailed test.

Source: Corson, et. al. (1985), Tables III.10 through III.14.

Exhibit 8 summarizes the cost of the demonstration treatments and compares these to the reductions in UI benefit weeks that were achieved. Cost estimates were derived from a detailed work measurement effort that captured the staff time and cost associated with performing various demonstration functions. Benefit estimates were derived by multiplying the reduction in weeks collected for each treatment group, by the average UI weekly payment. Results suggest that demonstration treatments were inexpensive to implement --

Exhibit 8

Demonstration Costs and Cost-Effectiveness

	Treatment Group 1	**Treatment Group 2**	**Treatment Group 3**
Reduction in Mean UI Benefits			
Weeks Collected	.76	.61	.55
Dollars Collected	$73.14	$58.71	$52.93
Net Additional Administrative Cost Per Claimant	$17.58	$13.17	$4.72
Net Reduction in UI Cost Per Claimant	$55.56	$45.54	$48.21
Lower Bound	$18.99	$10.89	$12.60
Upper Bound	$92.13	$80.19	$83.82

Source: Corson, et. al. (1985), Table V.1.

the average net additional administrative cost ranging from about $5 for the strengthened work test treatment, to about $18 for the most comprehensive treatment package. Combining these with estimates of treatment benefits yields net reductions in UI costs per claimant of between $45 and $55 per treatment. Net reductions were greatest for the most comprehensive package of treatments, but interestingly, the savings was only $8 more than for the strengthened work test only treatment.

3.3 Summary of Key Findings

Results from these four studies provide reasonable evidence that both ES labor exchange and work test activities are cost-effective from society's perspective. While they do not answer all of the questions of importance to us, the four studies do contribute a great deal to our understanding of the motivations for use of the ES and its impacts on reemployment and UI benefit receipt. Specifically:

- For those individuals who are not required to register with ES as a condition of benefit receipt, it appears that there are substantial delays -- on average, two quarters -- between unemployment and application at the ES. It appears that users consider the ES as a "backstop" job finding strategy, turning to it when other search methods do not yield results.

- A minority of ES applicants receive job referrals, counseling, or placement help; the percentage appears to vary between 20 and 30 percent. The best predictors of service receipt are indicators of motivation, attitude, or work ethic, suggesting that self- and agency selection play an important role in whether an individual obtains help.

- Mandatory registrants are significantly less likely to receive a referral than volunteers, even after controlling for a host of other variables and various indicators of motivation. This may be the case because ES perceives voluntary applicants as more motivated to find work, or it may be the case that these individuals possess attitudes that are not conducive to referral or placement.

- Women who receive an ES referral appear to experience a reduction in time to reemployment, and increased earnings during the six months after application. This arises mainly because they spend more of the period employed and less time out of the labor force.

- Once delays in ES utilization are controlled for, ES activities may benefit men as well, reducing subsequent time to reemployment by as much as 10 weeks. The effect is more pronounced the longer the individual delays in turning to the ES -- not surprising in light of the fact that the counterfactual for such people is probably prolonged unemployment.

- In large part because of the their low cost, ES labor exchange activities have a favorable benefit-cost ratio, estimated at 1.8:1. For every dollar invested, the program returns benefits of $1.80.

- Strong work registration requirements appear to increase the incidence of nonmonetary determinations, and in turn, benefit denials. It is also supposed that strong requirements also carry deterrent effects, with those who are likely to run afoul of requirements not bothering to apply for UI benefits.

- Work registration requirements appear to influence the behavior of men, but not women. Much of this effect appears to be driven by recall expectations and work in an industry where employment is seasonal.

- Strong work registration requirements also translate into fewer benefit weeks claimed: on average, between one half and three quarters of a week. While this seems like a small amount, because the estimated cost of strengthened requirements is so low, there is a sizeable net savings to society.

The main threats to the validity of these findings, in order of their severity, are the following:

- *Self and agency selection into the treatment condition.* Most of these studies compare experiences of individuals who receive services from the ES, with a like group of applicants who apply but do not receive these services. Data suggest, however, that service receipt is not random, and is probably most driven by factors -- like attitude and motivation -- which are highly correlated with future employment. This selection, if not addressed, can bias estimates of ES impact.

- *Generalizability beyond the evaluation sample.* With the exception of the National Evaluation, which relies on a representative sample, the other studies concentrate on one particular state or a small handful of states. Findings may thus reflect the unique experience in that particular state and may not hold if the study was replicated elsewhere.

- *Generalizability beyond the evaluation time period.* The ES' mission and funding have changed in important ways since the National Evaluation was undertaken. So has the labor force participation of women. It is not clear whether results from this study in particular would still generalize to the current environment.

Below we suggest some replications or extensions of these four studies which might serve to address these concerns, as well as others which would fill important gaps in our knowledge.

SECTION FOUR

HIGHLIGHTS OF FINDINGS AND A RESEARCH AGENDA

This concluding section highlights findings from this review and presents a brief agenda for future research in four key areas: labor exchange functions; work registration requirements and their interaction with labor exchange functions; ES effectiveness for various population subgroups; and last, employer utilization of ES services -- a topic that we have not addressed in this review since we lack even basic information.

4.1 Labor Exchange Services: Key Findings and Opportunities for Future Study

The 1983 *National Evaluation* provides reasonable evidence that ES referrals provide positive short term benefits for women, reducing their time to reemployment; increasing the amount of time they spend employed as opposed to out of the labor force; and increasing their earnings, during the six month period following application. Though only women appear to benefit, because the cost of providing referrals is so low, the program yields a favorable overall cost-benefit ratio in spite of the fact that only women benefit.

Katz, in turn, finds that once delays in the timing of ES use are controlled for, and once ES users and non-users are compared at the same point in their unemployment duration, ES use may reduce remaining unemployment by as much as 50 percent.

The main threat to both of these studies is the comparability of the ES user and non-user groups; both suggest strong evidence of self-selection, both in the decision to seek help from the ES and in the timing of the help request. Given that the factors which prompt a person to seek help may also be powerful predictors of reemployment, estimates of ES effect may be biased.

In view of this concern, we recommend two related extensions to these works. One would focus on learning more about self- and agency selection processes that: (1) lead some, but not others to seek services from the ES; and (2) that condition who among the applicants actually receives referrals and placements. Focus groups with various groups of

ES "eligibles", as well as groups with agency staff, might serve as a starting point for such a study; observation of staff-client interactions might also be informative. Based on this information, a larger scale survey of eligibles and agency staff might be undertaken to determine the prevalence of various attitudes and behaviors.

The second extension would involve estimating ES impacts via an experimental design involving random assignment. While the ES dictum to "serve all in need" has been used in the past to preclude random assignment, it is also clear that the current program does not serve all comers: less than half of all applicants receive any type of service. Randomly allocating assistance among individuals with similar needs and qualifications might be the fairest way to distribute resources, at the same time as it would yield more defensible estimates of program impact.

This could be accomplished by assigning applicants with similar attributes and motivation, to treatment or control status following an initial assessment. Treatment individuals would receive referrals if such were received by the ES and were appropriate; control status individuals would not receive referrals until after a specified period. Alternatively, both treatment and control group members might be offered the opportunity to use ES self-placement services, with treatment group members receiving the actual referrals; impact and cost effectiveness estimates would then pertain to the incremental effectiveness of the referral activities.

As part of this effort, it would also be useful to investigate recent trends in automation of job matching and labor exchange functions. Specifically, we might be interested in learning whether such systems are more cost-effective than labor intensive approaches.

4.2 Work Registration Requirements and their Relationship to Labor Exchange Functions

ES involvement in work registration activities appears to pay off, in terms of increased incidence of nonmonetary determinations and reduced UI benefit weeks claimed. What we do not know, unfortunately, is whether these findings hold on a national level; and how sensitive they are to variations in the intensity of work search requirements as enforced by individual states. Also, despite claims concerning conflicts in the use of resources, we do not really know how work search and labor exchange activities interact with each other. We do not know whether the current condition, in short, is the optimal one that can be achieved.

An important extension to current knowledge would address both of these issues. This might be done in a smaller-scale study similar to the 1984 Mathematica study of nonmonetary eligibility. One might select a manageable sample of states with varying intensity of work registration requirements, obtain ESARS and UI nonmonetary determination and benefit weeks data, and actually compare service use and outcomes for mandatory and non-mandatory registrants. This would be supplemented with fieldwork which would consider actual implementation of work registration requirements "on the books." Of particular interest would be selective implementation of registration requirements for various categories of individuals (e.g. dislocated workers, those expected to find a job), as well as differential treatment of these individuals by agency staff.

To address issues relating to workload, the study would also examine staff time allocation, using time use diaries or similar data collection tools, for selected periods to ascertain the impact of mandatory registration on staff workload. Since ESARS Cost Accounting System records would not provide information on the actual expenditures associated with different functions, such a new data collection effort would need to be implemented.

4.3 Effects for Population Subgroups

A third issue that might be examined concerns effects for subgroups, in particular, mandatory registrants required to register with the ES to maintain eligibility for UI benefits. Understanding how such individuals respond to work registration requirements, and how they are perceived and treated by agency staff, would be useful in evaluating claims made regarding the deleterious effects of registration requirements.

A second important subgroup concerns dislocated workers. While Katz' work provides some evidence about ES effectiveness for this group, his results are largely limited to one state; a more comprehensive assessment of impacts and costs of serving this group, should be conducted. This effort would examine the following themes:

- *Factors which influence receipt of ES services.* These would include work registration requirements, as well as self- and agency selection practices, and attributes of job orders listed. Of particular interest would be an investigation of the timing of receipt of services, and the role of self and agency selection processes as determinants.

  Given the attributes of dislocated workers relative to other ES users -- generally older, with more work experience and education, and higher average earnings on the layoff job -- one might expect, a priori, that different selection criteria might be employed in making referrals. This may also be the case in view of attributes of job orders on file. Thus, we feel that a comprehensive investigation considering all of these factors, would be extremely useful.

- *Impacts of ES services on reemployment and earnings, not only in the short run, but in the longer run as well.* The range of outcomes should be broad: time to reemployment, reemployment wages, hours and earnings, and time spent employed during the followup period. These might be measured six months following application, as well as 18-24 months out.

- *Cost-effectiveness of ES services.* The standard by which most social investments are judged is whether their benefits are equal to, or exceed, their costs. This calculus should be performed for those investments targeted at dislocated workers.

4.4 Employer Utilization of ES Services: Participation, Benefits and Costs

A final series of research issues involves employer utilization of ES services, and the benefits and costs of such activity. This is an area of which we are almost completely ignorant, since most prior research has focused on the individual job seekers. More information should be gathered -- initially from focus groups, later from a more structured survey -- on employers' utilization of the ES and reasons for/against; benefits obtained; and associated costs. This final piece of the puzzle would help us to better evaluate whether payroll taxes levied actually translate into benefits from the perspective of employers and society.

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The Effectiveness of the US Employment Service

by

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14 March 1994

Second Draft

for the

Advisory Commission on Unemployment Compensation

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1.0 INTRODUCTION

This paper describes the role of the Employment Service (ES), develops a framework for evaluating how effective the ES is in meeting its objectives, presents relevant evidence on ES effectiveness from existing evaluations, and discusses what additional information is needed to assess and improve the ES's performance.

To begin, it is important to understand how the ES as an institution and the ES's core service, direct job placement, relate to the range of government programs and services aimed at assisting individuals find jobs and increase their earnings.

Today, US workers with strong attachment to individual firms receive some measure of protection from temporary and permanent job loss from a range of public and private programs. The public employment-security system mainly consists of:

- The Unemployment Insurance System.
- The US Employment Service.
- The Trade Adjustment Assistance Program.
- Job Training Partnership Act (JTPA) Title-III programs as modified by the Economically Dislocated Worker Adjustment Act (EDWAA).
- Pell Grants and Stafford Loans supporting retraining at community colleges, proprietary schools, and similar institutions.

Economically disadvantaged workers, who rarely have strong attachment to individual firms, are assisted primarily by such programs as:

- Aid to Families with Dependent Children (AFDC) and other welfare programs.
- Job Opportunity and Basic Skill (JOBS) Training for AFDC recipients.
- Food Stamps.
- The US Employment Service.
- Job Training Partnership Act (JTPA) Title-II programs.
- The Job Corps (JTPA Title-IV).
- Pell Grants and Stafford Loans supporting training at community colleges, proprietary schools, and similar institutions.

Collectively, these programs provide income support contingent on being unemployed or having very low income, job search assistance, counseling, training, direct placement services, and referral to other public and private programs.

Workers needing assistance in finding jobs who do not meet stringent income tests or lack sufficient work experience to qualify for UI primarily receive government assistance from:

- The US Employment Service.

Among the set of governmental social safety-net programs the Employment Service is unique in that it places no restrictions on the characteristics or number of individuals served. As a result close to 20 million individuals seek assistance from the ES each year. Among other DOL programs only the UI program provides aid to as many people, and only during severe recessions. In contrast, JTPA/EDWAA programs serve about one-tenth the number of individuals served by the ES. TAA and the Job Corps reach only about one-hundredth the number served by the ES.

The primary service provided by the ES is direct placement at jobs. Over 3 million job vacancies are filled by the ES in an average year. The ES carries out its referral/placement services by acting as a public labor exchange open to all firms. Thus, a second unique feature of the ES is that it places no restrictions on the number or types of firms that list vacancies with its labor exchange.

The ES and UI programs are run by State Employment Security Agencies (SESAs) established under the Wagner-Peyser Act. Typically, SESAs establish 50 or more separate ES offices each with its own labor exchange, and use computers to tie local offices together to permit searches across their own and neighboring states. States have considerable discretion to tailor programs to suit local needs.

The UI and ES is largely funded by the Federal Unemployment Tax Act (FUTA) payroll tax. Thus, the UI/ES system is unique among safety net programs in that it has its own source of revenue, covering over 60 percent of its total budget.

UI/ES payroll taxes raise more than $20 billion each year. Depending on economic conditions, from $13 to $18 billion goes to paying UI benefits and from $1.4 to $2.0 billion goes to UI administration. The Wagner-Peyser contribution to the ES amounts to about $1 billion a year, about 62 percent of the ES's total budget. Thirteen percent of the ES budget comes from EDWAA and JTPA Title-II funds. SESAs receive additional federal funds to provide services to veterans; participants in JOBS, Food Stamps and other programs; and to develop labor market information in cooperation with DOL's Bureau of Labor Statistics. ES budgets also are supplemented with state appropriations. Those funds have grown substantially in recent years, but only account for about five percent of the nationwide ES budget. All sources of funding taken together provide about $1.6 billion per year to run the ES.

The central feature of the ES is that it has a virtually unlimited clientele, but a severely limited budget. As a result the ES only spends about $80 per registrant. Yet, policy-makers often expect the ES to produce outcomes equal to those of job seekers with superior information-- unique personal contacts; and/or perform as well as DOL programs that spend thirty times more per capita. <u>The unreasonable expectations for the ES's performance relative to its resources largely explains why, inside the beltway, the ES is often regarded as ineffectual.</u>

A second feature of the ES that makes it unpopular with policy-makers is that its central function of matching workers to jobs is not seen as having the capacity to substantially raise individual's earnings. This view is held in part because most of the placements are at relatively low-paying jobs, and in part because classroom training is seen as a more effective mechanism for raising workers' earnings. Available evidence, however, suggests that what is learned by working, even at low-paying jobs, is at least as effective in boosting long-term earnings as learning in a classroom setting.

That direct placement services are held in low regard is evident in the design of the recent National JTPA random-assignment evaluation that placed primary emphasis on measuring the value of retraining. Since only about one-third of JTPA participants receive training, the study separately examined the effectiveness of job search assistance. No attempt was made, however, to distinguish the return from direct job placement versus teaching participants to more effectively search for work. Yet, both direct and indirect placement activities are major elements of most local JTPA programs. Key staffing decisions are made regarding how much effort should be placed on job-development and referral versus job finding workshops and other indirect forms of job search assistance.

A central fact is that there has never been a comprehensive random-assignment evaluation designed to measure the merits of a wide variety of ways to boost earnings for different populations. Thus, we know relatively little about how direct-placement and job-development efforts compare to counseling, instruction on how to search for work, classroom training, and on-the-job training.

2.0 FRAMEWORK FOR EVALUATING THE EFFECTIVENESS OF THE ES.

Standard benefit/cost analysis provides a suitable framework for examining ES effectiveness. Technically, a more appropriate framework would be to analyze the equity and efficiency of a public labor exchange (and other aspects of labor market information gathering and dissemination). The complexity of such a framework makes such an analysis inaccessible to most policy-makers, however, and the paucity of relevant data makes such an exercise sterile.

The key benefits of ES services from the viewpoint of job seekers' using the ES are reducing the duration of joblessness, increasing initial pay, and increasing long-term earnings.

The mechanism by which the ES can raise long-term earnings appears to be widely misunderstood. It is not necessary for initial wages to be high as long as: (1) the worker has good prospects for subsequent wage growth with the initial employer, or (2) the initial job can be a stepping-stone to jobs with other employers that offer substantial wage growth. Also, subsequent wage growth can stem from acquiring new skills on the job or in school, demonstrating existing desirable qualities, as well as by learning about job openings that more appropriately match existing skills to employers' needs.

The key benefits of ES services from the viewpoint of firms are reducing UI taxes and more quickly filling vacancies with highly suitable workers. Because UI taxes are experience-rated, firms have a strong interest in seeing their own redundant workers rapidly end their spells of compensated unemployment.

The ES, therefore, has been given a special responsibility to monitor the job search of UI claimants. Registering with the ES is mandatory for most UI claimants, and UI claimants must accept offers of "suitable-work." The UI system severely penalizes refusal to accept suitable-work and refusal to act on a referral to a suitable job. In most states the "refusal" penalty is an "indefinite" denial of UI benefits. That is, claimants must hold a new job for a specified period before they reestablish their eligibility for UI. Other work-test violations lead to a loss of one week's payment, but no loss of entitlement. Available evidence suggests that the threat of an indefinite denial has by far the strongest effect on job search.

Firms also have an interest in rapidly filling vacancies with highly suitable workers. A public labor exchange has the potential to increase the net efficiency of the labor market in filling vacancies. For example, in rural areas it may be particularly difficult for firms to effectively advertise their job openings. Similarly, firms employing low-skill workers who must travel considerable distance to the work site, such as inner-city residents, may also have difficulty effectively advertising openings.

Almost nothing is known about the ability of the ES to improve overall labor market efficiency. Evidence showing that the ES helps users find jobs more quickly does not necessarily imply job slots are being filled more efficiently. What is clear is that certain types of employers heavily rely on the ES. Those firms generally employ workers with few specialized skills, are willing to accept high turnover, and therefore, pay low wages.

From those firms' viewpoints the ES is more cost-effective than other means of filling those slots, but the payroll tax paid by those firms may cover only a small fraction of the cost of the services received. The potential negative effects on the firms implicitly paying the subsidy would have to be known in order to judge the ES's overall effect on labor market efficiency.

Finally, the key benefit of ES services from society's viewpoint is reducing transfer payments such as UI, AFDC, and Food Stamp payments. Thus, the ES has been given special responsibility to facilitate and monitor the job search of AFDC and Food Stamp recipients, as well as UI claimants.

It would seem that the ES has great potential to assist workers who receive transfer payments for prolonged periods because such workers are likely to have exceptionally poor information about the location and pay of job vacancies. In addition, because of society's interest in reducing transfer payments the ES can be cost-effective even if it only "levels the playing field" so workers receiving transfers obtain jobs that would otherwise go to workers not receiving transfers.

Society would gain even more if the ES led to more efficient filling of job slots. For example, there may be economies of scale in pooling information about attributes of vacancies and job seekers. Thus, a public labor exchange could reduce initial search costs for workers and firms, and reduce subsequent turnover. The savings would free resources to increase output and most likely increase the demand for labor which would raise wages as well.

In summary, in order to evaluate the effectiveness of the ES it would be highly desirable to measure the following potential benefits:

- Increases in short- and long-term earnings of ES-users.
- Reductions in UI, AFDC, Food Stamps and other transfer payments among ES-users.

- Reductions in the time it takes to fill vacancies and subsequent worker turnover among firms listing vacancies with the ES.

The primary cost of the ES is its own budget. Because the expenditures are so low per capita, the ES can more than pay for itself by generating small per capita gains. For example, reductions of about half a week of UI payments per claimant served would more than cover the cost of the ES services to claimants.

Typically, the budget of government programs is treated as a "black box" in benefit-cost studies. In other words, the total cost is divided by the number of participants and no attempt is made to determine how costs vary across participants or service providers. In the case of the ES, however, differences in the cost of delivering similar services to different individuals has been examined. Such studies are feasible in part because there is substantial cross-state variation in ES staffing levels, use of automated equipment, management procedures, and client mix. That analysis provides valuable information about ways to reduce costs without adversely effecting benefits, and permits determining the costs and benefits of shifting resources to different uses for different client groups.

A secondary cost of the ES is adverse effects on the job search of workers who do not receive ES services and firms that do not list vacancies with the ES. Those costs take the form of:

- Decreases in the short- and long-term earnings of non-ES-users.
- Increases in transfer payments to non-ES-users.
- Increases in the time it takes to fill vacancies and subsequent worker turnover among firms not listing vacancies with the ES.

It is possible that all user benefits are precisely offset by costs imposed on others. Much more likely, however, is that considerably less than a third of the direct benefits are offset by those indirect costs. Because the central role of the ES is offsetting job seekers' information deficits and reducing the disincentive effects of UI and other transfer programs it is likely that the ES substantially increases the effectiveness of job search of users. This expands the effective supply of labor as long as non-users react to greater competition by increasing their search intensity and/or lowering their reservation wage.[1] By and large we lack the research needed to describe distribution of benefits and costs among consumers, job seekers using the ES, job seekers not using the ES, firms using the ES, and other firms.

3.0 MEASUREMENT OF THE EFFECTS OF THE ES.

Studies of the direct effect of government programs on participants are called partial equilibrium studies because, unlike general equilibrium studies, they ignore the influence of the program on non-participants. Ignoring the secondary effects is reasonable until a program is shown to have large net positive benefits. For example, the program could be shown to have a small positive effect on each of a large number of participants, or a large positive effect on relatively few participants.

Given that the ES registers about 20 million workers annually (roughly one-quarter of all job seekers), refers over 3 million workers to jobs, and about 1.5 million workers accept jobs to which they were referred, there is considerable potential for the ES to have strong effects on non-users.[2] But there has been no support for general equilibrium studies of the ES, possibly because of skepticism about whether ES placements have an appreciable positive effect on users' job search.

<u>The key to evaluating how the ES affects users is measuring what would have happened to the users had they not received services from the ES.</u> Usually the most clear-cut results from government program evaluations are derived from random-assignment experiments. Such analyses compare the experience of individuals receiving a given government service to those who applied for and were

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[1]Expanding the supply of labor by reducing disincentive effects of transfer programs lowers the net cost of production and leads to increased output. Although some workers would suffer small reductions in earnings, society benefits because the net utility gained by consumers from expanding production is greater than the reduction in workers' purchasing power. Economists call that gain the "consumer surplus."

[2]Earlier it was noted that over 3 million jobs vacancies are filled by the ES. However, the number of vacancies filled is about twice the number of registrants placed at least once. About one-third of ES referrals are to "temporary" jobs, jobs expected to last less than 150 days. A high percentage of those registrants are referred and placed at a series of temporary jobs each year.

eligible for services, but who were randomly assigned to a control group to which services were not provided.

There has never been a random assignment study of the ES and it is unlikely that services could be withheld from a designated group. Withholding of services is close to impossible because, unlike JTPA and most other government programs helping workers locate jobs, the ES traditionally has been open to all comers, and has a responsibility to firms to refer suitable job applicants.

A quasi-experimental design, however, might prove feasible. For example, the ES could be asked to match a worker referred to a given vacancy with another similar ES registrant who is looking for work, but was not referred in a given week.

Reasonably accurate analysis appears to be possible by carefully tracking registrant actions, but not using random assignment. Because speed is critical in placing registrants UI offices often simply refer the first qualified individual who happens to come into the office after a vacancy is listed. Alternatively, the ES searches through its registrant list to find a suitable worker as soon as a vacancy arrives. It then phones to see if the registrant is interested in the job. The first qualified registrant who happens to be home and is interested is given the referral. Thus, a reasonable case can be made that "the luck of the draw" plays a major role in determining which workers are referred and placed.

That most ES referrals are entry-level positions requiring little in the way of special skills makes it even more likely that registrants' timing in use of the ES and that availability to act on referrals, rather than unobserved registrant characteristics, account for differences in the receipt of referrals.

Existing studies that attempt to measure what would have happened in the absence of the receipt of ES services use comparison groups selected among persons who closely resemble those who received services from the ES, but did not receive services. Such analyses are greatly aided by state UI/ES computer systems that record the timing and nature of each ES service as well as maintain detailed employment histories for each registrant and non-registrant employed in firms covered by UI.

To my knowledge the studies of the US-ES effects on job search outcomes using comparison groups and appropriate methodology are limited to:

- A DOL funded study of 25 ES offices in 23 different states conducted by Terry Johnson and his colleagues at SRI in the early 1980's.
- On-going studies of the Pennsylvania ES that I conducted with Arnold Katz over the last five years that build on Katz' 1977 analysis.
- An on-going study of the Washington State's Claimant Placement Program (CPP) and other ES programs I conducted with the assistance of Washington State officials.

All three efforts compare ES-users to similar non-users using multiple regression to hold constant a host of observed factors that affect job search outcomes. The central issue in assessing the validity of the studies is whether there are unobserved characteristics that effect both use of the ES and job search outcomes. The studies differ in how they deal with selection bias; and the type of data used. Johnson et al primarily relied on a survey, while the Pennsylvania and Washington studies used administrative data.

In addition to studies of the degree to which the ES reduces the duration of joblessness, saves transfer payments, or increases earnings; there are studies of service delivery efficiency. Those studies primarily focus on factors that affect the probability different registrants are placed.

Just as with studies focusing on benefits, the key to accurately measuring cost differences is adequately controlling for external factors that influence the difficulty of making a placement. Once external factors such as local labor market and worker characteristics are held constant, the remaining variation in cost presumably is due to differences in management effectiveness.

To my knowledge the studies of sources of variation in the probability the ES places users at jobs are limited to:

- A series of studies conducted by the US General Accounting Office (GAO) under the direction of Robert Rogers.

- A study of the Pennsylvania ES I carried out.[3]

Both studies use very large and detailed data bases to examine factors influencing placements. The GAO work is particularly revealing because it collected and analyzed remarkably detailed data on specific management practices in 438 local ES offices. In distinct contrast to studies of job search outcomes, placement productivity studies reach clear-cut conclusions because they are not encumbered by major selectivity bias problems.

4.0 STUDIES OF THE EFFECTS OF THE ES ON REGISTRANTS' JOB SEARCH

4.1 Johnson-Dickinson-West's DOL Study.

Terry Johnson, Katherine Dickinson, and Richard West examined the difference in job search outcomes (the number of weeks required to find a job and earnings in the first six months following registration) between a sample of ES-registrants who were referred to jobs versus those not referred. A base-line and nine-month follow-up survey was administered to unemployed first-time ES registrants in a nationally representative sample of 30 local ES offices from July 1980 through May 1981. ES records were used to monitor services received and UI wage records were used to measure earnings before ES registration.

About 25 percent of the registrants were excluded because they were employed or had already registered at the ES prior to the intake period. Five local offices could not provide the necessary ES service data and were dropped from the study. Suitable data were received for 62 percent of the remaining registrants. Most of those dropped refused to be interviewed or could not be located for the follow-up interview. However, the pre-ES-use characteristics of those not reinterviewed were similar to the characteristics of the sample studied.

[3]State SESAs have created their own measurement systems to compare management effectiveness across offices and even across workers within a single office, but to my knowledge, the information on the design of those systems has not been collated.

Regressions were used to control for differences in personal attributes, prior work-histories, and local labor market conditions. Importantly, regressions showed that the annual earnings prior to registration of those referred was about $150 below those not-referred, and these small differences were not statistically significant a year prior to registration.

An unusual aspect of the analysis was that the survey assessed the strength of registrants' motivation to find work, confidence about their ability to find suitable work, and feeling of overall efficacy. Registrants with high indexes of each of these measures were more likely to receive referrals, but the effect on job search of those variables was not reported in the study.

Considerable attention was devoted to determining if unmeasured differences between those referred and not-referred biased estimates of ES effectiveness. Line 1 of Table 1 shows the key effects of the ES for men and women controlling for observed differences. Line 2 shows the effects corrected for selectivity bias based on Heckman's suggestion to use the residual of a regression on the probability of being referred as an independent variable. Line 3 shows the effects corrected for selectivity based on Maddala and Lee's suggestion to use an estimate of the probability of being referred as an independent variable.

The central finding is that uncorrected for bias the ES reduced the time it took women to find a new job by 2.8 weeks and thereby boosted earnings by $325. The earnings increase was entirely attributed to the more rapid return to work as the wage rate effect (not shown) was insignificant. In contrast, the results on line 1 suggest that the ES had no statistically significant effect on the time it took men to find jobs.

Corrections for selection bias substantially raised the estimate of the ES's ability to help workers rapidly find jobs. The increase in effectiveness suggests ES-referrals were made to registrants who otherwise would have more difficulty finding jobs than registrants with identical observed characteristics who were not referred. In other words, the ES tended to offer jobs to registrants who were substantially less able to locate jobs on their own than registrants with similar observed characteristics.

Table 1. The Effects of ES Referrals on Earnings and Unemployment Duration in the Six Months following Registrations using a Nationally Representative Sample for 1980-81 Program from Johnson et al's Study.

	MEN		WOMEN	
	Earnings (dollars)	Unemployment Duration (weeks)	Earnings (dollars)	Unemployment Duration (weeks)
Effect of all referrals made in first six months:				
1. OLS	-98 (141)	-.64 (.48)	325* (94)	-2.79* (.51)
2. Heckman Adjusted	3947* (1192)	-9.02* (4.03)	275 (643)	-6.25 (3.55)
3. Maddala-Lee Adjusted	4595* (1467)	-10.40* (4.95)	155 (723)	-7.62 (3.98)
OLS effect of referrals made in:				
4. First month	311 (164)	-2.12* (.55)	709* (106)	-4.85* (.58)
5. First three months	67 (148)	-1.38* (.50)	513* (97)	-3.90* (.53)

* Indicates estimate statistically different from zero at the five-percent confidence level.
Standard errors are in parentheses.

Even though selectivity adjustments indicate that the time it took women referred by the ES to locate jobs was underestimated by OLS regressions, the same selectivity adjustments suggest that the increase in earnings following referral was overestimated. This puzzling result could be due to the inclusion in the regressions of ES registrants who had no earnings in the six-month follow-up period. A more appropriate treatment would exclude "zero-earners" and introduce sample selection correction for being employed similar to the correction for being referred.

That the selectivity adjustments on earnings versus duration of joblessness are in opposite directions for women, but not men, supports the view that failure to exclude zero-earners and correct for differences in the probability of returning to work are the source of the inconsistency. It is evident that women in Johnson et al's sample are much more likely than men to have withdrawn from the labor force.

The authors observe that the adjusted gains for men are implausibly high. They argue the gains are greater than the average earnings of those referred and the earnings of those not referred cannot be less than zero. Thus, the magnitude of the selectivity adjustment probably is overstated. But it is plausible that the direction and general magnitude of the bias is correct.

First, registrants who have reasonable prospects for finding work on their own are likely to refuse referrals. Thus, referred registrants probably would otherwise have low earnings. Second, the large standard deviations of the adjusted estimates suggest that point estimates themselves are imprecise.[4] Third, inappropriately keeping "zero-earners" in the sample also most likely contributes to the point estimates over-stating the "true" effect.

Leaving selectivity bias aside, a clear indication that the OLS estimates on line 1 do not adequately capture the effectiveness of ES referrals is that the effectiveness of the ES is much greater when the timing of the referrals are taken into account. Lines 4 and 5 of Table 1 show OLS coefficients measuring the effects of referrals made within the first month, and first three months of registration, respectively. Although women still show much stronger positive effects than men, the results for men are positive and statistically significant when the return to work is observed for three to six months following an ES referral.

It is self-evident that only examining the influence of ES actions over the first six months following registration biases downward estimates of overall effectiveness. The six months following referral is a more appropriate period for comparing outcomes of those referred by the ES to job-seekers not referred. This is particularly true for male UI claimants whose UI entitlement lasts about six months, and whose incentive to return to work increases substantially after benefits are exhausted.

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[4] The standard error of the earnings estimate increases from $94 without the corrections for selectivity bias to about $700 with the corrections for women, and increase from $141 to about $1,300 for men. Those large increases in the plausible range of the "true" effect maybe a result of having a relatively small sample of referred and placed ES users. Although 4,564 ES registrants provided suitable data for analysis, only 30 percent of those interviewed were referred to jobs and only 30 percent of those referred were placed by the ES (roughly 225 in each of the two gender cohorts).

Table 2 breaks-out the OLS results by an array of additional registrant characteristics. The usefulness of these estimates is limited because we can not judge the importance of selectivity bias. Taking the estimates at face value suggest that the ES is most effective in aiding women who are relatively low paid, high school graduates, with one to nine years of work experience, who are not mandatory registrants.

The results suggest that (in the six months following registration) the ES would be reasonably effective in aiding women on welfare, but not very effective in aiding male UI claimants. (Most men whose registration is mandatory are UI claimants, while for women the mandatory registration group probably is equally divided between UI claimants and welfare recipients.) The UI claimant results for men probably is valid for the six months covered in this study, but ignores the effectiveness of the ES in aiding male UI claimants after their benefits have been exhausted.

Despite the shortness of the follow-up period and the fact most men apparently are referred late in that period, Johnson et al found that the overall benefit-cost ratio for the ES is 1.6, based on upper-bound estimates for the cost of making the placements.

The bottom line is that the only nationally representative analysis of the ES that uses a comparison group to measure job search outcomes strongly suggests that the ES was highly effective in aiding registrants, given the very low cost of those services.

4.2 Katz and Jacobson's work for the Upjohn Institute.

Several papers comparing job search outcomes of UI claimants who used the ES versus non-users have been written by Katz and Jacobson over the past four years. This work builds on a study Katz completed in 1977 which used UI/ES administrative data to analyze the effects of the ES on users' earnings. The recent work, which was largely supported by the W.E. Upjohn Institute, is nearing completion, but has not yet been published.

The key feature of the Katz-Jacobson analyses is the explicit recognition that the ES is turned to only after other job finding methods failed or were unavailable to searchers. Thus, non-ES users have superior job search outcomes (shorter periods of joblessness and higher reemployment earnings)

Table 2. Estimated Impact of ES Referral on Earnings in 6 Months after Application by Applicant Characteristics using a Nationally Representative Sample for 1980-81 Program from Johnson et al's Study.

Characteristics	Dollar Difference in Regression-Adjusted Mean Earnings of Referred and Non-Referred Groups (Standard errors are in parentheses)	
	Males	Females
Race		
White	-207 (107)	358* (164)
Black	18 (334)	153 (220)
Other	426 (401)	366 (281)
Education		
Not a high school graduate	-280 (247)	92 (185)
High school graduate	-20 (106)	396* (166)
Age		
Less than 21	-88 (255)	312 (162)
22-44	-209 (120)	313* (177)
Greater than 45	670 (462)	446* (271)
Earnings in the 2 years before baseline ($)		
No earnings	-113 (566)	442 (225)
Less than $12,000	-10 (232)	349* (126)
Between $12,000 and $24,000	-63 (235)	238 (170)
Greater than $24,000	-273 (268)	95 (438)

Table 2. cont'd

Years of work experience		
None	-57 (460)	163 (269)
1-3	-246 (304)	302 (165)
4-9	44 (245)	448* (158)
10 or more	-142 (218	254 (175)
Whether mandatory registrant		
Mandatory registrant	-208 (227)	250 (164)
Not mandatory registrant	-34 (176)	358* (111)

* Indicates estimate is statistically significant at five-percent level.

than ES users with similar work-history and demographic characteristics. However, once the delay in the use of the ES is taken into account the ES is shown to have a positive impact on job search.

The authors suggest that these results are a natural outcome of a sequential search process where the best leads are followed first. Surveys of job seekers consistently show that by far the best leads for finding jobs come from "informal" sources of information, primarily tips from friends and relatives. Next in effectiveness are direct application at work sites and responding to newspaper want ads. Public and private employment agencies are generally turned to only after the best mechanisms are exhausted or are not available to a job seeker.

The expected consequences of sequential job search is that, other things equal, individuals with the best sources of information locate the highest paying jobs most quickly. As time passes only individuals with poorer sources of information remain in the job-seeker pool. The authors argue that the ES's primary value is in helping job seekers who have exhausted or lack superior sources of information. Moreover, it is perfectly appropriate for a government program to play the role of a backstop to aid individuals who lack the resources to help themselves.

Unfortunately, critics of the ES, and even some ES administrators, have difficulty recognizing the importance of the backstop role, and feel the ES should "be all things to all people"-- a role the ES cannot hope to fulfill at its present or any feasible funding levels.

To accurately measure the effectiveness of the ES, Katz and Jacobson use hazard analysis to capture the effects of the timing of ES-use and other time-dependent variables such as the amount of UI remaining.

The initial Katz-Jacobson study was restricted to the first use of the ES between 1979 and 1987 by UI claimants living in Pittsburgh, from 25 to 53 years old, with at least three years of tenure (employment with the same firm). Given the enormous restructuring that occurred in Pittsburgh in the early 1980s, these restrictions created a sample with heavy concentrations of dislocated job seekers.

The emphasis on high-tenure workers is partly designed to ensure accurate measurement of the timing of key events. The delay between job loss and ES use is most clear-cut for high tenure workers because they are very unlikely to have searched for jobs or used ES services several years prior to dislocation.

In sharp contrast, low tenure workers have recently searched for work, and may have used the ES to help them locate their most recent job. Economically disadvantaged job seekers typically only work intermittently and also are likely to have recently searched for work. It is difficult to break down search periods into unique segments for such individuals.

Policy concerns also played a role in restricting the analysis to high-tenure workers. The DOL has expressed a strong interest in developing performance measures for the ES. Previous performance measures have not come close to measuring the true value of the ES.[5] Development of

[5] Traditional measures of ES performance include the placement-rate (the percentage of ES registrants placed at jobs by the ES), the fill-rate (the percentage of vacancies listed with the ES filled by referrals), and the market penetration-rate (the percentage of ES registrants placed as a percent of all job vacancies filled). Historically, none of these measures were adjusted to hold constant changes in funding, ES responsibilities, labor market conditions, or client characteristics. Thus, it is difficult to know what to make of comparisons of those measures across areas or over time. In addition, the measures do not reflect the difference made by ES

better measures would be invaluable for making key resource allocation decisions. Thus, demonstrating that it is feasible to measure the value of the ES even for a limited population would focus attention on the proper conceptual framework for evaluation.

Also of great importance, there has been a growing recognition that inexpensive job search assistance plays a major role in aiding dislocated workers. Thus, solid information on the cost-effectiveness of such services could greatly aid policy-makers in deciding the appropriate role of the ES.[6]

Katz and Jacobson gradually broadened the sample of UI claimants analyzed as a variety of methodological issues were resolved. The most recent work, which is discussed below, used a sample of over 30,000 Pennsylvania UI claimants, 25-53 years old, who filed claims from 1979 to 1987, and were not recalled by former employers. Like all previous ES work by Katz and Jacobson the data are derived from administrative records routinely collected by the UI/ES systems.

Administrative data do not provide as much information about each sample member as survey data, but the low cost of assembling administrative data permits use of much larger samples. Assembling the UI/ES administrative data costs less than $2 per observation while obtaining comparable survey data would cost more than one hundred times that amount. Further, administrative data track claimants' work histories for years, and have a low non-response rate.[7]

The most recent Katz-Jacobson analysis employs a model that is similar to the Cox-proportional hazard model to examine the effectiveness of the ES in reducing joblessness. That model

actions. But "value-added" measures are essential to determine the overall effectiveness of the ES, and differences across states or over time.

[6]In contrast, there is a growing belief that welfare recipients and other economically disadvantaged workers who have great difficulty finding jobs on their own need more comprehensive assistance than can be delivered through the ES. Whether or not the effectiveness of the ES is underestimated for other groups, DOL policy makers have reassessed the role of ES and similar low cost services for dislocated workers. It appears likely that the ES will play a major role in a revamped program.

[7]Johnson et al followed registrants for only six months and had close to a 40 percent non-response rate. Non-response bias can be a major problem because individuals with the same pre-treatment characteristics, but inferior outcomes, are often particularly difficult to track.

is ideal for capturing the influence of the timing of ES use relative to job loss on subsequent unemployment duration. The model also controls for time-varying and fixed effects of the UI system on job search.

In initial work the influence of UI receipt was so strong that separate coefficients on the effect of the ES were estimated before and after UI was exhausted. However, recall in the period prior to exhausting UI (roughly the first six months following the start of an unemployment spell) was found to be associated both with short durations of joblessness and a strong propensity to not use the ES. Once recalled workers were dropped from the sample, UI collection variables alone were adequate to control for the effect of receiving UI, and it was unnecessary to separately estimate pre- and post-exhaustion effects.

The model separately breaks-out the effects of placements, referrals (more precisely referrals not leading to placements), and other services (primarily counseling and testing). The comparison group is UI claimants who did not receive ES services. No distinction was made based on whether claimants registered with the ES. Although registration is not mandatory in Pennsylvania, it is in most other states. Thus, use of claimants as the comparison group produces a model directly-applicable for analysis of states where ES registration is mandatory. Finally, the model controls for demographic and work history variables (such as age, sex, earnings, and tenure).

The hazard ratios represent the difference made by ES-use relative to non-use on the probability of ending a spell of joblessness in an average 13-week period.[8] For example, the strongest positive effect of the ES in Table 3 is a 71 percent increase in the probability of ending a spell in a calendar quarter among women.

[8]Hazard models used to predict taking a job are derived from statistical methods used to create survival tables originally applied to predict life expectancy. Technically, they predict the probability of the occurrence of an event such as death or failure of an electronic part over successive time periods. A particularly attractive feature is that the "hazard" can change over time. For example, the chances of living one year at first increases then decreases with each year lived. A separate calculation must be made to translate a set of hazard estimates to a more conventional measure of the average time it takes an event to occur. The estimates in Table 3 are ratios with the denominator the probability of returning to work without use of the ES. Because the probability of returning to work varies greatly over a jobless spell, it is difficult to determine by how many weeks ES-use reduces the duration as time unemployed increases. Katz and Jacobson's analysis suggests that ES placements reduce joblessness by about 2 weeks prior to UI exhaustion and by over 13 weeks after UI is exhausted.

Table 3. Hazard Ratios of Reemployment for ES-Users relative to Non-Users among UI Claimants who were Not Recalled to Former Jobs, Pennsylvania, 1978-87, from Katz-Jacobson study.

	Men	Women
Placements	1.52* t=(7.3)	1.71* (8.3)
Referrals not leading to placements	1.22* (4.3)	1.32* (4.9)
Other services	1.00 (0.6)	1.09 (0.6)

* Indicates estimate is statistically significant at five-percent confidence level.

Note: The hazard ratios in this table reflect the average quarterly difference in the probability of ending a spell of joblessness among UI claimants not recalled who received ES services versus similar claimants who did not receive ES services.

The key results shown in Table 3 are that:

1. Placements have a far stronger effect on ending a spell of joblessness than referrals or other services. Claimants who accept a job to which they were referred are over 30 percentage points more likely to return to work than claimants referred to jobs that did not lead to placement.

2. Placements have a stronger effect on women than men. Women placed by the ES are 71 percent more likely to return to work than women who did not use the ES, while placed men are 52 percent more likely to return to work than non-user men.

3. Referrals not leading to placement are less potent than those that lead to placements, but still have strong effects. Women referred, but not placed, are 32 percent more likely to return to work than similar non-users. Men, referred, but not placed, are 22 percent more likely to return to work than similar men.

4. Other services (counseling and testing) alone appear to have no statistically significant effect on the duration of joblessness.

That placements have stronger affects in reducing joblessness than either referrals or other ES services is a very important indication that ES placement services are responsible for the positive outcomes. Although not shown in Table 3, a high proportion of claimants unemployed for six months or more who return to work are placed by the ES. That strongly suggests the information provided by the ES was crucial to obtaining a job. Had placements been less important, use of the

ES might only be a "marker" for high search intensity. That is, other things equal, claimants who most intensively search for work might use the ES along with other job finding methods. ES-use would then be correlated with superior outcomes, but its causative effect on job search overestimated.

Separate tabulations showed that the probability of ES use and placement increases as UI benefits are used up. This suggests that the ES refers claimants to jobs that pay less than the perceived returns from collecting UI and continuing to search through other means. The authors suggest that claimants also may have unreasonably high expectations about the pay of available jobs.

That referrals not leading to placements have a positive and statistically significant effect suggests that the ES is providing these individuals with valuable information about the characteristics of available jobs. ES-referred registrants who do not receive offers, or reject offers, more rapidly accept jobs found by other means. Presumably, the experience of using the ES, even if it does not lead to a placement, helps registrants recognize that other vacancies are worth taking because they are unlikely to do better by continuing to search.

That the ES is much more likely to locate acceptable jobs for women than men suggest the ES is most effective in placing workers at relatively low paying jobs. Women placed by the ES accepted jobs paying about $6,500 a year compared to $10,700 a year for men. In comparison, female non-users earned about $8,400, and male non-users find jobs paying about $15,400.

That the ES places workers at low wage jobs does not necessarily imply the ES is ineffective. The appropriate measure of effectiveness is the difference made by the ES not the level of placements or pay. Non-users also have higher pay levels prior to becoming unemployed. It is likely, therefore, that low paid workers have less capacity to locate suitable jobs without ES assistance. It also generally is the case that the ES would be most effective if it focused its activities on workers who have had trouble finding work on their own, and therefore, are most in need of aid.

Katz and Jacobson also analyzed the effect of ES services on earnings in each of four years following reemployment. Comparisons were made between registrants aided by the ES versus claimants who did not receive services but who were unemployed at the point services were provided to the users. That technique understates the earnings gain of ES-users because it ignores their quicker return to work. Taking into account the increase in earnings due to being unemployed for a shorter period would boost first year earnings of ES users by 20 percentage points or more above those of non-users.

In addition, the estimates probably are biased downward because the comparisons include workers recalled to previous jobs who are unlikely to use the ES. A more appropriate comparison limited to workers who change jobs or leave the labor force is in progress.

A regression model, similar to that used in non-experimental program evaluation, was employed to hold constant observable variables that influence the outcome of the search process. Demographic, UI-collection, and work history characteristics were included.

The inverse Mills ratio of the probability of becoming reemployed was entered to control for potential bias introduced by omitting from the analysis workers with "zero earnings." That is, workers who do not return to work or enter employment uncovered by the UI data base.[9] Finally, the Heckman-Robb adjustment was used to control for selectivity bias stemming from the correlation between ES-use and unobserved variables (such as poor alternative information sources) that also influence post-unemployment earnings.

The key result shown in Table 4 is that, contrary to commonly held views, being quickly placed by the ES did not come at the expense of receiving lower-pay. Indeed, the first-year earnings of women placed by the ES were considerably higher than non-users' earnings. Also, fourth-year earnings of men and women placed by the ES were higher than non-users' earnings.

The earnings of those referred but not placed and those given other services, however, were considerably lower than similar non-users in the first year. Earnings growth in the second and third year was greater for users than non-users. Thus, by the third year earnings were higher for men who

[9]About 85 percent of the sample returned to Pennsylvania firms covered by the administrative records. Other studies with Pennsylvania data suggest that zero-earners are evenly divided among those: (1) employed in uncovered sectors (primarily self-employment or employment outside of Pennsylvania), (2) dropped-out of the labor force, and (3) working in covered employment, but had their social security numbers misrecorded. The results are only slightly affected by use of the censoring correction, suggesting incomplete employment histories are not a serious problem.

Table 4. Annual earnings of ES-user relative to non-user claimants, Pennsylvania, 1978-87, from Katz-Jacobson study.

	Post-Unemployment Year:			
	1	2	3	4
Men				
Placements	.99	.97	1.02	1.02
Referrals not leading to placements	.86	.94	1.10	.94
Other services	.89	.93	1.02	1.05
Women				
Placements	1.07	.99	.95	1.06
Referrals not leading to placements	.92	.96	.98	.99
Other services	.84	.87	.99	.92

were referred or received other services than similar no-users, and about the same for women users than non-users.

Probably the effect of placements on first-year earnings is positive and the effect of referrals not leading to placements negative simply because better paying jobs were more likely to be accepted. Since ES-users are likely to have had inferior alternative information sources relative to non-users, we would expect that, when the ES was unable to find acceptable jobs, ES-users found jobs on their own which paid less than jobs obtained by similar non-users.

Importantly, the subsequent earnings growth of those referred, but not placed, contradicts the widely held notion that starting out at relatively low paid jobs leads to dead ends. The subsequent narrowing of the difference in earnings between those placed versus those referred, but not placed, suggests that both groups had similar earnings capacity. In contrast, had the authors found large, persistent differences between the two groups that would imply that those referred, but not placed, had unmeasured characteristics that made them less desirable employees.

In summary, the Katz-Jacobson analysis strongly suggests that the ES is effective in reducing joblessness of UI claimants. Those gains are particularly strong after UI is exhausted and generally stronger for women than men. Direct placements have by far the greatest positive effects, but referrals that do not lead to placements also appear to provide valuable information about the availability of high paying jobs that also shortens the period of joblessness.

In addition, placements lead to earnings gains. Thus, the more rapid returning to work of those placed does not come at the expense of reduced subsequent earnings. Initial earnings for ES-users who are referred but not placed are lower than for similar non-users. Most likely that is a result of ES-users having inferior information sources, not unmeasured quality differences. Importantly, the difference in earnings between those referred and similar non-users diminishes over time. That suggests the ES not only gets users back to work sooner, it puts them on a path to earnings parity with non-users.

4.3 Jacobson's Analysis of ES Activities in Washington State.

The final study discussed in this section is Jacobson's examination of UI payment reductions generated by Wagner-Peyser funded ES activities 1983-91 and a state funded Claimant Placement Program (CPP) 1985-91 in Washington State. CPP was run by the ES, providing the same types of service, but was targeted solely on claimants having difficulty finding work.

The analysis was funded by Washington State to develop accurate measures of CPP benefits to firms paying a special payroll tax to support the program. This is the only study that provides methodologically sound estimates of UI trust-fund savings resulting from ES services. It also provides useful basic information about ES operations needed to understand performance measurement in general.

The estimation procedure was derived from the Katz-Jacobson analysis, but much less time has been devoted to this work. As a result, the analysis is not fully refined. The study also used UI/ES administrative records, but Washington State data includes considerable more detail than Pennsylvania data. Key additional variables describe education, occupation, and the reason for separation.

The study measured the net savings generated by all CPP services together, as well as the individual savings from ES and CPP referrals and placements. To measure the net effect a regression was run on the number of UI payments holding constant demographic, work history, UI benefit entitlements, local ES-office and labor market characteristics. The key coefficient indicates that on average receiving CPP services saved 1.25 weekly UI payments relative to claimants eligible for CPP, but who did not receive any services. (The standard error of the estimate was only .36 weeks.)

Given that 79,800 of the 266,280 claimants designated eligible for CPP received services in Washington State from 1985 through 1991, the net number of UI payments saved was 100,150. Given the average payment was $168, the total savings was $16.8 million. This is about $2 million more than the cost of the Claimant Placement Program over this period.

The accuracy of the savings estimate hinges on eligibles with identical observed characteristics having identical expected payment durations independent of whether or not they received services. What is clear is that the program was able to target claimants who received about 2.3 more UI payments than claimants not designated eligible for CPP services, even after a host of factors were taken into account. What cannot be ruled out at this point is whether, despite the program's intentions, there is a bias in the direction of providing services within the CPP eligible population to those claimants more likely to locate jobs in the absence of ES aid, other factors equal.

The effectiveness of referrals and placements were examined in much greater detail. Table 5 shows that ES and CPP activities early in an unemployment spell saves by far the most payments.[10] The savings declines steeply over the first three months, levels off, and then declines to close to zero as benefits are exhausted.

[10]The estimates in Table 5 were based on comparisons between UI claimants and claimants receiving services from the ES or CPP. In contrast, the net savings measure compared claimants designated eligible for CPP but who did not receive service to CPP eligibles receiving services. Estimates of the effectiveness of CPP services are greater when the sample is restricted to CPP eligibles probably because claimants likely to be recalled were rarely declared eligible for CPP services.

Table 5. Weeks of UI Payments Saved by Washington State Placements and Referrals by the Employment Service (ES) 1983-91 and Claimant Placement Program (CPP) 1985-91 over Five-Week Intervals from Jacobson's study.

	CPP		ES		Adjusted R-square
	Referred, not placed	Placed	Referred, not placed	Placed	
Claim-weeks:					
0-4	.98* (.32)	2.49* (1.13)	1.04* (.22)	1.19* (.63)	.09
5-9	.66* (.24)	1.48* (.73)	.69* (.17)	1.43* (.41)	.11
10-14	.34 (.18)	.66 (.49)	.36* (.14)	.87* (.30)	.20
15-19	.19 (.13)	.30 (.34)	.42* (.10)	.63* (.21)	.37
20-24	.25 (.10)	.70 (.25)	.20* (.08)	.63* (.17)	.50
25+	.14 (.06)	.26 (.13)	-.03 (.05)	.06 (.08)	.48

* Statistically different from zero at 5-percent confidence level.
Standard errors are in parentheses.
Note: Each line represents the results of a separate regression.

The effectiveness of ES and CPP referrals and placements are about equal, and the effect of placements about twice as great as referrals not leading to a placement. The only major exception is that ES placements made in the first month of a UI claim are half as effective in reducing payments as CPP placements, and about equal in effectiveness to referrals not leading to placements. Perhaps CPP more rapidly reached claimants likely to take permanent jobs than the ES.

The savings in UI payments are about equal to the Katz-Jacobson estimate of the reduction in unemployment of UI claimants who do not exhaust UI. The greater reduction in payments early in the spell does not contradict the Katz-Jacobson finding that the probability of ES-use and placement increases as UI benefits are used-up. As shown in column 2 of Table 6, the reduction in payments is greatest following actions early in a UI claim simply because more payments remain to be made early in a claim.

Table 6. Shift in Number of ES-CPP Participants, UI Payments, Probability of Being Referred and the Referral Leading to a Placement as a UI Claim Progresses in Washington State 1983-91 from Jacobson's study.

	ES and CPP Participants Collecting UI at the end of period. (1)	Remaining number of payments to claimants collecting UI at end of period. (2)	Probability of being referred during period. (3)	Probability a referral leads to a placement. (4)
Period: (claim-weeks)	(percent)	Mean (standard deviation)	(percent)	(percent)
0-4	78	12.1 (8.7)	1.2	8.3
4-9	75	8.8 (7.8)	2.0	10.7
10-14	59	6.4 (6.3)	2.8	12.9
15-19	38	3.7 (5.1)	3.2	13.6
20-24	22	1.6 (3.7)	4.2	14.6
25-30	0	0.0 (1.9)	5.4	18.3

Further, column 3 of Table 6 shows that the receptivity to ES referrals quadruples between the first and sixth month of unemployment. Column 4 shows that the probability a referral leads to a placement triples between the first and sixth month. Similar increases were found in the Pennsylvania study.[11]

[11]In contrast to the Pennsylvania study, the duration of joblessness and earnings of CPP and ES users in Washington was not examined. Such analysis, however, would be highly desirable. It could determine: (1) whether the positive effect on payments of referrals not leading to placements is associated with a more rapid return to work or more quickly acknowledging being out of the labor force, and in general (2) determine if the quicker return to work has any negative effective on subsequent earnings.

It is noteworthy that at comparable points in the claim spells the estimates of payments saved shown in Table 5 are only a small fraction of the average number of payments remaining to be made in Table 6. That UI payment savings are small relative to payments remaining implies that the ES aids claimants who are likely to find work on their own.[12]

The large difference between the estimated effect of ES actions and UI payments remaining also strongly suggests that the "Vermont Model" dramatically overstates the true value of ES activities. The Vermont model is commonly employed by state UI/ES systems. It's core assumption is that a claimant placed in week "n" would otherwise go on to collect the average number of remaining payments made to claimants who did not end their benefit collection by week "n". Indeed, Washington State officials became skeptical of results generated by the Vermont Model because savings estimates reached implausibly large levels as the CPP program became fully operational.

In summary, the Washington State analysis suggests that ES activities substantially reduce UI expenditures. Precise estimates, however, require estimation of a more refined model such as the Cox proportional hazard model. In addition, the mechanism by which payments are reduced merits additional work. There are two distinct ways payments can be reduced. One is by leading claimants not meeting the active search requirements to stop claiming benefits, while the second is by helping claimants return to work sooner.[13] Both the extent to which ES actions affect enforcing the UI

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[12] As the spell continues past UI exhaustion, however, it appears that claimants not finding jobs through the ES are likely to remain unemployed for a very long period.

[13] Several studies support the view that ES actions to help workers find jobs more rapidly save UI payments by encouraging claimants not seeking work to stop collecting UI. A study of the Arizona work-test by Jacobson and Schwarz-Miller using detailed micro-data showed that claimants failed to return to work following indefinite denial of UI payments based on the refusal of suitable work. (An indefinite denial requires claimants to return to work for a specific period before reestablishing UI entitlement.) Since job seekers not receiving UI should more quickly return to work than otherwise identical UI claimants, failure to return to work is strong evidence that denied claimants were not actively searching for work.

The study also showed that definite (one-week) denials had no effect on UI payments or job search outcomes. Perhaps most importantly the study indicated that the increased enforcement of work-test violations leading to indefinite denials caused UI claimants not screened to either search assiduously or voluntarily end UI collection. Several subsequent studies, such as Blank and Card's, lends support to the view that work-test stringency affects the probability jobless workers not searching for work will claim UI.

Precisely what role the ES plays in enforcing the work-test has not yet been clearly delineated by research. Practioners claim most refusal-denials are a result of being "turned-in" by creditors or employers setting-up job interviews for claimants. There is only one study I am aware of that focuses on the interaction between work-test enforcement and ES referrals and placements: Corson and Nicholson's Evaluation of the work-test and the return to work merit investigation. In particular, earnings effects should be examined to make sure that the savings in trust funds are not at the expense of taking lower paid jobs than would otherwise be obtained.

4.4 Summary of All Three Studies.

The studies discussed above suggest that direct placement services provided by the ES substantially reduce the duration of joblessness. ES services also appear to have small positive effects on subsequent earnings. Because the cost of ES services is extremely low, the ES appears to be highly cost effective.

Importantly, referrals have a positive effect whether or not they lead to placements, but referrals leading to placements are more than twice as effective as other referrals in reducing joblessness.

Charleston Claimant Placement and Work Test Demonstration. The authors concluded that: (1) the demonstration substantially reduced the number of UI payments; (2) the primary cause of the savings was denials following failure to carry-out special requirements to report to the ES for referral, job development, and job search workshops; and (3) although referrals and placements were dramatically increased among those who showed-up for the special services, those activities had little, if any, effect on job search outcomes.

The authors did not pin-down the precise role of the ES in generating benefit denials. Rather conclusions were based on two pieces of indirect evidence: (1) although fewer UI payments were received by claimants selected to receive special treatment, their return to work and subsequent earnings were no different than that of a control group receiving ordinary services; and (2) most of the difference in UI receipt was due to claimants selected for special treatment ending UI benefit collection prior to making the required ES-office visit within two weeks of the initial UI payment.

Those results are consistent with the view that ES-use by claimants should be voluntary (as it is in Pennsylvania). Instead of mandating that the ES provide supportive services, large UI trust fund savings should accrue from UI personnel systematically calling-in and interviewing UI claimants about their job search.

On the other hand, Corson et al's evaluation of the New Jersey UI Reemployment Demonstration Project concluded that the services provided were associated with a quicker return to work and higher earnings. Unfortunately, neither the Charleston nor the New Jersey studies directly estimated the independent influence of indefinite denials, definite denials, ES referrals, and ES placements, although that would have been feasible with the data on-hand. Thus, it is possible that the causal mechanism that leads to reductions in UI payments, and, an increase in earnings in the New Jersey demo, was the added burden of periodically reporting in-person to UI or ES offices, plus the increased threat of benefit cut-off.

Possibly the difference in job search outcomes arose because routine New Jersey UI administrative practices much more strongly discouraged filing of UI claims by jobless workers who were not assiduously searching for work than did South Carolina practices. That less than 5 percent of the New Jersey claimants failed to show-up for periodic interviews, compared to over 20 percent of Charleston claimants supports the view that far more Charleston than New Jersey claimants felt their claims could not survive close scrutiny.

The studies suggest that the effectiveness of the ES is greater for women than men, and greater after UI benefits are exhausted than prior to exhaustion. The very positive benefit cost ratio of 1.6 developed by Johnson et al, therefore, is remarkable given the six month follow-up excluded the post-exhaustion period of most of the UI claimants studied.

A major weakness of these studies is that they do not tell us very much about how effective the ES is for workers that lack the work history of UI claimants, especially economically disadvantaged registrants. The results hint that the ES is effective in aiding registrants who are willing to accept relatively low paid jobs, but far more definitive analysis would be of great value.

The studies also suggest the ES plays an important role in reducing UI payments. Additional work is needed to more accurately estimate UI payment reductions, and examine how ES activity affects the UI work-test, the return to work, and subsequent earnings.

Perhaps the most important conclusion of these studies is that the ES should be viewed as a "backstop" aiding job seekers who lack good information about the pay and location of jobs, or have failed to find jobs using other job search methods. Recognition that the ES acts as backstop explains why simple comparisons between registrants referred to jobs by the ES versus other apparently similar job seekers show that ES registrants have poorer job search outcomes.

A related central conclusion is that accurate measurement of ES effectiveness is extremely challenging because it is difficult to measure the quality of information each job seeker possesses. Use of sophisticated econometric models that control for negative "selection-bias" due to unmeasured differences in information are essential to produce anything close to reliable estimates of ES effectiveness.

Holding constant the elapsed duration of job search at the point ES services are received as suggested by Katz and Jacobson coupled with omitting recalled workers from the analysis and using a three to four year follow-up period appears to be sufficient to control for selectivity bias and obtain reasonably accurate measures of the ES's effect on jobless duration.

More work is needed to determine how best to estimate the influence of the ES on earnings. In particular, recalled workers need to be excluded from the analysis, and various methods for dealing with unmeasured heterogeneity need to be tested. Those methods include the Heckman-Robb adjustments and use of fixed-effect models.

5.0 MEASUREMENT OF FACTORS THAT INFLUENCE REFERRALS AND PLACEMENTS

The preceding section focused on measuring the ability of the ES to reduce joblessness and save UI payments. The analysis of the amount of effort required to provide services for different registrants was rudimentary.[14] But most policy decisions require knowledge both of the value of the services provided to different registrants and the cost of providing those services. For example, priorities on how much effort should be expended to help different groups of job seekers should be set so as to equalize the returns to an additional dollar spent on each different registrant groups.

Knowledge of the how the ES's difficulty in making placements is influenced by factors both within and outside its control also are of enormous value in developing: (1) reasonable performance expectations across local offices, (2) evidence on what innovations work best, and (3) measuring the performance of state and local management.

Because information about state measurement systems is not readily available, this section examines two research efforts that analyzed the influence of various factors on the probability the ES will place a given job seeker. One is a series of studies conducted by the General Accounting Office (GAO) under the direction of Robert Rogers. The GAO, which is the US Congress' audit and evaluation agency, assembled a nationwide data base on the operations of the universe of 1,772 ES offices, and then collected very detailed data on a sample of 438 offices. Both data bases cover Program Year 1986 (PY-86, July 1, 1986 through June 30, 1987). The second study was conducted

[14] Johnson and Jacobson estimated the benefit-cost ratios simply by dividing aggregate benefits by the total amount of funds spent on the entire labor exchange programs examined. There was no attempt to examine how or why costs differed across local offices or different registrants.

by Louis Jacobson and used administrative data covering over 80,000 individuals who registered with the Pennsylvania ES between 1978 and 1987.

Both efforts measured the effect of environmental factors, such as local labor market conditions and the population mix, on the ability of the ES to make placements. Rogers' study went much further measuring the effects of specific administrative practices. Nevertheless, both studies are highly complementary because of differences in the unit of observation (local office versus individual registrant) and differences in the range of variation observed (time-series versus cross-section).

Both efforts are also similarly motivated. The Federal government substantially loosened its control over the ES and slashed its budget in 1981, while boosting expenditures on JTPA-provided training. There was considerable support for the complete "devolution" of the ES to the states, but, as noted by William Gainer, director of employment issues for the GAO, in the late 1980's the negative view of the ES began to be challenged:

> "... it is generally acknowledged that over the last two decades the position of the Employment Service in the nation's employment and training strategy has eroded. Recently, however, experts on employment and training issues have questioned the limited role of ES in the nation's employment policy."

It is disappointing that, since 1980, much of the literature on the ES (even by academics) consists of descriptions of the ES's deficiencies, and policy prescriptions to remedy the alleged problems--usually abolishing the ES or sharply changing its function. But by-and-large those "studies" lack an appropriate analytic framework, and do not present relevant evidence needed to test stated hypotheses. Thus, a major motivation of the work by the GAO, Jacobson, and Katz was to accurately measure the effectiveness of the ES and put the ES's achievements into a relevant analytic context.

5.1 Rogers' GAO Studies.

The first GAO study examined variations in local office performance using data on the 1,550 offices that provided adequate data out of the universe of 1,772 local ES offices. The key conclusion was that:

"... even after adjusting for differences in economic and demographic conditions, local offices and states varied greatly in their ability to place applicants in jobs. ... [L]ocal office performance is more than a random occurrence and the policies and practices of individual states may contribute to the variation in performance."

Eleven of the 47 states providing adequate data placed 23 percent or more of their applicants. Yet 24 states placed 16 percent or fewer of their applicants. The cost of ES services per applicant averaged about $56 (in Wagner-Peyser funds) for both above and below average performing states, but the cost per placement was about $308 in above average states compared to $400 in below average states.[15]

In essence, the initial GAO study reached the important conclusion that the variation in performance was only partly explainable by environmental factors outside of the control of the ES. It then launched a second study to find out precisely what was the effect of various factors under the control of the ES on productivity.

The GAO collected detailed data on management procedures in all states and 438 local offices, and used those data to reach several highly provocative conclusions:

1. States that: (1) set measurable performance goals reinforced by awards for achieving results, and (2) assessed local office performance through annual on-site visits, had placement rates that were double those of states lacking those attributes.

2. Placement rates were 44 percent higher in offices where managers were involved in many client services compared with offices with less manager involvement.

3. Offices with a self-service system placed 20 percent more applicants in permanent jobs compared with offices where job seekers could only see job lists with the help of ES staff.

4. Placement rates were about 20 percent higher in ES offices located apart from the Unemployment Insurance office compared with locations where these offices were collocated and shared the same managers. (This split may cause ES to be recognized as an "employment" office, thus making the office more attractive to employers and jobs seekers.)

[15]In its initial study the GAO noted that states which were more effective in making placements did not spend more Wagner-Peyser funds per registrant, but later they found that availability of additional funds from other sources had a positive effect on adoption of efficiency enhancing procedures.

5. Local offices that spent more time communicating with employers had 12 percent higher permanent placement ratios compared with offices with less frequent employer contacts.

6. Offices that held individual intake interviews had placement rates 24 percent higher than offices that used group intake.

7. More ES involvement with other placement or job training programs also was associated with better performance. Local offices with more interaction with JTPA had a seven percent higher placement-wage ratio for economically disadvantaged applicants than offices with little contact with JTPA.

8. State and local offices with greater resources were more likely to employ practices associated with better performance. The ability to supplement Wagner-Peyser funding, which declined by 14 percent between 1984 and 1991, with other sources of funds played a key role in securing the management and other staff needed to boost effectiveness. The extent to which states were able to find supplemental funds varied considerably-- from 50 percent of total expenditures in some states to less than 20 percent in others.

The overall conclusions were that:

> "the Secretary of Labor should work with the states to identify and solve problems affecting ES program quality and performance. In addition, Labor should increase technical assistance activities to promote program quality and share information on effective local practices. This leadership role should recognize the states as equal partners in program management, yet spur state action to improve program performance, when needed."

GAO also recommended that:

> "the Secretary assist states in the development of measurable goals and performance standards for their ES labor exchange functions. Meaningful goals and standards should be state-driven and tailored to local conditions and needs."

To reach these conclusions the GAO examined the importance of close to 50 different variables describing the operations and environment of 438 local offices. The analysts worked hard to distinguish direct and indirect effects. They did a credible job in describing many key sets of interactions, although they apparently did not have the resources to fully test all important hypotheses.

In particular, we know very little about the interaction between high quality performance and the ability to attract funds from other sources.[16]

Table 7 shows regressions presented in the report's appendix describing the statistically significant factors associated with variations in: (1) local office placement rates, (2) permanent placement rates (the ratio of placements at jobs expected to last more than 150 days to all placements), and (3) placement-wage ratio (the ratio of the average hourly wage of placements to the local area's average wage for all jobs).

These are not the regressions used to draw the above conclusions, but demonstrate the power of the information obtained to shed light on key issues.[17] For example, it is clear that management involvement in many facets of ES operations had a powerful effect on the placements and wages. A separate regression (not shown) showed that management involvement was strongest where there were more managers, more non-managerial staff, and where the state ES set performance goals.

Table 7 also shows that labor market conditions, measured by the unemployment rate, had a powerful influence on placements and wage levels of placements. Applicant characteristic also are important. As expected, economically disadvantaged applicants are more likely to obtain low wage, "temporary" jobs; youths and migrants are easy to place, but also tend to take temporary jobs, presumably mostly low-wage summer slots.

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[16] One finding that requires further analysis is the conclusion that ES interaction with JTPA programs leads to more positive outcomes. It is possible that the actual causality is in the opposite direction-- JTPA staffs may elect to establish close ties only with highly effective ES offices. Pinning down the value of ES/JTPA cooperation is particularly important given that it appears wages for JTPA participants placed by the ES are considerable higher than wages of similar economically disadvantaged applicants who did not participate in JTPA programs.

[17] The GAO notes that the coefficients displayed in Table 7 are standardized so that variables with larger coefficients indicate more powerful effects than variables with smaller coefficients. Precisely how this was done is not stated. My guess is that regressions were run after the dependent and independent variables were divided by their own means to produce elasticity measures. For example, an increase in labor market size served by a local office equal to one-percent of the average size (641 workers) would decrease placements by .28 percent. For zero-one variables such as ES-UI collocation the relevant comparison is a 100 percent change. Thus, rural offices make on average 21 percent more placements than urban and suburban offices.

Table 7. Statistically Significant Administrative and Environmental Coefficients from ES Performances Regressions using a Nationally Representative Sample for Program-Year 1986 from the GAO Study.

Independent Variables		Dependent Variables		
		Placement Rate	% Placement at Permanent Jobs	Wage at Placement as a % or average
Variable names:	Means:	Significant Regression Coefficients:		
A. Management attributes				
1. Manager involvement	3.7	.17	--	.10
2. ES-UI collocated	1.6	-.15	--	--
3. Automated application	.9	-.14	--	--
4. Employer contacts	7.5	--	.16	--
5. Total staff	13.2	--	.12	--
6. Open listings	65.8%	--	.10	--
JTPA involvement				
7. II-A (disadvantaged)	3.5	--	--	.17
8. III (dislocated)	3.0	--	--	.11
9. II-B (summer youth)	3.2	--	--	-.21
B. Exogenous attributes				
1. Unemployment	8.2%	-.31	--	--
2. Rural location	33.0%	--	--	--
C. Mixed environmental/ program attributes				
1. Size of labor force	64,182	-.28	--	-.24
2. Applicants/labor force	16.7%	-.23	--	--
Applicant characteristics				
3. Youths	21.7%	.28	-.19	--
4. Migrants	.5%	.24	-.19	--
5. Women	43.2%	--	--	.37
6. Disadvantaged	11.3%	--	-.18	-.25
7. UI claimants	34.3%	--	--	-.17
Adjusted R-square		.35	.26	.31
Mean of dependent variable		17%	66%	53%

Description of Management Attributes:

Manager involvement - the number of different client service activities from 0 - 9 in which the local office manager was involved.

ES-UI collocated - 0 = not collocated, 1 = collocated with separate managers, 2 = collocated with a single manager.

Automated application - 0 = manual processing (mostly personal interviews), 1 = batch processing (mostly group interviews), 2 = fully automated (mostly computer transfer of information from UI and other agencies).

Employer contacts - sum of intensity rating on scale of 1-3 (3 highest) for use of each of four contact methods: phone, mail, visits, conferences. Range 4-12.

Total Staff - full-time equivalent managerial and non-managerial workers.

Open listings - percentage of job openings that can be viewed without staff assistance.

JTPA involvement - number of different activities from 0 - 7 conducted in cooperation with agency running each of three JTPA titles.

Perhaps the most interesting finding is the strong support for the view that the ES is most effective when it has the flexibility to provide personalized services to voluntary applicants. In particular, the strong negative coefficients on the size of the labor market served suggests that offices serving relatively small populations are far more effective than offices serving large populations. Quite likely the size variable picks-up the negative effect created by recent trends to combine offices due to budget cuts.

The strong negative association between placements and a high applicant to labor force ratio suggests that having to make many mandatory registrations reduces the ability to make placements.[18] The strong negative association between wages and UI claimants registered also suggests that mandatory registration creates a drag on ES resources. The same conclusion can be drawn from the large negative coefficients in the placement equation on ES-UI collocation and the impersonal automated application.

These results also support the hypothesis that many UI/ES offices are so strapped for resources just to register UI claimants and welfare recipients they have almost no resources left for maintaining a high quality labor exchange.

Also of considerable importance, the ES is shown to make higher wage placements in rural than in urban/suburban locations. This suggests that the labor exchange mission of the ES is much more valuable where obtaining information by other means is more difficult. It appears that rural offices deal with a broader range of applicants and jobs. Since rural offices also tend to serve smaller populations, the rural/urban coefficient may understate the wage difference because the labor-force-size variable is in the regression as well.

Finally, Table 7 provides strong evidence that ES offices able to devote more resources to working with employers are much more effective in obtaining vacancies for permanent positions.

[18]The logic behind this conclusion follows from recognizing that if registration was voluntary, high applicants ratios would be associated with a positive effect on placements stemming from effective offices drawing in above average numbers of job seekers. The reverse result suggests that high applicant ratios are a result of involuntary registrations stemming from requiring above average numbers of adults to register with the ES.

Additional evidence (not shown here) suggests that "job development" activities are highly effective, but most offices simply do not have the resources to perform that task.

The bottom line is that the GAO's major effort to obtain and carefully analyze detailed information about ES operations strongly suggests that: (1) reductions in resources have played a major role in reducing effectiveness, (2) the ES is charged with many activities that detract from the effectiveness of its labor exchange, and (3) despite these disadvantages, changes in management procedures could dramatically improve performance.

In short, an objective appraisal suggests that the ES is highly effective in many areas, and overall effectiveness could be greatly increased by careful attention to monitoring ES activity and adopting a number of specific productivity enhancing improvements. The ES has had its budget cut and suffered from neglect from the Federal government in large measure because its critics used an inappropriate framework for evaluating the contribution of the ES relative to other employment programs.

5.2 Jacobson's Study of ES Placements.

Louis Jacobson examined factors that influenced ES placement probabilities, and thereby, put the ES's strengths and weaknesses into an appropriate context. The study employed the same Pennsylvania data base later used to carry-out the Katz-Jacobson studies described above.

At the outset it was known that standard measures of ES performance had fallen dramatically over the preceding 20 years. Many observers claimed that the reductions in the proportion of registrants placed and proportion of vacancies filled was evidence that the ES's labor exchange function was outmoded. There was a widespread call for building new government institutions and/or dramatically altering the ES's activities.

Yet, viewed objectively, it seemed that the ES was providing useful services at low cost. In program-year 1985 the ES filled 3.4 million vacancies at a cost of about $200 per placement. Perhaps 60 percent of those vacancies were filled by applicants eligible for JTPA assistance. In contrast, only about 470,000 job seekers entered employment after receiving JTPA services. About 85 percent of JTPA participants found jobs at a cost of about $3,000 per job obtained.[19]

In order to investigate the reasons ES performance declined, Jacobson examined a number of factors that could affect performance including changes in: (1) registrant characteristics, (2) labor market tightness, and (3) ES funding.

Table 8 shows that the placement rate in Pennsylvania from 1978 through 1987 averaged 16.2 percent. The placement rate was as high as 29.5 percent for economically disadvantaged registrants not receiving AFDC. It was as low as 8.0 percent for job stayers, registrants who returned to the pre-registration employer. Perhaps of greatest importance, 47.0 percent of registrants were either job stayers or economically disadvantaged individuals whose registration was mandatory under the WIN

Table 8. Referral and Placement Rates for All Pennsylvania ES Registrants 1978-1987, Job Stayers, Economically Disadvantaged Registrants and Job Takers, from Jacobson's Study.

	All	Job Stayers	Economically Disadvantaged		Non-Stay Non-WIN	Job Takers
			WIN	Non-AFDC		
1. Referral Rate	32.8	21.4	21.4	47.6	42.9	49.1
2. Placement Rate	16.2	8.0	11.5	29.5	21.8	28.6
3. Match Rate	49.3	37.1	53.7	61.9	50.8	58.3
4. % of Registrants	100.0	21.9	25.1	10.3	53.0	17.8

Note: Conventional ES measures divide the total number of placements or referrals by the number of registrants in a given program year. In contrast, the figures in this table measure the probability a given registrant would be placed or referred at least once. Thus, these measures are lower than conventional measures because multiple receipt of the same service is ignored. Also, the match rate measures the probability a registrant referred one or more times will accept at least one of those jobs. The job stayer/taker distinction is based on comparing the employer following the end of a program year, if any, to the employer prior to that year's registration.

[19]The JTPA entered-employment rate assigns credit for a "placement" regardless of how participants found their jobs, while the ES only receives credit for placements at jobs to which registrants were referred. The ES's much more stringent performance measure substantially understates its accomplishments relative to JTPA's. In fact, as many as a third of JTPA participants may be placed by the ES, and both institutions are credited with those placements.

program. Both groups were very difficult to place, and clearly the inception of WIN increased the proportion of hard-to-place registrants. Thus, part of the fall-off in performance is traceable to increases in registrations of hard-to-place individuals.

Section 2 of Table 9 shows that the placement rate fell from 22.6 percent in program-year 1979 to 9.4 percent in program-year 1983. Much of that decline reflected the enormous amount of job loss and economic restructuring that occurred in western Pennsylvania. The placement rate rebounded during the recovery, but only reached 13.3 percent in program-year 1986. Since economic conditions were similar in PY79 and PY86 other factors must account for the 41.2 percent decline in the PY86 placement rate relative to the PY79 rate.

Table 9. Comparison of Referral and Placement Rates for Pennsylvania ES Registrants in PY79, PY83 and PY86, from Jacobson's study.

	Comparison Between:		
	PY79 and		PY83 and PY86
	PY83	PY86	
1. Referral Rate			
a) PY83/PY86	20.8	28.6	
b) PY79	41.8	41.8	
Difference:			
in rate	-21.0	-13.8	7.2
% of PY79 rate	-50.2%	-33.0%	17.2%
2. Placement Rate			
a) PY83/PY86	9.4	13.3	
b) PY79	22.6	22.6	
Difference:			
in rate	-13.3	-9.4	3.9
% of PY79 rate	-58.4%	-41.2%	17.3%
3. Match Rate			
a) PY83/PY86	45.1	46.4	
b) PY79	54.2	54.2	
Difference:			
in rate	-9.1	-7.8	1.3
% of PY79 rate	-16.8%	-14.4%	2.4%

Note: Employment growth and decline across Pennsylvania firms were similar in program-year 1979 (PY79) and PY86, but employment declines in PY83 were extremely large, rivaling those during the Great Depression.

A major factor was that ES funding fell by over 10 percent between PY79 and PY86, while registrations increased by over 15 percent. Also in PY86, the ES was still recovering from larger cuts that occurred in the early 1980s. It is reasonable to conclude that the 25 percent reduction in resources per registrant coupled with shifts in the proportion of hard-to-place registrants and structural changes in the industrial mix accounted for most, if not all, of the decline in the placement rate.

Finally, because the Pennsylvania database used by Jacobson included a five percent sample of all workers, not just ES registrants, it was possible to examine differences in earnings of job seekers placed by the ES versus those finding jobs by other means. That comparison provides perspective on the common complaint that the ES "only" places workers at low wage jobs.

Line 3 of Table 10 shows that overall the ES placed 6.1 percent of all job seekers hired at jobs, and it was most successful in placing workers at jobs paying between $5,000 and $10,000. Placements at jobs paying above $20,000 were relatively rare. But line 6 of Table 10 shows that only 10.6 percent of all job seekers located jobs paying $20,000 or more.

Other methods of job finding were only a bit more successful than the ES in locating high paying jobs. However, as discussed earlier, highly educated and/or highly skilled workers are likely to have adequate sources of information needed to find the specialized slots they seek, and therefore, less likely to need the assistance of the ES. Indeed, highly paid workers are most likely to receive leads from professional associates, want ads, and private employment agencies.

It is hard to see how the ES, given its low level of funding, can effectively augment the search of high wage workers looking for just the right slot. Given the ES's relatively meager resources, it is remarkable that the ES was able to place about 6.1 percent of all job seekers in Pennsylvania, and had listings sufficient to refer 10.5 percent of all individuals taking a new job.

Perhaps the key lesson from Table 10 is that, as shown on line 6, most newly hired workers have low earnings. About 45 percent of Pennsylvanians who took new jobs in a given year earned less than $5,000. Even the GAO, which concluded the ES was effective in making placements, compared the hourly wage of those placed by the ES to the average wage of all workers, rather than the more appropriate comparison group of all newly hired workers.

Table 10. ES Referrals and Placements as a Percent of All Hires in Pennsylvania 1978-87 by Post-Hire Earnings Levels, from Jacobson's study.

	Post-Hire Earnings Level					
	1	2	3	4	5	6
	1-4K	5K-10K	11K-20K	21K-30K	> 30K	All
1. ES Registrations as a % of Hires	20.5	24.7	23.8	16.7	6.4	21.4
2. Referrals as a Percent of Hires	10.9	12.3	10.4	6.7	1.6	10.5
3. Penetration Rate	6.5	7.2	5.7	3.9	0.9	6.1
4. Match Rate	59.6	58.6	54.6	57.9	55.0	58.3
5. % of Registrants	43.6	29.1	20.9	5.1	1.2	100.0
6. % of Hires	45.4	25.2	18.8	6.5	4.1	100.0

Notes: Earnings are for the year following hire, if the job was still held that year. Otherwise, earnings are for the year of hire.
Penetration Rate: Placements divided by hires.
Match Rate: Workers placed divided by workers referred.

Given the low pay received by newly hired workers regardless of which job finding method was used, and the likelihood workers located high paying jobs have superior sources of information, it appears that the frequent criticism of the ES for making low wage placement is based on an unreasonable comparison.

In summary, Jacobson's work indicates that much of the criticism leveled at the ES is based on inappropriate comparisons. Although ES placement rates have declined, much of the decline can be traced to substantial reductions in federal funding and increased mandates to register hard-to-place individuals such as welfare recipients and UI claimants. In addition, the criticism that the ES primary places workers at low wage jobs is based on comparing ES placement wages to the wages of all workers, rather than the much lower wages of all newly hired workers, and ignores the ability of high wage workers to find jobs by other means.

6.0 SUMMARY OF RESULTS AND SUGGESTIONS FOR FUTURE RESEARCH

6.1 Findings.

This paper described the operating characteristics of the ES, a framework for objectively measuring the ES's effectiveness, and the relevant research assessing the ES's performance. The key points that emerge from this review are that:

1. The ES labor exchange is unique among employment programs in that it places no restrictions on the characteristics or number of job seekers and firms served. At the same time, most UI claimants and many welfare recipients must register with the ES. The ES has a responsibility to monitor the job search of those registrants and to provide evidence of insufficient activity to agencies with the authority to deny benefits.

2. The ES registers about 20 million individuals a year, and places at jobs about 1.5 million of those registrants at a cost of about $80 per registrant. However, the ES is frequently expected to perform as well as institutions that spend more than 30 times as much per person, and can select the individuals served.

3. Analysis suggests that prior to UI exhaustion ES placements reduce joblessness of claimants by about 2 weeks, and by more than 13 weeks for the roughly 20 percent of claimants exhausting UI. Referrals not leading to placements also reduce joblessness, but the effects are less than half that of placements. The effects of the ES are considerably greater for women than men.

4. The earnings of placed ES users are, if anything, slightly increased above what they otherwise would be. The earnings of users who are referred, but not placed, maybe slightly lower initially, but differences diminish to zero (or small gains) over the following four years.

5. On average each ES referral (whether or not it leads to a placement) to a UI claimant saves about 1.25 UI payments or almost $200.

6. The above numbers should be taken as ball-park estimates. Obtaining accurate estimates of the ES is extremely difficult. Simply controlling for readily observable differences in personal, work history, and labor market characteristics between those served by the ES and others is insufficient to control for difference in access to other sources of information, and other difficult to observe characteristics influencing placements, referrals, and job search outcomes.

7. There is strong evidence that the ES is turned to by workers who lack access to the best sources of information or have failed to locate suitable work using other sources. Thus, non-users tend to have better job search outcomes than ES-users in the absence of taking the availability of alternatives into account. That the ES apparently helps individuals particularly

unlikely to be able to help themselves is a strength of the institution, but one that greatly complicates analysis.

8. Because the ES labor exchange grants unlimited access to job seekers and firms it is difficult to believe a random assignment experiment can be used to measure the ES's effectiveness.[20] ES studies have relied on a variety of non-experimental means to take "difficulty finding work" into account. Katz and Jacobson used administrative information about the duration of job search prior to receiving ES services as a key control for difficulty finding work. Johnson-Dickinson-West used survey responses to develop a psychological profile and details about job search expectations to control for factors influencing placements and outcomes. Both studies also used Heckman-Robb style econometric adjustments to further control for "unmeasured heterogeneity."

9. Although the ES appears to be highly cost-effective, there is considerable variation in efficiency across ES offices. Those differences are associated with whether or not the state SESA has implemented a performance measurement system, monitors offices through site visits, and office managers are involved in a range of activities. Also, offices that provide more personalized services to job seekers and firms have higher placement rates, other things equal.

10. Differences in funding and staff levels have a major affect on the adoption of attributes that boost effectiveness. Wagner-Peyser funds going to the ES have fallen by over 20 percent in real terms since 1979. They currently cover only about 60 percent of the ES's budget. States have found alternative funding sources, but the amount of supplements varies from 50 percent of the total budget in some states to 20 percent in others, and these funds are largely earmarked for special purposes and target groups.

11. ES placements are also strongly affected by the characteristics of registrants and local labor market conditions. In particular, mandatory registrants (most UI claimants and many welfare recipients) are difficult to place and seem to reduce the overall effectiveness of the ES's labor exchange.

12. Finally, for job seekers whose previous earnings are less than $20,000 the ES is able to find jobs that generate similar earnings to those found through other means. Typically, the earnings of ES clients are inappropriately compared to those of all workers, most of whom have held jobs for a considerable period, rather than new hires. Again, it is very likely that the ES places few job seekers at high wage jobs because workers able to obtain those jobs have access to excellent information from other sources, and therefore, do not need the assistance of the ES.

[20] It may be possible, however, to use a random assignment experiment to compare the effectiveness of direct placement versus indirect forms of job search assistance in JTPA.

6.2 Gaps in our Knowledge and Implications for Future Research.

All the analysis discussed here is derived from four data bases. Three are micro-data on individual job seekers: (1) DOL's nationally representative survey of 8,000 ES registrants conducted 1980-81, (2) a Pennsylvania ES/UI administrative data base covering close to 80,000 registrants 1978-87 assembled by Jacobson and Katz, and (3) a similar Washington State data base covering about 100,000 registrants 1983-91 assembled by state officials with my help. The fourth is aggregate ES office level data: (4) GAO's very detailed nationally representative sample of 438 local offices supplemented with survey data covering all state headquarters and all 1,773 local offices.

Moreover, only the analyses based on the DOL and GAO's surveys have been published.[21] Nevertheless, the results from the four studies strongly reinforce each other. Hopefully, when complete the Pennsylvania and Washington analyses will provide consistent evidence on the effect of the ES on the duration of joblessness, earnings over four or more years, and UI payment savings of UI claimants. But Pennsylvania and Washington have high UI payment maximums. It, therefore, would be informative to add information from a state with a low benefit maximum, such as Missouri, to round out our knowledge of the effectiveness of the ES for job seekers who are UI claimants or otherwise have strong attachment to relatively highly paid jobs.

<u>One key area, not covered by current work, is the affect of ES referrals on the UI work test.</u> This issue is of great importance because the existing studies suggest that mandatory registration of claimants does not lead to many additional placements and may reduce the probability other referred registrants will be hired and the willingness of firms to list vacancies with the ES. A major untested justification for mandatory registration, however, would be the positive role of referrals not leading to placements in supporting the UI work test. The negative effects an hiring those referred and listing vacancies could be more than offset by the benefits to society of reducing transfer payments.[22]

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[21]The Pennsylvania analysis should be completed by the end of the summer, and Washington State is working with DOL to fund a field test of the value of providing enhanced information to UI claimants that would include completing estimation similar to that in Pennsylvania.

[22]Potential UI claimants as a group could benefit from stringent work-test enforcement because funds otherwise going to individuals able to find work could be used to increase UI entitlements to those unable to locate suitable work. In fact, Western states tend to offer higher UI payment levels and more stringently enforce the work-test than other US regions.

Such a study would be particularly relevant at this time since the SESAs are currently implementing new UI profiling rules requiring mandatory referral to supportive services, including the ES. Thus, it should be possible to evaluate the effects of the work-test, mandatory ES registration, and mandatory referral to services on job search outcomes and UI payment savings.

Because states vary greatly in the stringency of their work test and Pennsylvania is among the most liberal in its work-test administration, a comparison between Washington and Pennsylvania would be useful. It also would be useful to include two additional states with low UI maximums and strong differences in work-test stringency. Consideration might also be given to using the data bases created for the Charleston and New Jersey Demonstration projects (assuming they still exist) to examine the independent effect of indefinite denials, definite denials, ES placements, and ES referrals.

Compared to our knowledge about the ability of the ES to aid UI claimants, we know little about the ability of the ES to aid economically disadvantaged workers. This too is an area of great importance. Economically disadvantaged job seekers are even more likely than UI claimants to lack the information needed to locate suitable work.

In addition, registration for many disadvantaged individuals is mandatory, and since AFDC can be paid far longer than UI the potential transfer payment savings by work-test enforcement for the disadvantaged could be far greater than for claimants. Thus, even though few mandatory registrants are placed by the ES and those registrants appear to adversely effect overall ES performance, the benefits to society of reduced transfers could still offset the costs imposed by reducing placements.

Given the current interest in reforming welfare to encourage work, knowledge about the effectiveness of the ES and the work-test for low income users also would be particularly timely.

The methodology developed by Katz and Jacobson provides sufficiently accurate estimates of the returns to placements and referrals of UI claimants. It is unclear how well those estimation techniques will work for the economically disadvantaged. One key problem is that it is difficult to measure the duration of job search for workers who are only employed intermittently. Similarly, it is difficult to determine the search intensity of such individuals, and even whether or not they are in the labor force.

Analysis of administrative records could be conducted at low cost, but the accuracy of the conclusions probably would be substantially increased by combining a baseline and one-year follow-up survey with use of administrative records for long-term follow-up. Some of the psychological and expectation information collected by Johnson et al had high explanatory value, even if they were not completely successful in eliminating selectivity bias.

Another major gap in our knowledge is the effect of ES actions on non-users. It should be possible to draw inferences on the magnitude and incidence across job seeker groups of the indirect effects from the work of Davidson and Woodbury on UI bonuses. It probably only would be worthwhile to study the indirect effects of ES activity if there was reason to believe the effects are large and/or the incidence falls heavily on non-users receiving UI, AFDC, or other transfers. A parallel analysis of the distribution of benefits and costs of ES services across groups of firms might also be of value.

A final area of enormous importance is acting on the GAO's suggestions for improving ES effectiveness. The strongest recommendation of the GAO was to ensure that all states implement reasonable performance standards and monitor compliance by on-site visits. ICESA has advocated use of traditional measures similar to those used by the GAO, such as the use of placement rates, and the DOL implemented new requirements to collect data needed to track performance of the traditional measures.

The work discussed here, however, suggests that far better measures can be developed than the traditional measures. It would be a major advance to follow the GAO's example and regression adjust the traditional measures to hold constant cross-state or cross-office differences in registrant and labor market characteristics. Because of the complex interaction among factors a lot of additional work is required to determine which variables and estimation procedures should be used for such a system.

A key element of such an effort would be to use the GAO's data and similar data used in several states to determine what model is most appropriate in different situations. Serious consideration should be given to resurveying the GAO's sample of 438 local offices to identify what changes have occurred over the past seven years, and be able to relate those changes to changes in productivity.[23]

Regression adjusted traditional measures have the potential to hold constant factors largely outside the ES's control, and thereby, reveal differences in management effectiveness. It is appropriate to do what the GAO did and try to find out what distinguishes high versus low performers and use the evidence gained from such models as a basis for changing management procedures and administrative rules.

It is of utmost importance, however, to recognize that use of regression adjusted traditional measures as **standards** would not necessarily maximize the net benefits generated by the ES's labor exchange. Regression adjusting traditional measures aids effective management by placing ES offices with a more difficult task (because of adverse local labor market conditions and/or hard-to-serve clientele) on an equal footing with other offices. But it does not provide guidance on which individuals would be helped the most by the ES. Yet, aiding those who are helped the most by the ES would do the greatest amount of good with the ES's limited resources, and should be the basis for resource allocation decisions.

For example, it is widely recognized that it would be unreasonable to penalize local offices with low placement rates unless the rates were adjusted for cross-office differences, even though doing so would maximize the number of placements within a given state. What is often forgotten, however, is that rewarding local offices that are most effective in placing hard-to-serve individuals can be equally unreasonable.

What regression adjusting fails to do is take into account the ability of workers to find jobs on their own. High-wage, job-attached, workers are very difficult for the ES to place because they already hold reasonable jobs and usually have access to excellent information about alternative vacancies. Rewarding offices focusing efforts on job attached workers, therefore, would make little

[23]Ideally, individual records of ES registrants should be collected from the same sample of offices and linked to administrative earnings and benefit collection records. Such a combined data set would greatly enhance our ability to measure the net value added by ES activities and administrative characteristics.

sense. Yet, regression adjusting standard measures would assign equal value to placing those workers as workers who are not job-attached.

The only way to ensure that the value assigned to an ES action is equal to its true value is to know the benefits and costs of various actions. Appropriate measures of benefits include the reduction in joblessness, increases in earnings, and savings of transfer payments relative to the levels that would occur in the absence of ES actions. Appropriate measures of costs include differences in the amount of staff time and other resources required to undertake a given action.

Arguably, developing a performance measurement system that accurately assesses the value of a given activity to job seeker, employer, and society net of the cost of carrying out that action would be by far the most productive step that could be undertaken with the aid of researchers. The research discussed here can be extended to create the relevant measures. My view is that top priority should be given to developing such a system.

Disseminating information about the productivity enhancements uncovered by the GAO and future research also is of great importance. A relatively inexpensive first step would be for the DOL to sponsor discussions with SESAs to consider which recommendations should be implemented and how change can be effectively carried out. In my opinion the potential savings from the concrete recommendations are so great that providing funds for states to demonstrate the value of an array of changes would be well worth the costs. Because the changes are likely to more than pay for themselves it might be possible to "borrow" the start-up costs from Wagner-Peyser trust funds. Also, a carefully crafted RFP could play a major role in making SESAs aware of the ways ES productivity could be enhanced, and leverage demonstration funds with state contributions to encourage change.

The bottom line is that, although few in number, existing ES studies provide a great deal of information that puts the ES's performance into perspective and suggests ways to improve the ES's effectiveness. That work, however, has barely touched on a number of important topics. It is highly feasible to carry-out new work that would not only provide very valuable new information, but very likely lead to dramatic improvement in the ES's effectiveness.

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THE ROLE OF UNEMPLOYMENT INSURANCE IN ADDRESSING STRUCTURAL UNEMPLOYMENT: LESSONS FROM OTHER NATIONS

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ABSTRACT

Most industrialized nations, including the United States, are currently faced with rising long-term unemployment associated with increased structural unemployment as a fraction of total unemployment. The U.S. leads the world in funding demonstration projects designed to test the effectiveness of active labor market policies in combating structural unemployment. However, we have much less experience than many other nations with the actual implementation of these policies on a nationwide basis. This paper examines unemployment compensation system reforms recently implemented in Britain, Australia, and Canada as responses to rising long-term unemployment. Broadly speaking, these reforms are intended to enhance work incentives of unemployment compensation recipients and to strengthen the linkage between unemployment and reemployment services. A number of lessons are drawn from the experiences of these nations that might be usefully considered in reforming our unemployment insurance system.

INTRODUCTION

A recent U.S. Department of Labor (1993) report documents a disturbing upward trend in structural unemployment, where structural unemployment refers to a permanent loss of the old job and difficulty in finding a new job. Since the structurally unemployed are permanently displaced from their jobs, they are also termed displaced workers. Evidence supporting the existence of such an upward trend includes the fact that since the 1950s, long-term unemployment (those out of work for six months or longer) has risen at a faster pace than overall unemployment (see Table 1). For example, the average unemployment rate for the 1990-93 period is only slightly higher than the average rate for the decade of the 1970s, but the proportion of unemployment consisting of long-term unemployment has jumped by nearly half (from 11.0 percent to 16.0 percent). In 1992, moreover, some 76 percent of unemployed job losers did not expect to be recalled to their old jobs (USDOL 1993). The 76-percent level is the highest proportion of job losers not on temporary layoff ever recorded since these data become available in 1967.

The implication drawn from this evidence by USDOL analysts (1993) and many other observers is that traditional short-term income support provided through the unemployment insurance (UI) system is not sufficient to meet the challenge of rising structural unemployment. The argument is that UI income support is well-suited to workers on temporary layoff during a cyclical downturn and to those who can readily find new jobs on their own. At the same time, it is not a sufficient policy response to the needs of the structurally unemployed whose reemployment prospects are not expected to substantially improve with an upswing in the economy. Using the Organization for Economic Cooperation and Development (OECD) distinction between "active" and "passive" labor market policies, the USDOL report goes on to recommend increased funding for an active labor market policy designed to speed up reemployment and improve the long-run earnings potential of displaced workers. The expectation is that if active labor market programs are successful in expediting the reemployment of the displaced, fewer dollars will need to be spent on passive labor market programs like unemployment insurance.

Most of the suggested components of this active labor market policy were subsequently incorporated in the Reemployment Act of 1994 proposed by the Clinton administration in March of that year. Major provisions of this legislation are the following:

- A comprehensive reemployment system designed to assist all jobless workers, regardless of the cause of their job loss. This would replace the current multiplicity of categorical displaced worker programs. Major programs replaced would include the Economic Dislocation and Worker Adjustment Assistance (EDWAA) program, the main program currently supplying reemployment assistance to the displaced, and the Trade Adjustment Assistance (TAA) program, which provides income assistance and retraining to trade-displaced workers.
- One-stop career centers designed to make it easy for the unemployed to access all available reemployment services at one location.
- A nationwide labor market information network.
- Early identification of displaced workers -- termed "profiling" -- so that they can promptly be referred to reemployment services before disillusionment and loss of self-esteem destroy the capacity to carry out an effective job search.[1]
- Long-term retraining services including income support provided to workers seeking to upgrade their skills in order to compete for jobs in growing industries and occupations. Displaced workers enrolled in an

approved training program who had worked at least three years for a previous employer would qualify for up to one year of income support beyond the usual six-month maximum. Workers with one or two years of job tenure would qualify for up to six months of additional support.

- Changes in the UI system to expedite the return to work including reemployment bonuses to claimants who find jobs quickly and measures to promote self-employment.[2]

The Reemployment Act failed to receive Congressional approval during the 1994 session. However, a policy response to the needs of displaced workers is not an issue that is likely to go away.[3] To help in formulating new policies to assist displaced workers, the United States is in the fortunate position of being able to draw on the recent experience of other industrialized nations. Three nations -- Britain, Australia, and Canada -- are focused on in the analysis . The reasons for their selection are two-fold. As described in the next section, (1) each of these nations faced a more severe problem of long-term unemployment than confronts U.S. policymakers today, and (2) each has recently taken action to make its unemployment compensation system more directly responsive to the problem of structural unemployment. The second of these points deserves emphasis. While the U.S. leads the world in funding demonstration projects to test the effectiveness of labor market policy innovations, we have less experience than most other nations with the actual implementation of such policies. As is widely noted in the evaluation literature, effects of a permanent program may be quite different from those of a comparable experiment.

After summarizing the earlier discussion, the final section of the study attempts to draw lessons from the experiences of these three nations that might be usefully considered in reforming our UI system.

UNEMPLOYMENT COMPENSATION REFORMS IN OTHER NATIONS

The English-speaking nations of Britain, Australia, and Canada are like the U.S. in that custom and national labor market policy make it much more likely that employers will respond to demand shocks by laying off workers than by adjusting compensation or hours of work. For their part, jobless workers in these countries are typically expected to prepare for and locate vacant jobs with relatively little government assistance. In contrast, the national governments of most Western European countries support a comprehensive and stable institutional structure through which a wide variety of employment and training services are supplied. Industrial relations systems in these nations typically impose advance notice and severance pay requirements on employers who dismiss workers and encourage hours adjustments in lieu of layoffs.

An important way in which the English-speaking countries mentioned differ from one another is in the resources devoted to income support programs. The unemployment compensation (UC) system of the United States is undoubtedly the least generous of the four. Table 2 raises the important distinction in UC systems between unemployment insurance and unemployment assistance (UA). UI benefits are paid for a limited time period to workers who are involuntarily unemployed and actively seeking new employment. Eligibility for UI benefits depends on a past record of insured employment, and the amount of benefits received often hinges on past earnings. These conditions imply that many individuals may be unemployed but not eligible for UI benefits. Indeed, an important issue facing U.S. policymakers is the downward trend over the postwar period in the fraction of the unemployed receiving UI benefits. Blank and Card (1991) point out that although over 90 percent of employed workers hold jobs that are covered by the UI system, less than 30 percent of unemployed workers currently receive UI benefits. This low ratio of UI claimants to unemployed workers would be less of a concern if all unemployment was associated with labor market entry and reentry. As the authors (1991: 1157) note, however, the decline in the ratio during the 1980s is especially puzzling since it has occurred at the same time that the fraction of women in the labor force has stabilized and the baby boom has matured.[4]

As distinct from unemployment insurance, unemployment assistance is typically of indefinite duration with eligibility for benefits independent of employment history. Moreover, the amount of UA benefits is determined by the level of income and assets of other household members via a means-test. Since unemployment assistance is not conditional on past employment history and may be of indefinite duration, the coverage of UA programs is likely to be greater than the coverage of UI. Table 2 points out that Australia offers a UA program to its workers, while in Britain UI is combined with a UA program to provide eligible workers with income support of unlimited length. Canada and the U.S. both offer unemployed workers an UI program only, but the potential duration of benefits is much longer in Canada. Canada's unemployment insurance program is unusual, moreover, in that benefits are available to voluntary job leavers.

Economists tend to view unemployment compensation in terms of a tradeoff between adequacy of benefit levels and a disincentive effect in discouraging active job search. Atkinson and Micklewright (1991: 1680) warn that this tradeoff is a "dangerous oversimplification" because it assumes that policy can be judged solely in terms of reducing unemployment and increasing employment. Nevertheless, Table 3 shows using OECD data for the second half of the 1980s that Britain, Australia, and, to a lesser extent, Canada all faced a more severe problem of long-term unemployment than did the U.S. In each case, policymakers took steps during the late 1980s or early 1990s to reform their UC systems to address the problem of rising long-term unemployment.

Britain

In 1911 the U.K. became the first highly industrialized country to institute a national compulsory unemployment insurance program. Blaustein and Craig (1977: 224) describe that during the 1920s and early 1930s high levels of unemployment meant that benefit outlays far surpassed the resources generated by the UI program's contributory financing structure. A reform carried out during the mid-1930s placed a limit on benefit duration beyond which workers who were still unemployed and could meet a means-test were eligible for further income support from a new unemployment assistance program. This program became known as Income Support.

Britain's public employment service is responsible for disbursing unemployment benefits and providing employment services including labor market information, job matching, and job placement. Placement service offices -- called Jobcentres -- were separated from unemployment benefit offices in 1973. In 1982 the legal requirement that benefit claimants register for job placement was dropped. Following a period of minimal intervention during the first half of the 1980s, British policymakers began in 1986 to implement programs emphasizing the provision of reemployment assistance services to the long-term unemployed. This programmatic emphasis was strengthened after 1989 by a multi-year program to bring about a gradual merging of the Jobcentres and Benefit Offices networks. The OECD (1993: 19) notes that by the mid-1990s, the network of merged Jobcentres and Benefit Offices will include between 1,000 and 1,200 "integrated" offices offering one-stop access to employment services. Beginning in 1988, the provision of adult training services was made the responsibility of a national network of Training and

Enterprise Councils (TECs). The TEC network represents an effort to decentralize the delivery of training services to the local labor market level and to increase the role of the local business community in designing and operating training programs.

Unemployment benefits. As noted in Table 2, unemployed British workers are eligible for up to 12 months of unemployment insurance benefits. Britain's UI system is unusual in that benefits are unrelated to previous earnings. Reubens (1989) suggests that academic research indicating that higher earnings replacement ratios tended to prolong unemployment led to the abolishment in 1982 of earnings-related UI benefits, leaving only a basic flat rate benefit. Storey and Neisner (1992: Table 3) report for the early 1990s that UI benefits are fixed at £34.71 weekly for a single person and £56.12 weekly for a person with a dependent spouse. Using December 31, 1991 exchange rates, these benefit levels convert to $64.91 (U.S.) weekly for a single person and $104.94 (U.S.) weekly for a person with a dependent spouse. The earnings replacement ratio is therefore quite low, except for persons with low previous earnings.

It is interesting to note that the academic research referred to by Reubens suggests that only quite large cuts in benefits would have much of an impact on length of unemployment spells. For example, a well-known study by Lancaster and Nickell (1980) estimates the elasticity of unemployment spells with respect to the earnings replacement ratio to be about 0.6. This means that a 10 percent rise in benefits would be associated with an increase in length of unemployment of one week, given an average unemployment duration of 17 weeks. Using a larger micro data set, a more recent study by Narendranathan, Nickell, and Stern (1985) concludes that this elasticity is even smaller, lying somewhere in the 0.28 to 0.36 interval.

Estimates of the relationship between duration of benefits and length of unemployment spells are not available for British data. However, evidence on this relationship for Germany has recently been presented by Hunt (1995); and her results are worth noting because the unemployment compensation systems in both Germany and Britain combine UI and UA. Hunt examines a series of statutory changes beginning in 1985 that extended the duration of UI benefits (Arbeitslosengeld) for older workers with considerable work experience. Although most of these workers would be eligible for UA assistance (Arbeitslosenhilfe) once their UI benefits ended, extended UI benefits are preferred by German workers to UA because of UI's higher replacement ratio and the fact that UA benefits are means-tested. Her results indicate that 44-48-year-old Germans remained unemployed longer because of the extension of UI benefits. In addition, the impact of extended UI benefits is estimated to be greater for workers in this age bracket who ultimately dropped out of the labor force than for those who found employment. For 49-57-year-old workers, the UI extension caused those who ultimately dropped out of the labor force to remain unemployed longer, although the estimated effect is smaller than that for 44-48-year olds. Hunt (1995: 111) comments that the large magnitude of exit-out-of-the-labor-force coefficients in comparison to exit-to-employment coefficients suggests that many who left the labor force were not really looking for work, but described themselves as unemployed while receiving UI benefits, and then dropped out of the labor force when their benefits were exhausted.

Returning to British unemployment benefits, Table 2 points out that unemployed workers who have exhausted their UI benefits or who failed to qualify for UI because of a lack of previous employment experience are eligible for the unemployment assistance program Income Support. Assuming no other sources of income, Income Support benefits are fixed at £39.65 ($74.17) weekly for a single person and £62.25 ($116.45) weekly for a couple, plus allowances for children (Storey and Neisner 1992: 26). Benefits may be received for an indefinite period, and there is no requirement that claimants actively look for employment. Income Support claimants are also eligible for a housing subsidy under the Housing Benefit program. Reubens (1989) points out that between 1973 and 1983, the balance of unemployment compensation expenditures shifted from unemployment insurance to Income Support benefits. During the same period, the proportion of the unemployed who received neither UI nor Income Support benefits shrank from almost 25 percent to less than 13 percent. A recent estimate suggests that three-quarters of all unemployed workers receive only Income Support benefits (see OECD 1992: 141).

Because Income Support benefits are related to family size, the OECD (1993: 66) poses the issue that Income Support plus Housing Benefit payments might well exceed after-tax earnings from low-wage jobs.[5] In addition, Murray (1990) observes that among young male Britons, in particular, a belief in the "right" to full unemployment benefits has developed without any sense that this right hinges on a past record of employment and on a reciprocal obligation to actively seek employment. In his view, their high level of economic inactivity recorded in the early 1980s suggests that young men are growing up without being socialized into the world of work.

Given the broad coverage of the unemployment compensation system and Britain's low wages relative to prices, it is not surprising that discussions of the economic malaise affecting many of Britain's industrial cities emphasize the long-term dependency of workers on safety net programs (see, for example, Horwitz 1991). In an important White Paper presented to Parliament in December of 1988, the Department of Employment (1988: 55) writes that

> . . . there is evidence that a significant minority of benefit claimants are not actively looking for work. Some are claiming benefit fraudulently while working at least part-time in the black [or underground] economy. Others seem to have grown accustomed to living on benefit and have largely given up looking for work, despite the high level of job vacancies which are increasingly available throughout the country. Others believe, mistakenly, that they might be financially worse off taking a job or are reluctant to travel daily more than a short distance to where jobs are available.

The government's response to the problem of long-term dependency on the unemployment compensation system took two forms: (1) increase the return to work even at low wages through the Family Credit program and (2) encourage the long-term unemployed to reestablish contact with the labor market through the "scheduling and programming strategy." The first of these responses will be briefly described, while the second will be considered at greater length.

The Family Credit is designed to make most Income Support claimants financially better off working, even at a low wage, than they would be remaining unemployed. The program was initially restricted to individuals working over 24 hours a week on average; in 1992, this restriction was reduced to 16 hours per week. Workers meeting the weekly hours restriction receive a tax-free Family Credit benefit in addition to their labor market earnings, but the benefit is withdrawn at an implicit marginal tax rate of 70 percent applied to net earnings. Since the tax rate is high and the breakeven level of earnings is low, Family Credit payments are most likely to have a sizable impact on family income in cases where wages are low or one wage-earner is supporting a large family. The OECD (1993: 66) estimates that only about half of potentially eligible workers claim the Family Credit. Since 1989, the government has supported advertising campaigns to increase the program's takeup rate. An illustration of this effort is a 1993 Employment Service pamphlet titled "How to be Better Off In Work" that presents five examples in which families in different circumstances would enjoy increased disposable income by taking advantage of the Family Credit.

The scheduling and programming strategy. The broad coverage but low level of British unemployment compensation benefits leads to a situation in which extra encouragement and modest active help might make a substantial difference in the overall propensity of claimants to return to work. In 1986 the Restart program was launched at Jobcentres throughout the nation. Its aim was to revitalize job search activity among the long-term unemployed through a series of personal interviews and the offer of various forms of reemployment assistance. A Restart interview was initially required of individuals who had been unemployed for more than 12 months. The OECD (1993: 85) suggests that the early success of the Restart initiative led to it being extended to those unemployed for more than six months. Interviews are currently scheduled every six months for the duration of the unemployment spell (see Table 4).

In 1988 the role of the New Client Adviser was introduced. For many years previously, new benefit claims typically involved only processing the necessary paperwork. The New Client Adviser offers a new claimant a verbal presentation of the various reemployment assistance options immediately, rather than after months of unemployment. As indicated in Table 4, the initial interview with a New Client Adviser also results in a mutually agreed upon back-to-work plan which lays out a strategy for the claimant to follow in seeking reemployment. If the unemployment spell continues, subsequent interviews are conducted by a Claimant Adviser, who is an expert on in-work benefits and the local job market.

During the first six months of unemployment, most unemployed workers are viewed as being basically job-ready but in need of assistance in locating suitable employment opportunities. This attitude changes after six months when an unemployed individual becomes eligible for the Restart program.

Restart is intended for unemployed workers who may be beginning to doubt the value of continuing to actively engage in job search. As noted, the program begins with an interview with a Claimant Adviser. These interviews are repeated every six months with the same Claimant Adviser so that a good working relationship can be achieved between adviser and client. Depending on the needs of the client, the adviser can recommend a variety of services including a slot in a TEC-operated adult skill training program, a place in a Jobclub, access to the Job Interview Guarantee program, a temporary public sector job under the Employment Action program, financial assistance to set up a small business, and a slot in a Restart Course. Eligible unemployed workers undergoing skill training qualify for continued income support payments plus a training allowance of £10 per week. Among the other available programs, two of the more important are Jobclubs and Restart Courses.

Jobclubs offer workers unemployed for at least six months two weeks of four half-day sessions per week of structured training on how to look for jobs, write more effective job applications, and come across well in interviews. Following these two weeks, clients are given access to "resource areas" containing telephones and sources of labor market information. At this stage, there is more support from other club members and less from the Jobclub leader. Jobclub participation is limited to four to six months to prevent the clubs from becoming used more for their social aspects than for their job search function. There are about 1,000 Jobclubs in Britain, with an average entry of about 150 unemployed persons per club per year. About 200 of the Jobclubs are operated by Employment Service staff, with the other 800 operated by outside suppliers on two-year contracts.

Attendance at a Restart Course is mandatory after two years of receiving unemployment benefits for those claimants who have turned down the opportunity to participate in other programs. Restart Courses are directed at those long-term unemployed persons who have settled into the mind-set that they are unlikely to ever get another job and have adjusted to life on the dole and a low standard of living. In addition to providing information on the range of available job search assistance options, Restart Courses involve small groups of about 12 individuals in exercises intended to overcome barriers to work, sort out career options, motivate the resumption of active job search, and produce an individualized plan of action for reemployment. Courses are typically five days in length.

Fraudulent claims. As might be expected, the most common form of fraud in Britain is continuing to claim Income Support benefits while receiving undeclared earnings which would, if declared, reduce or eliminate these benefits. Atkinson and Micklewright (1989) document for the 1979-88 period that the credible threat of benefit disqualification increased due to a number of steps taken by the British government to tighten monitoring of benefit claims and job search activity. As of 1989, the OECD (1993: 88) reports that more than 10 percent of claimants sent a letter of invitation to attend a Restart interview stopped claiming benefits rather than attend the interview. More recently, experience with compulsory Restart Courses indicates that about nine percent of those scheduled for the courses stopped claiming benefits. While these dropout rates could be reflecting a successful reemployment experience, it is more likely that many who dropped their claim on benefits have access to other sources of income, in which case their Income Support claim could well be fraudulent. Supporting this conclusion is U.S. evidence for the Charleston, South Carolina and Washington state UI reform experiments. (The Charleston experiment is evaluated in Corson, Long, and Nicholson 1985 and the Washington state experiment in Johnson and Klepinger 1994.) This evidence suggests that strengthened work search reporting requirements and mandatory attendance at a job search workshop reduced UI payments. But the saving in UI expenditures was achieved by raising the costs of remaining on UI rather than by enhancing the job search abilities of claimants.

There are also individuals illegally claiming benefits who are undeterred by job search requirements. To combat this form of benefit fraud, the British Employment Service as of 1991-92 maintained a staff of about 1,000 fraud inspectors (in comparison to about 20,000 staff members in other benefit administration tasks). The OECD (1993: 72) reports that in recent years, inspectors investigated 300,000 to 400,000 cases per year leading to 65,000 to 85,000 withdrawals of claims and 3,500 to 4,500 prosecutions. Given that withdrawn claims lead to savings in Income Support payments and Housing Benefits and to increased tax receipts, it seems quite likely that fraud detection more than pays for itself.

Australia

Created in 1944, Australia's unemployment compensation system provides unemployed workers with benefits that vary by income, age, marital status, number of children, amount of rent, and location of residence. Benefits continue for an unlimited period of time. Although it is an unemployment assistance program (see Table 2), Australia's UC system was designed to meet the traditional goal of maintaining the incomes of workers temporarily unemployed during cyclical downturns in the economy. Long-term unemployment was largely unknown, and the system remained essentially unchanged for its first 40 years.

The event that led to the system's revision was the surge in structural unemployment during the recession of 1982-83 (see Jones 1983: 196-97). Left to their own devices, many unemployed Australians laid off during the recession appeared to lack the skills and motivation to take advantage of job opportunities created by the economic recovery of the mid-1980s. The consequence was a period beginning in the mid-1980s and continuing into the 1990s in which Australian policymakers were confronted with the dilemma of labor shortages co-existing with rising long-term unemployment. Table 3 indicates that in 1987 Australia faced a long-term unemployment problem that was less severe than Britain's but more severe than Canada's, while Australia's national unemployment rate was lower than that of either nation.

The policy response to this dilemma is the Newstart program implemented in July 1991.[6] Newstart replaces the existing unemployment compensation program with two new mechanisms for providing unemployment benefits -- the Job Search Allowance paid to workers older than age 18 for the first 12 months of unemployment (and to eligible persons under 18 years of age) and the Newstart Allowance paid beyond 12 months of unemployment. Primary objectives of the Newstart program are to (1) prevent long-term unemployment by early intervention, (2) differentiate the services provided to the short-term and long-term unemployed, and (3) emphasize the reciprocal obligations of benefit recipients. The size of benefits is unchanged under Newstart. Table 5 outlines the timing of Commonwealth Employment Service (CES) interventions in the Newstart program.

The Job Search Allowance. The Newstart program is designed to encourage unemployed workers to actively engage in job search from the time they first apply for unemployment benefits. As indicated in Table 5, eligibility for Job Search Allowance benefits over the first 12 months of unemployment is conditional on meeting requirements, known collectively as "activity testing," to ensure that the unemployed are genuinely seeking employment. For the job-ready, activity testing involves contacting employers about possible vacancies, applying for suitable jobs, and indicating a willingness to accept suitable offers. Benefit recipients must provide the CES with a list of employers contacted on a bi-weekly basis.

Unemployed workers who are determined not to be immediately job-ready may meet the activity testing requirement by making themselves available for a job search assistance or skills training course. Job Search Allowance payments continue while clients are enrolled in an approved training program, and program participants receive a training allowance to help defray out-of-pocket costs of training. Failure to meet the activity test means a temporary suspension of the Job Search Allowance or its permanent cancellation. All clients are interviewed by CES staff members after three months of unemployment and again, if they remain unemployed, after six months.

The Newstart Allowance. Continued receipt of unemployment benefits beyond a period of 12 months is not automatic. After 12 months, the unemployed must apply for the Newstart Allowance if they wish to continue receiving income support. At the time they apply for the Newstart Allowance, all applicants must review their situation with a CES staff member and develop a reemployment plan known as a Newstart Plan and Activity Agreement. The idea behind the Activity Agreement is to diagnose and remedy those problems preventing the applicant from competing successfully in the labor market. The Activity Agreement takes into account the client's education, work experience, and physical capacity in setting employment goals. Then the agreement lays out an action plan to succeed in attaining these goals. Activities that may be proposed by the CES and agreed to by the client include job search training, vocational skills training, paid work experience, and measures designed to eliminate or reduce labor market disadvantages such as medical treatment or rehabilitation. As with the Job Search Allowance, participants in an approved training course receive the Newstart Allowance plus a training allowance.

For their part, each Newstart Allowance recipient must indicate his or her understanding of and agreement to the following statement:

> My Newstart Allowance will continue to be paid only if I take reasonable steps to keep to this Agreement, and to my responsibilities set out in my copy of the Newstart Allowance claim form.

Once an Activity Agreement is in place, Newstart Allowance recipients are required to maintain a record of their activities which is reviewed on a regular basis by a CES staff member (typically, the same CES staff member to encourage a more personal relationship between the CES and clients). Employer cooperation is sought in monitoring the work search effort of recipients. Employer Contact Certificates issued to job seekers by the CES are one form of evidence of active job search. When appropriate, employers may be requested to verify the job seeker's attempts to find work by completing the back of these certificates. Employers doubting the sincerity of a job seeker are encouraged to contact the CES with their concerns so that action can be taken.

An issue discussed earlier in connection with the British Family Credit program was how to make income support recipients financially better off working than they would be remaining unemployed. Under the Newstart program, long-term unemployed Australians are eligible for a variety of inducements to encourage reemployment. To help meet the costs of returning to work, the long-term unemployed are eligible to receive an employment entry payment of $100 (Aus.) when they notify the Department of Social Security (DSS) of their reemployment. At least partial payment of the Newstart Allowance is usually also paid to help tide over the newly reemployed until their first pay check. Continuing help to low-income workers with dependent children is available through the Family Allowance Supplement (FAS). Under FAS, individuals taking low-wage jobs may continue to receive a DSS payment for children and rent assistance in addition to their earnings. Finally, individuals who accept a short-term job are eligible for immediate resumption of the Newstart Allowance, when their job ends, without serving another waiting period.

Atkinson and Micklewright (1991: 1719) suggest that the $100 employment entry payment may be viewed as a reemployment bonus, albeit a reemployment bonus with a long qualifying period (since it is available only to the long-term unemployed) and a modest bonus payment. Underlying the reemployment bonus concept in general is the notion that a bonus payment for finding a new job quickly helps overcome the disincentive of unemployment compensation claimants to engage actively in job search. Japan is a noteworthy example of a nation that has implemented a reemployment bonus program offering a strong incentive to find reemployment quickly (see OECD 1993: 59-60). Implemented in 1984, the Japanese reemployment bonus is paid to claimants who start a new job within the first half of their benefit entitlement period, where the benefit entitlement period ranges between 30 and 120 days. Depending on the original benefit entitlement and the timing of the start of the new job, the bonus is equivalent to between one-third and two-thirds of the remaining benefit entitlement. For example, a claimant eligible for 90 days of benefits who finds a new job within 45 days will receive a lump-sum payment of 30 days of regular benefits. Prior to 1985, reemployed claimants received a payment (called an "outfitting" allowance) to help pay for the costs of starting a new job.

For the United States, evidence on the effectiveness of a reemployment bonus in expediting reemployment has been gathered from a series of four random-assignment experiments. In the original experiment in Illinois, new UI claimants who found a job within 11 weeks of their initial claim received a $500 cash bonus. The evaluation of the Illinois experiment by Woodbury and Spiegelman (1987) indicates that UI benefits were reduced by a striking $2.32 for every $1 paid out in bonuses to claimants. Based on these highly favorable results, the USDOL funded additional experiments in New Jersey, Pennsylvania, and Washington state designed to provide evidence on how to fine-tune the reemployment bonus concept by introducing more variation in the amount and timing of bonus payments. As it turned out, however, the three more recent experiments all provided estimates indicating much smaller effects (see Meyer 1992). In particular, estimated effects of reemployment bonus plans on weeks of UI payments are about the same as those achieved, but at much lower cost, by more closely monitoring work search behavior in the Charleston and Washington state job search experiments referred to earlier.

Canada

Canada's UI system. The passage in 1940 of the Unemployment Insurance Act established Canada's UI program with the primary objective of providing insurance against risk of income loss due to unemployment. Coverage was restricted to workers with a previous labor market attachment who faced a risk of unemployment and who were willing and able to work. As noted earlier, included among covered workers are those who quit their previous job. Also in 1940, the National Employment Service (NES) was created to provide labor market information and placement services designed to aid the return to work of the unemployed. These services are delivered through a national network of Canada Employment Centers (CECs). The NES is also responsible for enforcing the work search requirements of the Unemployment Insurance Act.

Between 1940 and 1971 only gradual changes were made in the UI program, mainly in the direction of extending coverage to previously excluded groups. Much more dramatic changes occurred in 1971. These changes included the following:

1. A substantial increase in coverage, so that virtually all wage and salary workers came under the UI Act.
2. An increase in the earnings replacement ratio to 67 percent of pre-tax earnings (with 75 percent provided to claimants with dependents who had low earnings or prolonged unemployment).
3. `An increase in the maximum benefit, with the maximum benefit subsequently indexed annually to changes in average wages.
4. A reduction in the minimum period of previous employment required to qualify for benefits.
5. Introduction of a benefit structure raising duration of regular benefits to a maximum of 43 weeks depending on claimants' previous work experience. Regular benefits could be followed by extended benefits the duration of which depend on the national unemployment rate and the number of percentage points by which regional unemployment rates exceed the national unemployment rate.
6. Making benefits taxable.
7. The introduction of new benefits in cases of earnings interruptions due to sickness, maternity, and retirement.

Green and Riddell (1993: S106) suggest that the most important of these changes in terms of their labor market impacts were the reduction in the minimum qualifying period and the increase in the maximum duration of benefits. Commenting on the minimal period of previous employment required to qualify for UI benefits, the Wall Street Journal (1993: A8) writes:

> [U]nemployment insurance is so generous that in some parts of the country it has become a way of life: three months of work and nine months on the dole. In the little province of Prince Edward Island, the

> New Democratic Party lays off its leader for three months each year so he can collect federal unemployment benefits and save the party about $3,100.

Green and Riddell also point out that the introduction of regional extended benefits represented a shift in objectives away from a pure insurance program toward concern with altering the regional distribution of income.

The 1971 amendments were followed by a rapid expansion in UI expenditures throughout the 1970s and continuing into the 1980s. By the second half of the 1980s, Table 6 indicates that the ratio of Canadian passive program expenditures to Gross Domestic Product (GDP) was closing in on the ratio for Britain. As described earlier, Britain is well-known for its problem of large numbers of people living more or less permanently on the dole. The ratio shown for Canada is considerably higher than that for Australia and much higher than the ratio for the U.S. (Sweden is included in the table as an example of a Western European nation with a high ratio of total labor market expenditures to GDP but with a quite different mix of active to passive programs compared to the English-speaking countries.)

Table 7 compares Canada and the United States on important characteristics of their respective UI systems. The table shows that unemployed Canadians enjoy a higher earnings replacement ratio and receive UI benefits for a longer period than unemployed Americans. But the most important consideration explaining Canada's greater UI expenditures is the near certainly that an unemployed Canadian will receive UI benefits, whereas just over one-quarter of unemployed Americans are UI recipients. One factor involved in explaining the difference in UI receipt is that, as noted, workers who quit their previous job are covered in Canada but not in the U.S. Also important is the decline in insured unemployment in the U.S. since 1982 (see Blank and Card 1991).

After the 1971 amendments, expenditures on UI benefits grew from about $700 million in 1970 to almost $2 billion in 1972 and almost $12 billion in 1983. The rapidly rising cost led to a number of proposals for UI reform. In 1985, the Macdonald Commission suggested returning UI to its original social insurance objective while recommending the creation of a comprehensive negative income tax plan to supplement incomes of the poor, especially of the working poor. The Forget Commission in 1986 also recommended focusing the UI program on insurance objectives and developing a separate comprehensive income security program. Large federal government deficits during the second half of the 1980s made it difficult to implement the more comprehensive program of income support recommended by the two commissions as a necessary component of UI reform. Instead, the Canadian government embarked on an alternative strategy of shifting resources away from passive income support programs to active labor market policies including job training, employment subsidies, and job search assistance. This strategy has been strongly recommended by the OECD in various publications throughout the 1980s and into the 1990s (see, for example, OECD 1990).

The 1989 Labour Force Development Strategy. In 1989 the Canadian government released a White Paper describing the problems of structural unemployment and perceived skill deficiencies among Canadian workers and outlining the components of a new policy initiative -- the Labour Force Development Strategy (LFDS). The White Paper describes that (Employment and Immigration Canada 1989: 1-2):

> . . .[M]ore than one million workers experience difficulty in finding work every year as a result of a lack of skills. The consequences of unemployment, for individual Canadians and the country as a whole, cannot be overestimated. The damage to self-esteem and confidence that arises from extended spells without work often makes the process of looking for a job harder and longer. . . .

When older workers lose their jobs, they often have a more difficult time finding new ones. Extended service with a particular firm, the expectation of continued employment, the length of time since undertaking formal training, and family commitments combine to exacerbate the difficult process of adjustment.

The policy response put forward in the LFDS has the purpose of tightening up on UI eligibility while, at the same time, allowing a reallocation of UI resources from income support to active labor market policies. The main changes in the UI program include the following.

1. Increased eligibility requirements. While retaining the UI program's sensitivity to differences in regional unemployment rates, a new UI benefit schedule increases the minimum weeks of employment required to become eligible for UI benefits in most regions of the country. For example, the minimum weeks worked requirement for workers in Saskatoon, Montreal, and St. John's would increase from 10 weeks to 13 to 16 weeks under the new regulations.

2. Reduction in the maximum duration of benefits. The maximum period of UI entitlement is reduced from 50 weeks except in those regions suffering very high unemployment rates. For example, claimants who lived in regions with an unemployment rate of 11 percent and who had worked for at least 30 weeks were entitled to 50 weeks of UI benefits. Under the new regulations, these claimants would qualify for just 42 weeks of UI.

3. Penalties for voluntary job leavers without just cause. Workers who quit their jobs without just cause are still eligible to receive UI benefits, but they face a seven- to-12-week delay before benefits commence, and the duration of their benefits is reduced. In addition, the before-tax replacement ratio for insurable earnings is cut to 50 percent from 60 percent. There is no penalty imposed on those who leave their employment for just cause, such as hazardous working conditions, following a spouse to a new location, and sexual harassment.

WAGE INSURANCE:
A POLICY REVIEW

by

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I. THE ECONOMIC PROBLEM

Although the ideal family consumption stream may vary over time as family size and family composition change, it appears to be stable over periods of intermediate duration. Families prefer that consumption not fluctuate greatly year to year or even month to month. Similarly across states of nature, families appear to be risk averse, preferring secure consumption to a fair gamble, at least over large gambles. Unfortunately earnings, the principle source of income of working families, are not stable over time. Worker fortunes ebb and flow; they gain jobs and lose them; they receive raises and suffer pay cuts. Fluctuations in an industry's demand for labor are common and sometimes predictable (e.g. seasonal factors), while business cycle and firm idiosyncratic demand fluctuations are frequent but often less predictable. An important policy question is the extent to which private and public mechanisms designed to shape stable consumption streams from unstable earnings are successful, and, if they are not, what policy reforms might improve consumption smoothing mechanisms in the United States.

The focus here is on the unemployment insurance (UI) system and recent proposals to reform and extend the current system.[1] Of special interest then are earnings shocks induced by job displacement or involuntary job separations.[2] Typically the displaced worker faces a period of unemployed job search during which earnings are zero, followed by a lengthy period of earnings recovery as many new jobs offer reduced hours or wage rates. The current unemployment system has focused on the first of these problems, but the second is important as well. In a recent study of UI administrative data from the state of Pennsylvania, Jacobson, LaLonde, and Sullivan (1993) document the long term earnings impact on one of the most vulnerable groups, high-tenure prime age workers. The authors summarize their findings with:

> ...high-tenure prime age workers endured substantial and persistent earnings losses when they were displaced from firms with substantial employment declines. Even in the fifth year after separation, their quarterly earnings remained $1,600 below expected levels. This loss corresponds to approximately 25 percent of their 1979 earnings....there is little evidence that displaced workers will ever return to expected levels. Clearly, displacement is a major setback for experienced workers. (p.10)

Large earnings losses following displacement are not limited to high-tenure prime age workers. Data from the National Longitudinal Survey of Youth (NLSY), a panel survey of approximately 12,600 youth who were 14 to 21 as of January 1, 1979, reveals that wage rate declines as well as employment difficulties follow job displacement.[3] In Table 1 I report the distribution of real wage growth by turnover status between the 1981 survey and the 1982 survey for male respondents employed at the time of the 1981 survey. (For respondents not employed at the time of the 1982 survey, the wage reported on their "next job", derived from the 1983 survey, was substituted.) Job stayers, not surprisingly, reported the greatest stability in real wages--almost fifty percent of stayers report a real wage change of less than ten percent in absolute value while only 19 percent report wage losses of ten percent or more.[4] Quitters were the most likely to experience large wage gains, almost fifty percent report significant real wage increases (10 percent or more), although quitting was not without risk--29 percent of all quitters experienced real wage losses of more than 10 percent. Involuntarily separated workers report the bleakest wage outcomes. Laid off workers report both fewer large wage increases--32 percent--and more large wage declines--34 percent--than did quitters.

Even large earnings fluctuations need not translate into consumption fluctuations. Private capital and insurance markets can in theory bridge the gap between the reality of unstable earnings and the objective of stable consumption. If the fluctuations in earnings are predictable, then saving and borrowing alone should be sufficient to smooth the consumption stream. Groups as diverse as farmers, construction workers, and elementary school teachers face periods during the year when earnings are zero. Indeed with farmers the zero income period may be most of the year. Small, uncorrelated random shocks to earnings can also be efficiently absorbed by appropriate saving and borrowing strategies.

The capital market is seriously incomplete, however, with borrowing possibilities especially constrained. Moreover for some types of earnings shocks, the capital market is not efficient. Large, low-probability earnings shocks or smaller, but highly correlated ones are more efficiently absorbed by insurance. Unfortunately the private market for earnings insurance is largely limited to life insurance because of the adverse selection and moral hazard problems that have hampered the development of broad markets for other types of insurance.

The public response to the incompleteness of these private financial markets has been the establishment of an unemployment compensation system. Whether this public response is itself sensible depends on the ability of the government to overcome the problems that limit private initiatives in this area. The compulsory nature of the public system eliminates one major problem in the private market, adverse selection by those most prone to unstable earnings. The moral hazard problem remains, however: many individuals would prefer not to work if they could continue to be paid. Indeed the form of the UI program, with its deductible (the first week of unemployment) and its limited benefits and duration, clearly reflects concerns about the moral hazard problem.

The question is whether the combination of private savings and insurance and public unemployment compensation and welfare programs permit working families to smooth variable earnings into stable consumption streams. Several studies have recently attempted to assess the extent to which American families are "insured" against fluctuations in income, whether through formal insurance programs or informal ones, including family support networks and private charity, Mace (1991) and Cochrane (1991).[5] Although Mace concludes that consumption insurance in the United States is surprisingly complete, Cochrane notes several areas of insurance failure. Specifically Cochrane concludes, "[The hypothesis of] full insurance is rejected for long illness and involuntary job loss, but not for spells of unemployment, loss of work due to strike, and an involuntary move." (p.974). In a study more directly focused on unemployment shocks, Gruber (1994) estimates that UI benefits reduce by two thirds the fall in food consumption during spells of unemployment (although they have no impact on post unemployment food consumption levels) and he raises the possibility that UI benefits may be overly generous, at least as the system is currently designed.

In the remainder of this paper I explore several proposals to improve earnings insurance against involuntary job loss. These proposals include ones designed to improve consumption smoothing over time (saving/borrowing) as well as over states of nature (insurance). Before introducing the proposed reforms, it will be useful to examine more carefully the structure of the labor market, the financial markets, and the current UI system. In the next section I introduce the major features of the current system in an informal way. Then in Section III I present a more formal model of the system. Three proposals to reform and extend the current UI system are then outlined in Section IV and subject to critical discussion in Section V. The paper concludes with a discussion of unresolved problems in the design of earnings insurance systems.

II. THE ECONOMIC BACKGROUND

The Labor Market. The labor market is characterized by a great deal of uncertainty--jobs are won and lost, promotions and demotions earned--although there appears to be considerable structure underneath the randomness. The market can be usefully modeled as a "dual" labor market, with one set of jobs offering high wages and stable, full-time employment and another lower wages and less stable, often part-time employment.[6] The split appears to be driven both by the institutional structure of the firm, especially its real or perceived vulnerability to unionization, and by the firm's own need for a stable workforce, perhaps due to heavy job training costs.

Job separations are higher in low wage firms not only because the firm is more likely to lay off workers but also because the worker is more likely to quit. As a consequence there will be more vacancies in equilibrium in the least desirable firms. A great deal of empirical evidence speaks to the accessibility and flexibility of part-time work. In a classic paper, Munts (1970) lays out the odd part-time work incentives of the partial benefit provisions of the UI system in Wisconsin and two other states and the predictable adjustments workers make in response to those incentives. Similar findings routinely appear in the social security literature, in which older workers eligible for social security benefits target their work activities to fall just below the earnings test level, e.g. Sander (1968). Indeed it appears that it is only in situations of extreme unemployment that the worker cannot patch together SOME type of employment, although perhaps at a cost in wages and various fixed costs of obtaining multiple jobs.

In this framework, unemployment is voluntary but full employment is not necessarily optimal. A worker employed full-time at two or more part-time jobs is likely to have little opportunity to search for stable, high wage jobs. Depending on i) the discount rate, ii) the probability of securing a high wage job and iii) the high wage/low wage differential, it may pay the worker to remain unemployed or to take only one part-time job in order to search for a premium wage job. Of course obtaining a premium wage job is not guaranteed, even if the individual searches full-time, and in the end the unsuccessful searcher is likely to accept a low wage job in the secondary market. In this environment, the average earnings of displaced workers are likely to be depressed both during and after the spell of unemployment.

To introduce a distributional issue that will be important in later discussions, workers who secure employment in the high wage sector are more likely to demand earnings insurance than are those in the secondary market. In many ways the latter group is "protected" from truly large wage losses by the labor market itself while those employed in the primary market are not. A key aspect of job search is the decision to gamble on finding a job slot in a high wage, stable firm versus patching together one or more shorter term, lower wage jobs. Even full-time workers in the low wage sector occasionally hear of better jobs, so there is a trend toward workers securing jobs in the high wage sector as they age. Experience effects on earnings appear to be a combination of this upward drift and of the effects of experience on productivity which in turn affect market wages. So older, high tenured workers are especially vulnerable to wage declines if they lose their jobs.

Financial Markets. Consumption may be stable despite erratic earnings if capital and insurance markets are complete. Unfortunately they are not. The capital market, for example, is severely constrained. Borrowing against future earnings is a problem for many working families. Human capital is notoriously poor collateral, particularly so when the worker is unemployed and the future flow of earnings is especially uncertain. Spreading a negative shock over future periods may not be feasible.

Large/low probability random shocks are not efficiently absorbed by saving and borrowing behaviors in any case. It is simply not optimal for a worker to build up assets sufficient to smooth the arrival of a large/low-probability earnings shocks. If the shock never arrives, consumption has been seriously distorted. Such shocks may be optimally smoothed through pooling or "insurance" if the shocks are uncorrelated across participants in the pool. Unfortunately the private earnings insurance market in the U.S. is also seriously incomplete. Business cycle risk is uninsurable in the ordinary sense of the word; no pooling across individuals can eliminate an aggregate economic contraction. That problem aside, moral hazard is a serious problem in most earnings insurance plans. Earnings are rather easy not to have--a lack of effort will usually do it--so that verification of the reason for the earnings loss is often an important part of the insurance contract. Such verification is often expensive and may be uncertain even with considerable effort.

In the absence of complete capital and insurance markets, consumption stability is not obtainable in the private sector. In such a world, there are stern pressures for an unemployed worker to find a job, the most accessible of which are likely to offer relatively unattractive hours and/or wage rates. **It is important to note that the laissez faire outcome (work and search activities) with incomplete capital and insurance markets is not optimal. It is a second best solution, one that may well encourage undue haste in job search.** If the government can provide alternatives to these markets inexpensively, such action would be warranted.

The Unemployment Compensation System. The current unemployment insurance system was established in part to compensate for the incompleteness of various financial markets. The system partially protects workers against earnings losses during periods of involuntary idleness. Benefits rarely compensate wholly for lost earnings and are limited in duration with standard benefits exhausted after 26 weeks. The limited generosity of the unemployment program is in part the result of moral hazard problems, which are well recognized in the unemployment insurance literature, although there is much debate over the magnitude of effects, e.g. Atkinson and Micklewright (1991). The literature has focused on two specific problems: 1) a reduced propensity to leave unemployment because the economic returns to securing a job or leaving the labor force are reduced, and 2) an increased propensity to enter unemployment through layoffs if the system is incompletely experience rated (the program subsidizes severance pay, which firm and worker can use to their joint advantage).[7]

Most policy attention has focused on the first question, the increased duration of unemployment in an unemployment compensation regime. Several investigators, exploring the optimal UI design question, have proposed that unemployment payments be front-loaded so that workers receive a redundancy payment upon job loss and a lower payout for incremental weeks of unemployment, Bailey (1978), or a continuously declining with incremental weeks of unemployment, Shavell and Weiss (1979). See also Hopenhayn and Nicolini (1994). The logic is straight-forward. If each displaced worker is given a lump sum payment at the time of displacement equal to his or her expected wage loss, with no additional payments for unemployment of greater duration, the worker has no artificial incentive to extend the period of unemployment (although there will be an income effect of uncertain magnitude). Reemployment bonus proposals are a special case of the redundancy payment, with the payout conditional on speedy job search.

Clearly lump sum payments are poor insurance against adverse realizations of unemployment spells. The nature of this problem is most clearly revealed in the reemployment bonus program in which the lump sum payment is ONLY paid out to the successful job seekers. The winners receive additional winnings. At the same time, as Meyer (1994) has stressed, the front-loading of payments may exacerbate the second potential distortion highlighted in the literature, the artificially increased incidence of layoffs. If experience rating is incomplete, some firms can channel public resources to their employees by increasing the incidence of short term layoff. Of course it is important to note that neither the unemployment duration "distortion" nor the layoff rate distortion are necessarily social costs. As noted earlier, in a world of incomplete capital and insurance markets, the duration of unemployment and the rate of layoff in a pure market system may be suboptimally small.[8]

Recognizing that a small amount of work is not likely to impede job search yet increases family income, most states offer a partial benefits option in which some work can be undertaken without forfeiting all benefits. In the typical state program, some earnings are "disregarded" and benefits are reduced dollar for dollar as earnings rise above the disregard level, Unemployment Insurance Service (1991). The disregard is generally quite small and the impact on the unemployed probably correspondingly modest.

III. WAGE INSURANCE AND THE LABOR MARKET: A SIMPLE MODEL

In this section I develop a formal model that captures important elements of the previous discussion. Consider a model of the labor market in which the individual participates for three periods before retiring. There are two types of jobs in this economy, denoted high wage (H) and low wage (L), so

$$W_H > W_L$$

where W denotes the full-time wage in each sector. High wage jobs are full time and relatively stable, with a displacement rate of λ, $0 \leq \lambda << 1$.) They are also difficult to obtain, requiring significant search activity with no guarantee of success. Displaced workers can search while totally unemployed or while working half-time but not while working full-time. If the individual searches full time (does not work), the probability of finding a high wage job is θ, $0<\theta<1$. If he searches half-time and works half-time, the probability of securing a high wage job in the next period is $\kappa\theta$. Reflecting the possibility of diminishing returns to job search, assume that $1/2<\kappa<1$.

Jobs in the low wage sector are half-time and temporary, but, reflecting their relative unattractiveness, plentiful. A half-time job pays $0.5W_L$. A worker can patch together two half-time jobs to create full-time employment at a wage W_L. Each low wage job lasts only one period, although that is not a problem because such jobs are readily available-no explicit search activities are required to secure one.

The worker is an expected utility maximizer, where

$$EU = \Sigma U_i, \qquad i=0,1,2 \tag{1a}$$

where:

$$U_i = \beta^i [u(C_i)-b_i], \tag{1b}$$

where β is the time preference parameter, $0<\beta\leq 1$, C is consumption, and b the disutility of work or search. Assume that $\lim_{C\to 0} u(C) = -\infty$, $u'(C)>0$, and $u''(C)<0$. Assume also that the disutility of work and search are the same so $b_i=b$ $(b>0)$ if the individual works or searches in period i, and $b_i=0$ if the individual does neither, instead choosing leisure. Assume that b is sufficiently small that the individual will not choose to take leisure in normal market circumstances.

The individual's strategies are a function of his current employment circumstance. In particular, only workers currently employed in the high wage sector have the option of continuing in that job. Indeed individuals employed in the high wage sector who have not been laid off will always choose to remain in their current jobs. Their optimal strategy is:

S0: remain in high wage job til laid off (available only to individuals currently employed in the high wage sector).

All other workers, those not currently employed in the high wage sector, have the following menu of possible strategies in each period:

S1: search full-time and no work;

S2: search half-time and work half-time in the low wage sector;

S3: work fulltime (no search) in the low wage sector.

The worker is assumed to choose the strategy that yields the highest expected utility. Note that search in the current period is required if a job in the high wage sector is to be secured in the next period unless the individual is already employed there. In the final period, period 2, search will never be optimal. The individual will work full-time, either in a high wage job or a low wage one. The individual's choice of strategy will be strongly dependent on the structure of the financial markets as well as the labor market.

Consider first an economy with complete and costless financial markets. Not only can the individual borrow and lend but he can obtain insurance against any adverse market contingency, most prominently the loss of a high wage job and/or the inability to secure a new one. Note that there is no public insurance program in this economy nor is one needed. The individual's three period pattern of work and search and his saving/borrowing/insuring activities will be a function of his starting position in period 0 and especially on whether he is employed in the high wage sector. For those not employed in the high wage sector, period 0 activities are endogenous, and will depend on the economic environment, especially the crucial wage parameters W_L and W_H and search parameters θ and κ. Job search, for example, will be attractive if the wage differential $(W_H - W_L)$ is large, the probability of securing a high wage job (θ) is large, and the returns to search approximately constant (κ=1/2). Part-time employment and part-time search will be more attractive than full time search as the wage differential narrows and as the returns to part-time search increase (κ>>1/2).

On the financial side, the model yields a particularly simple solution if the lending and borrowing rate is 0 % and the time preference parameter β is one: the individual will choose the same level of consumption in each period and state of nature, namely the expected value of lifetime wealth divided equally across the three periods, where expected wealth is defined for the set of work/search activities that maximize expected lifetime utility. All risks will be insured away at their expected values and consumption will be equalized across periods by borrowing and lending. Consider some of these optimal activity and consumption streams in greater detail.

INDIVIDUAL EMPLOYED IN THE HIGH WAGE SECTOR IN PERIOD 0. The individual employed in the high wage sector in period 0 will continue in that job as long as he is not laid off. Layoff probabilities (λ) are nonzero between period 0 and period 1 and again between period 1 and period 2, however, and because all layoffs are permanent in this model, the laid off worker has the same decision path as any other worker not currently employed in the high wage sector. He can i) accept full-time work in the low wage sector or ii) work part-time in the low wage sector while searching for a high wage job or iii) search full-time for a high wage job and not work at all. If for example the optimal program is to accept full-time work in the low wage sector if released from the high wage sector, then his expected lifetime wealth is:

$$EW(HW_0) = W_H + \beta\{(1-\lambda)W_H + \lambda W_L\} + \beta^2\{(1-\lambda)(1-\lambda)W_H + (1-(1-\lambda)(1-\lambda)W_L\}. \quad (2)$$

If β=1 then consumption in each period is one third of this or $C^* = EW/3$. To achieve this consumption objective, the individual must 1) engage in two insurance activities, insuring against layoff at the end of period 0 and again at the end of period 1 if he is not laid off in period 0, and 2) reallocate resources from the first period to later periods in which the expected value of income is less. Note that the insurance is entirely over reemployment wages, not unemployment, because by assumption the individual's optimal strategy is to accept employment in the low wage sector immediately upon being laid off from the high wage job.

Depending on the parameters of the system, other post-layoff strategies may be optimal and may involve periods of unemployment and even other types of insurance. For example consider the individual employed in the high wage sector in the first period who calculates that full time searching for a high wage job will be optimal in period 1 if he is laid off at the end of period 0 (again recall that job search will never be optimal in the final period). In this case the expected value of lifetime wealth is:

$$EW(HW_0) = W_H + \beta\{(1-\lambda)W_H + \lambda 0\} + \beta^2\{(1-\lambda)(1-\lambda)W_H + \lambda(1-\lambda)W_L + \theta\lambda W_H + (1-\theta)\lambda W_L\}; \tag{3}$$

The individual would, in this case, wish to insure against an unsuccessful job search if laid off as well as against the possibility of being laid off after period 0 or period 1. Similarly if the optimal program is to accept half-time work in the low wage sector if released from the high wage sector, then:

$$EW(HW_0) = W_H + \beta\{(1-\lambda)W_H + \lambda 0.5W_L\} + \beta^2\{(1-\lambda)(1-\lambda)W_H + \lambda(1-\lambda)W_L + \kappa\theta\lambda W_H + (1-\kappa\theta)\lambda W_L\}; \tag{4}$$

INDIVIDUAL NOT EMPLOYED IN THE HIGH WAGE SECTOR IN PERIOD 0. For individuals not initially employed in the high wage sector, lifetime wealth and consumption will be less on average, and will in most circumstances be backloaded with greater expected wealth in later periods, both because intensive search (less work) for high wage jobs is more productive early in the life cycle and because the searching worker may find a high wage job in future periods, a possibilility whose expected value can be converted into consumption through the appropriate insurance instrument. The expected wealth of a period 0 secondary market worker who searches half-time in period 0 and period 1 (unless of course he finds a high wage job in the first period) is:

$$EW(LW_0) = 0.5W_L + \beta\{\kappa\Theta W_H + (1-\kappa\Theta)0.5W_L\} + \beta^2\{\kappa\Theta[(1-\lambda)W_H + \lambda W_L] + (1-\kappa\Theta)[\lambda W_L] + (\kappa\Theta)^2 W_H + (1-\kappa\Theta)\kappa\Theta W_L\}. \quad (5)$$

In the jargon of the public sector, this individual will demand both partial unemployment benefits and reemployment wage or earnings insurance.

Incomplete Financial Markets. Consider the converse of complete markets, the situation in which no financial market is operative. The individual consumes only what he earns in each period. Stable earnings can not be guaranteed unless the individual chooses full-time employment in the low wage sector. Some strong predictions arise from the model, most obviously that the individual will never choose full-time search, even though that might be optimal were financial markets complete. The individual must finance search activities in each period by working. In general search activities will be less attractive because the worker cannont insure against the possibility of not finding a high wage job if he searches. Because he is risk averse the utility of searching is reduced.

Saving is often possible when borrowing and insuring are not. The possibility of saving is most attractive to individuals employed in the high wage sector in the first period. Such workers will optimally save for later periods. These savings in turn may permit a laid off worker to engage in full-time search, though the prospects of that are reduced from the complete markets case because borrowing from the final period is not permitted and because the risk of full-time search is not insurable.

The public provision of earnings insurance is presumably intended to bridge the wide gap between the complete financial market ideal and the severely limited financial market reality. The current UI system is clearly a very incomplete response to the limited credit and insurance markets that workers typically face. Benefits are limited, even during the unemployment period--partial compensation is provided for full-time search. Moreover benefits for partial unemployment are severely restricted and compensation for low reemployment wages are nonexistent. In the next section I turn to three proposals for reform of the unemployment insurance system designed to expand its consumption insurance properties.

IV. EARNINGS INSURANCE: THREE PROPOSALS FOR REFORM

Given the apparent inability of current private and public institutions to provide workers with security against large declines in consumption, it is not surprising that a variety of reforms to the income security system have been proposed. The three wage insurance proposals I consider below have the objective of either i) improving the performance of the capital market, encouraging savings and/or borrowing, so that negative wage or earnings shocks can be smoothed across time, a form of self-insurance, or ii) providing wage insurance in the currently uncovered post-reemployment period. Besides contributing directly to the stability of consumption, these reforms may have important, positive effects on the unemployment duration distortions created by the unemployment compensation system.

A) TAX-INCENTIVES FOR INDIVIDUAL UNEMPLOYMENT ACCOUNTS (IUA's)

(Anonymous) An obvious extension of individual retirement accounts (IRA's) to the unemployment problem, individual unemployment accounts (IUA's) would encourage workers to set aside savings for periods of low or zero earnings by tax-sheltering savings and interest that are placed in such an account. Whether the savings and interest are sheltered, as in the original IRA program or simply the interest, as in the current IRA program, is a function of the size of the saving incentive the government wants to provide. Such an IUA plan would naturally roll over into an IRA if the worker reaches retirement age with a positive balance in his or her account.

B) EXPERIENCE RATING OF WORKERS IN THE UI SYSTEM

Saving is only one method of smoothing consumption flows across periods of low earning. It may also be possible to "self insure" against low earning periods by borrowing, permitting the family to smooth consumption over the future work life. One recent proposal by Robert Topel (1990) focuses on converting the current UI program into the equivalent of a human capital credit line.

> The incentive of unemployed individuals to inefficiently extend their spells would be eliminated if experience rating was extended to workers....Consider first a system that rates workers in the same way that employers are now rated... Each worker would have his own account with the UI system, and his tax rate would depend on the size of that account. A current insured spell of unemployment would raise subsequent taxes. Each account would be vested, so retiring workers would receive the current value of past net contributions. Since these systems represent implicit loans with a fixed pattern of repayment in terms of future tax liabilities, unemployed workers would properly internalize the cost of current benefits in their job search decisions. [Topel (1990, p.129)]

It is useful to note at the outset that the concept of experience rating of unemployment insurance taxes has very little relation to the idea of experience-rated premiums in the insurance literature. There "experience rating" means that premiums are adjusted so as to reflect expected future probabilities of a loss. In contrast, the existing UI system does not seek to estimate a firm's expected benefit outlay in the future, or to establish an actuarially fair rate (premium). Rather,...even for experience-rated employers the current system of UI financing merely advances interest-free loans to cover current benefit costs, enforcing repayment through higher future tax rates. [Topel (1990, p.109)]

C) REEMPLOYMENT WAGE INSURANCE

A reemployment wage or earnings insurance plan is proposed by Bailey, Burtless, and Litan (1993) for displaced workers. This proposal is designed to extend the current unemployment compensation system, focusing instead on insuring against earnings losses in the early reemployment period.

> Under [the BBL] proposal, workers would receive monthly or quarterly earnings supplements for a percentage of the earnings losses they suffer as a result of displacement. With a program that replaced 50 percent of lost earnings, for example, a displaced worker whose previous wage was $400 a week would receive a weekly insurance check of $100 if forced to accept a new job that paid only $200 a week. The percentage of earnings replacement could be tied to a worker's age and time on the job, with the supplement rising for older workers and those with longer tenure because they suffer proportionately larger and more permanent earnings losses than younger workers.
>
> A crucial feature of the plan is that earnings supplements would not be payable until a worker became reemployed and they would cease within a specified period after displacement occurs (say, after two years). Workers who found new jobs early in the two-year period would receive larger cumulative payments than those who delayed accepting a new job. [Bailey, Burtless, and Litan (1993, p.195)]

The BBL plan can be captured by the following benefit equation:

$$B_t = 0.5 * (E_{t0} - E_t) \text{ if } (E_{t0} - E_t) > 0 \text{ and } t\text{-}t0 < 2, \text{ zero otherwise,}$$

where B_t represents program benefits at time t, E_{t0} is pre-displacement earnings and E_t earnings at time t (post-displacement). The limited time dimension of the plan following layoff has elements of a reemployment bonus.

V. PROPOSAL CRITIQUES

In this section I consider some strengths and weaknesses of the three proposals outlined above.

A) TAX-INCENTIVES FOR INDIVIDUAL UNEMPLOYMENT ACCOUNTS (IUA's)

The private asset (savings) market is the most complete of the three markets that we will consider here (asset, loan, and insurance) and the rationale for public intervention therefore the least compelling. As with any economic activity, however, savings may be discouraged by income tax policies and propositions to encourage additional savings by exempting savings from income taxation therefore carries the same logic as exempting any sensible economic activity from taxation.

Before considering that issue, it is important to reiterate the obvious, saving is not insuring. Saving permits consumption smoothing across time, specifically moving consumption to a <u>later</u> point in time when consumption may be more seriously constrained. But saving is an inefficient method of insuring against the small probability of a large loss. If the event does not occur, the individual will have needlessly distorted his or her consumption pattern, inducing consumption instability where none need exist. As a consequence, unless the individual is extremely risk averse, the optimizing individual will not "self-insure" against large loss/low probability events, whether the savings are or are not subject to income taxation. However, more predictable periods of unemployment, say seasonal layoffs, are plausibly self-insuring events and may be influenced by tax policies on savings.

The empirical literature on IRAs and 401(k) plans provides a rich base for projecting the consequences of introducing an IUA plan. The evidence is not encouraging. Because contributions are voluntary, a large segment of the population is not likely to participate. As Andrews (1992, p.155) reports for 401(k) plans, "...59 percent of all eligible workers make 401(k) plan contributions." Put otherwise, 41 percent do not. Moreover, the families most likely to respond to tax-free savings accounts are not the families most vulnerable to layoff and unemployment. "In terms of wages, 79 percent of eligible employees earning $20 an hour or more contribute to their 401(k) plans, compared to only 35 percent of those earning $5 an hour or less." Andrews (1992, p.156). If the losses in tax revenue from the establishment of a program of this type were to be financed through the reduced generosity of the current UI system, there would almost surely be a redistribution of income toward wealthier households.

B) EXPERIENCE RATING OF WORKERS IN THE UI SYSTEM

In essence an unemployment insurance system with experience rating of individual workers offers the worker a line of credit secured by his or her human capital. The unemployed worker draws on his credit line during periods of unemployment and then repays through higher UI taxes when he or she returns to work. The plan provides the worker with the opportunity to spread earnings losses over the remainder of the work life. As with the IUA, which permits pre-event consumption smoothing, this plan is not an insurance plan. Earnings losses must still be absorbed by the individual and his or her family and lifetime consumption in total must be reduced, but the consumption reduction need not be concentrated in the low earning period itself. The possibility of truly large negative consumption shocks is therefore reduced.

Because this plan permits the worker to smooth consumption by bring income forward in time, it is especially useful early in life, when the remaining work life is long. If the remaining work life is sufficiently long and the earnings shock modest, the consumption smoothing that this system permits could be quite large and the potential distortions induced by the plan minimal. An important aspect of this plan, as Topel emphasizes, is that the job search activities of the unemployed worker would be "first best". The individual is simply bringing forward in time his or her own money and there is no distortion to search incentives.[9] In this circumstance, a variety of UI benefit eligibility rules designed to induce search activities could be dropped. Among the most important, perhaps, is the limitation on benefit payment when the unemployed worker is in schooling or other training programs. The UI rules are designed to limit payments to individuals who are not seriously searching for work, but the rules have obvious, perverse effects on the incentives to acquire skills during periods of unemployment. With the worker responsible for replenishing his or her own unemployment account, there would be no reason to limit payout in this way.

The proposal is not without difficulties, however. An individual subject to an unexpected unemployment shock will find that, as the period of unemployment (and unemployment benefits) lengthens, his or her future wage tax liabilities increase as well. This plan can be viewed as a rather perverse tax on the unlucky. Perhaps the most serious concern, however, is the "end point" problem. What happens if the individual retires with a negative balance? The logic of the program, for example the nondistorting effect on unemployment duration, relies on the notion that the individual is responsible for paying off his/her debt. This responsibility may be difficult to enforce as repayment becomes an increasingly large share of a retired worker's social security payment.

If negative balances at retirement are forgiven, the program loses much of its efficiency appeal. The unemployed worker need not treat the UI payments as his or her own. Activity monitoring would again be required. These problems become most extreme as the worker approaches retirement age. An unemployed worker only a few months from retirement with a large account deficit will presumably face an extremely large tax rate upon returning to work and may be induced to retire early if one can "escape" the wage tax by leaving the market entirely. Of course, one could limit borrowing in the program, stopping UI payouts when the worker's individual account is exhausted, but a crucial part of the consumption smoothing aspect of the plan would be lost. The program would be no more than a compulsory savings plan.

There are also ethical questions. In a sense the crucial element in the program is the absence of a bankruptcy provision. The plan would lose most of its value if the worker could simply wipe clean any negative balances in his or her "account." It is important that the individual be required to restore a zero balance. But bankruptcy laws are in place as a matter of social policy, they are not technologically required. What exactly are the purpose and function of the bankruptcy laws? Is there a distinction between wiping clean private debt and wiping clean public debt? Government student loan programs seem to be moving steadily if reluctantly toward such a conclusion. The logic behind this move remains poorly articulated.

C) REEMPLOYMENT WAGE INSURANCE

The reemployment wage or earnings insurance plan is targeted on an earnings problem ignored in traditional UI plans, namely the possibility of (relatively) low earnings on the new job. [UI coverage during the unemployment period itself is assumed.] As the evidence presented in the first section indicated, earnings on the job subsequent to a layoff are on average less than earnings prior to the layoff. The Bailey, Burtless, and Litan plan outlined above proposes that reemployed workers be paid benefits equal to a fraction, perhaps one half, of the difference between earnings before the spell of unemployment and earnings after the return to work for a specified time following layoff.

The direct insurance value of such a plan is both obvious and intuitively attractive. The quality of a job offer that a given worker uncovers is partly a matter of luck and a risk averse worker would surely cover him or herself against a major decline in earnings on subsequent jobs if markets were complete and costless. Moreover by encouraging the worker to accept jobs offering a lower wage, the plan provides an offset to the employment disincentives of the traditional unemployment compensation plan (assuming that duration is suboptimally long). The plan has the attractive feature that the subsidies target those most likely to be affected by UI benefits. Long tenured workers and older workers often face especially large wage declines upon reemployment, and, because UI benefits are determined by pre-layoff earnings, have little monetary incentive to return to work until UI benefits run out. By subsidizing those who have experienced the largest wage declines, the plan should hasten the return of these workers to the work force.

The reemployment bonus experiments suggest that such a subsidy would in fact accelerate the worker's return to the work force.[10] In an obvious way, wage insurance is a form of reemployment bonus, though one conditioned on the size of the wage rate penalty that the worker accepts when he or she returns to work rather than the speed of the return directly. Willingness to accept lower wages opens up a variety of employment opportunities. Unemployed workers can, for example, create a full-time work week from a series of part-time jobs, although they are likely to suffer wage and commuting cost penalties in doing so.

Although not discussed in the very brief proposal put forward by BBL, the source of lower earnings on the new job is potentially an important conditioning factor in the program. Earnings are the product of the wage rate and the number of hours worked ($E = w*H$). Either or both may be less on a new job. Indeed the most readily available jobs are likely to be part-time jobs at low wage rates. The BBL subsidy formula, if applied to earnings losses due to work hours, would lead to extraordinarily large effective wage rates on part-time jobs. The subsidy would offer a full-time worker before layoff the opportunity to accept a half-time job at three-quarters pay. Subsidies of this magnitude have the potential for inducing a large shift toward part-time work. Perhaps more important, the subsidy formula, if unrestricted by the number of hours worked, would be equivalent to the current UI system with a modified partial benefits formula. An individual who worked very little would receive one half his previous earnings, more or less what he would receive under the current UI program if he did not work at all. The most important corollary is that the extension of such a system from the current six months of the standard UI program to two years, as BBL propose, would lead to a massive increase in UI expenditures. It is possible, but not likely, that the modified wage strucure would induce a sufficently rapid return to work to offset this expansion. Unfortunately earnings-linked "disregards" with less than 100% implicit tax rates are currently operative in only a few states so we do not have a good empirical basis for predicting the effect of implementing such a program.

Two alternative wage insurance systems seem more attractive: 1) an hours-unrestricted wage rate system and 2) an hours-restricted wage system, say one that limits payments to full time workers, in which case the distinction between an earnings program and a wage rate program is lost. An hours-unrestricted wage rate program would subsidize workers according to the drop in wage rate they experience upon reemployment at whatever hours they choose to work. A deductible would surely apply here to limit small payments: A worker whose wage rate falls by 10% from $5.00 to $4.50 but who works only ten hours a week would only be eligible for subsidy payments of $10 per month, or one half his "loss," a payment level that would likely be dwarfed by the cost of administering the claim.

A natural way to limit the program is to restrict it to full-time workers, whether at a single job or multiple jobs. However even in this case serious administrative problems arise because of measurement problems in the wage. Compensation is multidimensional in at least two basic ways: 1) the compensation package often includes a variety of non-cash payments, such as health insurance and pension rights--and 2) there are nonpecuniary aspects of the job--some jobs are more pleasant than others. Presumably at some increase in administrative costs, the program could include a measure of the fringe benefits. However any fringe benefits that can be disguised from system administrators would be half paid by the program, which at least raises the possibility of serious compliance problems. The issue of job pleasantness seems more intractable. Under the usual compenating differential argument, earnings will be lower in more pleasant jobs ceteris paribus. Displaced workers would be encouraged to seek jobs that offer unmeasured amenities. The size of the distortion induced by this effect is unknowable at this time, but could be large.

A targeting problem exists as well. As noted above, in a dual labor market economy, not all workers are equally at risk from wage losses following job displacement. The main group covered by the wage insurance system would be those who have secured but then lost high wage jobs. Workers who have never been in the high wage sector have little to gain from a wage insurance program and should not be expected to contribute to it. In a sense the program provides the winners in the job search game the opportunity to secure their winnings. Chronic losers have little reason to contribute to the security of those better off than they. Taxes as well as benefits would be targeted in the ideal system.

BBL focus on the reemployment incentive aspect of the program and propose a fixed subsidy interval, perhaps two years, beginning not at the time of reemployment, but at the time of layoff. The present value of the subsidy systematically declines with the duration of unemployment and theoretically speeds the return to work directly, but reduces the earnings insurance value of the plan by doing so--the unemployed who have unusual difficulty securing a job, even a low wage job, will receive lower benefits than their more fortunate coworkers. I am unaware of any empirical evidence on the reemployment incentive effects of a time limited subsidy to low wage jobs.

IV. UNRESOLVED ISSUES IN PROGRAM DESIGN

Of the three reforms and extensions of the unemployment compensation system considered in this paper, the first--individual unemployment accounts or IUAs--would seem to hold little promise of significantly improving the stability of family consumption. At most an IUA will increase the rate of saving against "bad times" in the future. The group most sensitive to these tax incentives would be high income, white collar workers who have a low probability of being laid off and who would be better served by insurance.

Experience rating of workers has a variety of positive features, most obviously that the firm's layoff decision and the worker's unemployment duration would be optimally chosen. Monitoring of worker behavior, e.g. search requirements, would not be required. The major unresolved issue is the "endpoint" problem--how are those who leave the labor market with negative balances in their accounts to pay off their debt? How are the long term unemployed, especially those relatively close to retirement, to be encouraged to work when they face heavy payroll taxes because of large cumulative account deficits? Payouts could be limited by the requirement that the worker's account can never be negative, thereby avoiding the deficit problem entirely. This limitation would however eliminate one of the main features of the plan, the ability to smooth consumption by bringing earnings from the future. If the negative balance is forgiven, much of the efficiency claim disappears.

As for the wage insurance plan, a variety of design issues need to be resolved before the program can be implemented with any confidence. The magnitude of effect of the subsidy on acceptance of part-time jobs, perhaps multiple part-time jobs, needs to be estimated. Will the subsidy induce a restructuring of the work place toward part-time, low wage jobs? The social implications of encouraging a shift toward such work patterns also requires more careful thought.

The impact of the wage subsidy on the prevalence of high amenity, low-wage jobs also requires study. A long term subsidy to low paying jobs has the potential for seriously distorting compensating differentials over job amenities. An ideal solution would be the development of a reliable measure of work place amenities that could be incorporated into the wage subsidy plan, but the development of such a measure does not seem close. Current estimates of compensating differentials across job amenities are astonishingly variable. Some idea of the magnitude of the potential distortion caused by the subsidy is therefore needed.

The tax targeting issue is also crucially important. Wage insurance is of the most value to long tenured workers in the high wage sector. Workers in the low wage sector, even long tenured ones, are at little risk of being forced into significantly lower paying jobs. The question is how to identify and target the "high wage" workers most vulnerable to wage loss. Unfortunately the risk assignment question is not easily resolved. A straight-forward, progressive tax would not work, because "high wage" refers to work place wage premiums, not to the absolute level of earnings. A low skilled worker in the wage premium sector may have the most to lose, a high skilled worker in the low wage sector may be fully protected by the market.

One final issue of importance is the optimal duration of the wage subsidy. BBL propose two years from the date of layoff. The rationale for the two year interval is unclear, other than it is more than the six months of regular benefits and less than the remaining work lives of most workers. A limited duration of the subsidy may reduce the worker's incentive to settle into multiple part-time jobs or one with low pay and high amenity value, although as usual at a cost of reducing the insurance value of the program. Limiting the duration of the subsidy from the date of layoff rather than the date of reemployment is a type of reemployment bonus and may be optimal if reemployment bonuses are optimal and not otherwise provided.

Although these tasks seem daunting, insuring workers against major declines in wages is an important objective and deeper consideration of mechanisms to achieve that objective is clearly warranted.

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TABLE 1
The Growth in Real Wage Rate 1981-1982
Out of School Males 16 to 24 Years of Age
By Job Turnover and Race (in Percent)

		Growth in Real Wage Rate 1981-1982 (G)			
Job Status	Race	G=<-10% (1)	-10%<G=<+10% (2)	G>+10% (3)	Total (4)
Stayer	Total	18.9 (178)	46.7 (440)	34.5 (325)	100.1% (943)
	White	18.0 (129)	48.0 (344)	34.0 (244)	100.0% (717)
	Black	22.9 (39)	38.8 (66)	38.2 (65)	99.9% (170)
Quit	Total	29.4% (143)	21.0% (102)	49.6% (241)	100.0% (486)
	White	28.0 (103)	22.0 (81)	50.0 (184)	100.0% (368)
	Black	33.3 (34)	15.7 (16)	51.0 (52)	100.0% (102)
Layoff	Total	33.9 (82)	33.9 (82)	32.2 (78)	100.0% (242)
	White	31.2 (49)	35.7 (56)	33.1 (52)	100.0% (157)
	Black	42.5 (31)	26.0 (19)	31.5 (23)	100.0% (73)
Discharge	Total	35.8 (24)	38.8 (26)	25.4 (17)	100.0% (67)
	White	33.3 (15)	31.1 (14)	35.6 (16)	100.0% (45)
	Black	40.0 (8)	60.0 (12)	0.0 (0)	100.0% (20)

Source: National Longitudinal Survey of Youth. The total includes 86 males of races other than black and white.

FOOTNOTES

1. See Lester (1962) and Hamermesh (1977) for useful overviews of the UI system.

2. For a more detailed discussion of job turnover terminology, see Parsons (1977).

3. Parsons (1993) provides a more detailed account of the data and the empirical analysis.

4. The surprisingly high percentage of stayers who suffered serious real wage losses may in part reflect the high rate of inflation in this period.

5. For a comparable study in a less developed country, India, see Townshend (1994).

6. See Hotchkiss (1991) for one formalization of such a model.

7. Experience rating refers to the practice of adjusting the payroll tax rate of the firm to reflect system payouts to the firm's workers. In all states, there are upper bounds on the tax rate the firm faces. For firms at the upper bound, additional layoffs do not affect the firm's unemployment compensation payments.

8. I am grateful to Roger Gordon for pressing upon me the symmetry between the layoff remark and the duration remark. In an earlier draft, I had considered only the duration effect.

9. Differences in plan interest rates and those facing individual workers may affect individual behavior in unexpected ways.

10. For excellent reviews of these experiments, see Decker, O'Leary, and Woodbury (1993, Chapter 4) and Meyer (1994).

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