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UNEMPLOYMENT INSURANCE AND JOB SEARCH

James C. Cox and Ronald L. Oaxaca

I. INTRODUCTION

This paper examines the relationship between unemployment insurance (UI) benefits and the duration of unemployment. We review extant empirical evidence on the question of whether the receipt of UI benefits leads unemployed workers to prolong their spells of unemployment. Our objective is to examine representative studies of UI and worker job search responses with an eye toward eliciting implications for public policy and future research.

The current UI system was established by the Social Security Act of 1935 in response to the hardships imposed on the workforce by the Great Depression. Subject to minimum standards set by the federal government, the state governments administer the program and set their own requirements regarding eligibility and compensation. Funding for the program is derived from payroll taxes assessed against employers of covered workers. At the present time, well over 90% of wage and salary workers are covered by the UI system. Important eligibility requirements for covered workers who are unemployed include minimum earnings and employment over a base period preceding a spell of unemployment. In addition, states require a waiting period of one or two weeks before

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a claimant can begin receiving UI compensation. Within legislated minimum and maximum amounts, an unemployed worker's UI benefits are proportional to his or her previous earnings. The maximum duration of benefits is typically 26 weeks but can be higher in some cases. UI benefits have been subject to federal income taxes since 1978. Income maintenance and subsidization of job search are two objectives associated with the UI system. UI promotes income maintenance by providing a financial cushion for laid-off workers awaiting recall. UI assists workers on permanent layoff by financing job search; this is intended to promote better job matches than would be the case in the absence of the UI subsidy.

A disconcerting feature of empirical analysis of UI data is the wide range of estimated effects of policies and programs that abound in the literature. These disparate results stem from, among other things, differences in data sets, model specifications, time periods, and estimation techniques. One common finding in the studies reviewed below is a positive association between length of spells of unemployment and the receipt of UI benefits. However, this still leaves open the question of how to characterize the underlying behavior of unemployed workers. Two competing hypotheses have been suggested in the past: (1) UI benefits constitute a subsidy to leisure; and (2) UI benefits constitute a subsidy to job search. These competing hypotheses have implications for labor market efficiency and public policy.

Underlying the leisure subsidy argument is the basic static model of consumer choice of consumption and leisure (Moffitt and Nicholson, 1982). The income loss from a spell of unemployment overstates the value of the utility loss from reduced earnings. This is because of the value of the utility gain from the (forced) additional consumption of leisure. Hence, unemployment can have some value to workers because of the leisure consumption it makes possible. According to this view, UI benefits increase the attractiveness of unemployment by reducing the opportunity cost of leisure. Empirically, one would expect to find a positive association between UI and unemployment duration, and a clustering of unemployment terminations around the week of UI benefit exhaustion. In this framework, an increase in the potential maximum of UI benefit duration will have income and substitution effects for those who would otherwise have exhausted their entitlements. The consumption/leisure choice model predicts that, if leisure is a normal good, then the extension of potential duration of UI benefits will necessarily raise the duration of unemployment.

Job search theory holds that unemployed workers are searching for jobs in a manner that maximizes the present value of expected utility (expected income for a risk-neutral worker) from search (Mortensen, 1986). UI benefits serve to reduce the opportunity costs of the search process and can lead unemployed workers to search longer and hold out for better job offers. This model also predicts a positive association between UI benefits and the duration of unemployment as well as some clustering of unemployment terminations around the week of UI

benefit exhaustion. Also, an extension of maximum-benefit duration is predicted to lead to a longer spell of unemployment. Empirically, the job search model is distinguished from the leisure subsidy model by the prediction that UI benefits will produce better job matches including higher postunemployment wages. Our review will examine the available empirical evidence of the effects of UI on unemployment duration in light of these two alternative models. The papers included in our review were selected to be representative of the approaches taken to understanding the link between UI and job search. Our selection criteria also included the prominence of particular papers in the literature and whether the studies were funded by the UI service of the U.S. Department of Labor.

The remainder of our paper is divided into three parts. Section II is a review and summary of nonexperimental empirical studies of the effects of the current UI program. Section III reviews completed and ongoing laboratory and field experiments on job search behavior and possible changes in the UI program. Section IV is a discussion of the research and program policy implications of what has been learned to date about the incentive effects of UI at the level of the individual worker.

II. REPRESENTATIVE NONEXPERIMENTAL STUDIES

Ehrenberg and Oaxaca (1976) estimated the effects of UI benefit payments on postunemployment wages, duration of unemployment, and time spent out of the labor force. This study used 1966–1971 data from the National Longitudinal Survey (NLS) on four gender/age cohorts: males 14–24 and 45–59, and females 14–24 and 30–44. Ehrenberg and Oaxaca specified their UI variable as a wage replacement fraction, i.e., the ratio of weekly UI benefits to preunemployment weekly wages. Here we confine our summary to the Ehrenberg and Oaxaca results pertaining to workers who completed their spells of unemployment by changing employers.

Ehrenberg and Oaxaca estimated the effects of raising the wage replacement fraction by 10 percentage points, from 0.4 to 0.5. If one takes a base wage replacement fraction of 0.5, this represents a 20% increase in UI weekly benefits. Such an increase was estimated by Ehrenberg and Oaxaca to raise the duration of unemployment (in weeks) by 0.2, 1.5, 0.5, and 0.3 for males 14–24, males 45–59, females 14–24, and females 30–44. The wage gain effects of this increase in UI benefits were estimated to be 7% for males 45–59 and 1.5% for females 30–44, respectively. Among the 14–24-year-olds, the wage replacement ratio had no statistically significant effects on wage gains. Finally, this increase in weekly UI benefits was estimated to reduce time spent out of the labor force by 0.8 weeks for females 14–24 and 0.7 weeks for females 30–44. This effect was not estimated for the older male group and was not statistically significant for the young males.

As far as the job search interpretation is concerned, the Ehrenberg and Oaxaca results are somewhat mixed. The lack of UI-induced wage gains for young workers is consistent with any of the following explanations: (a) job search among young people is simply unproductive; (b) young people use their subsidized spells of unemployment to search for jobs with better on-the-job training opportunities at the cost of initially lower postunemployment wages; and (c) UI is serving as a subsidy to leisure for young people. The Ehrenberg and Oaxaca study does not shed much light on which (if any) of these alternative explanations is correct. However, Ehrenberg and Oaxaca observe that the apparent substitution of unemployment for time out of the labor force by the young women is consistent with the leisure subsidy argument. In the two cases for older workers in which UI benefits were associated with wage gains, the magnitudes of these effects are implausibly large. The wage gain effects imply that an additional week of search induced by UI is associated with a 5% wage gain. One factor underlying these anomalous results could be that the NLS data used by Ehrenberg and Oaxaca did not permit any control for the effect of UI on search intensity.

Burgess and Kingston (1976) estimated the impact of the weekly benefit amount (WBA) on the duration of compensated employment using data from the 1969–1970 Service to Claimants (STC) experiment. The sample consisted of UI claimants who had not exhausted their benefits and who were deemed "job ready" but not "job attached." Since the design of the STC experiment was intended to test the effects of special job search assistance to UI claimants and not to test the effects of UI itself on search outcomes, we discuss the Burgess and Kingston results here rather than in Section III. The Burgess and Kingston estimates imply that an additional $10 of WBA raises subsequent annual earnings by $250. A problem here is that the duration of compensated weeks of unemployment was included as a control variable by Burgess and Kingston in their wage gain equations. According to search theory, wage gain and duration of search are jointly determined. Consequently, Welch (1977) applied a rough correction to the Burgess and Kingston estimates and came up with estimates that implied that the total effect of an additional $10 inWBA raises subsequent annual earnings by $180 to $200.

Another study by Burgess and Kingston (1981) used the STC data in estimating the effects of WBA on compensated weeks of unemployment after controlling for a worker's maximum duration of benefits. This study found that an additional $10 of WBA raises the duration of compensated unemployment by 0.15 weeks. Combining this effect with the Burgess and Kingston earlier wage gain estimates implies that an additional week of compensated unemployment raises annual earnings between $1200 and $1300. Burgess and Kingston estimated that an additional week of potential benefit duration raises compensated unemployment by 0.61 weeks. Some differences by demographic characteristics were evident from the Burgess and Kingston 1981 study. WBA effects on com-

pensated unemployment tended to decrease with age while the potential duration effects on compensated unemployment tended to increase with age. Both the WBA and potential duration effects tended to be larger among nonwhites.

Classen (1977) sought to determine the effects of UI benefits on unemployment duration and earnings by taking advantage of the fact that legislated increases in maximum UI benefits took place in 1968. She used the Continuous Wage and Benefit History (CWBH) data set for 1960–1970. The duration variable was measured as weeks of UI benefits collected per successful claim, the UI variable was the WBA, and the earnings variable was high-quarter earnings in the year following a completed spell of unemployment. Using data for Arizona and Pennsylvania, Classen estimated that a $10 (1968 dollars) increase in WBA would cause an increase in benefit duration of 1.1 weeks in both states but have no significant effect on earnings in either state. Although the Classen and Ehrenberg and Oaxaca studies differ in many respects, they yield similar estimated overall effects of UI on duration. In addition to the lack of an earnings effect from UI, Classen's study cast further doubt on the productivity-enhancing effects of UI by failing to find any UI effect on the number of employers a worker had in the two-year period following a spell of unemployment. It was expected that if UI promoted better job matches, workers would have fewer employers following spells of insured unemployment.

Holen (1977) estimated the effects of UI benefits and potential weeks of eligibility on unemployment duration and subsequent wages using data from the STC experiment. Her results imply that a $10 (1969–1970 dollars) rise in WBA would raise compensated unemployment by about one week and quarterly earnings by $90. In other words, each additional week of UI-induced search raises quarterly earnings by $90. Holen conjectured that the wage gain effect reflected some combination of an extra week of search and increased search intensity. Her estimates also imply that extending the maximum potential eligibility period by one week would raise *compensated* weeks of unemployment duration by 0.8 weeks and quarterly earnings by $2.50. Holen investigated whether the extension of potential entitlement had any effect on search behavior. Her results suggested that the probability of short spells of unemployment was reduced and the distribution of total search duration was shifted toward longer spells of unemployment.

Hamermesh (1979) examined the entitlement effects of UI availability on labor supply and labor force participation among married women. The idea is that potential UI benefits make market work more attractive. Hamermesh drew a sample from the Panel Study of Income Dynamics for the period 1967–1971. His sample consisted of married white women between the ages of 30 and 54, who were married to the same husband from 1967 to 1972 and who resided in the same state from 1970 to 1972. For the entire sample, Hamermesh simulated the effects of a 20% increase in UI benefits. He estimated the resulting disincentive effects on total hours of work and the positive entitlement effects on labor supply

and labor force participation. While the net effects are slightly negative, it is hard to escape the conclusion that they are *not* significantly different from zero.

Solon (1978) questioned the desirability of measuring the work disincentive effects of the UI system by looking only at the unemployment effects. If the UI system encourages labor force participation among those who would otherwise be out of the labor force, the estimated UI effects on unemployment would overstate the true disincentive effects as measured by *employment* effects. Solon used data for a sample of former UI claimants in the state of New York who had established benefit years over the period September 1972 to August 1973, and who had also exhausted their 26 weeks of *regular* UI benefit entitlement. He found a marginally statistically significant negative effect of extended UI benefit eligibility on subsequent weeks of employment. His results indicated that for every ten weeks of *extended* benefit eligibility, employment was reduced by one week. This effect was mainly confined to those who had received UI benefits in two of the previous five years (repeaters). Solon's estimates of work disincentives were smaller than what one would have inferred on the basis of unemployment effects because of the out-of-the labor force effects.

Fishe and Maddala (1980) estimated the effects of UI benefits on the duration of unemployment from a structural model of joint wage offer and reservation wage determination. They explicitly incorporated the assumption of finite search horizons among unemployed workers in contrast to the infinite-horizon specification implicitly or explicitly used by most researchers. Fishe and Maddala used a CWBH data set for a sample of workers in Florida who had been unemployed at some point between 1971 and 1975. The UI benefits variable in the Fishe and Maddala study is defined as the potential weekly benefit amount (PWBA) an unemployed worker is eligible for (equal to WBA for actual periods of compensation). Fishe and Maddala's results imply that a 20% rise in PWBA raises the weekly reservation wage by 2.8% of the average weekly wage and increases the duration of unemployment by 1.4 weeks. Furthermore, the exhaustion of UI benefits was estimated to drop the weekly reservation wage by 16.4% of the average weekly wage. Fishe and Maddala found that the number of weeks remaining until UI exhaustion was positively related to the reservation wage and was statistically significant. As each week passes without a job acceptance, the weekly reservation wage declines by 1.4%. This result was interpreted as evidence of a declining reservation wage over the search horizon. Fishe and Maddala treated the finite search horizon as a parameter that they estimated to be 40.9 weeks.

Moffitt and Nicholson (1982) examined the impact of changing the maximum duration of UI benefit eligibility on the duration of unemployment. The labor/leisure choice (or leisure subsidy) model provided the theoretical underpinning for the employment (or labor supply) function in their study. A maximum-likelihood estimation procedure was used to incorporate the kink in the unemployed worker's budget line at the point of maximum UI benefit duration eligibil-

ity. Data for the study were drawn from a sample of workers who collected UI benefits under the Federal Supplemental Benefits (FSB) program. A negative estimated coefficient on the non-UI, nonwage income variable indicates that leisure is indeed a normal good. Moffitt and Nicholson estimated that increasing potential UI benefit duration by one week raises weeks unemployed by 0.1 weeks. This result implies that the 26-week extension of maximum duration of UI benefits triggered by the FSB program in response to the 1974–1975 recession increased unemployment duration by about 2.5 weeks. Moffitt and Nicholson also found that a 10% increase in the net wage replacement ratio would raise the duration of unemployment by 0.4 weeks for those with high probabilities of benefit exhaustion and by about 0.8 to 1 week for the typical claimant.

Feldstein and Poterba (1984) examined two major issues. The first is the existence of evidence that indicates that a significant proportion of unemployed workers hold unrealistically high and socially nonoptimal reservation wages. While one might question whether some of these individuals should be considered unemployed, they are nevertheless counted as unemployed according to the official definition of unemployment. The second issue is whether UI benefits exacerbate the problem of nonoptimal reservation wages by raising them further and hence prolonging unemployment.

Feldstein and Poterba used data obtained from a special supplement to the May 1976 *Current Population Survey* (CPS). Unemployed workers were asked to indicate the kind of work they were seeking and the lowest wage they would accept to do the specified work. From this group, Feldstein and Poterba selected a subsample of those unemployed who were receiving UI benefits. A reservation wage variable was constructed as the ratio of the reported reservation wage to the last wage received prior to the current spell of unemployment. Feldstein and Poterba constructed their UI variable as the ratio of WBA received to the previous wage adjusted for a constant marginal tax rate of 0.3.

Regression analysis by Feldstein and Poterba revealed that the UI wage replacement variable had a statistically significant positive effect on the reservation wage ratio for each of the groups of UI recipients in the sample ("job losers on layoff," "other job losers," and "job leavers"). Predictably, the UI effect was largest for "other job losers." For a worker in this latter category whose gross UI wage replacement ratio was 0.5 (0.7 after taxes), a 20% rise in WBA would increase the reservation wage by 6% of the previous wage. Feldstein and Poterba also found that the UI net wage replacement ratio had a positive statistically significant effect on the probability that an unemployed worker would have a reservation wage ratio in excess of 1.0. For the same worker as described above, a 20% increase in WBA would raise the probability that the reservation wage ratio exceeds 1.0 by 5 percentage points (31% of the "other job losers" sample had reservation wage ratios in excess of 1.0).

Moffitt (1985) attempted to deal with the problem of wide-ranging estimates of UI effects. Specifically, Moffitt focused on the effects of the maximum potential

duration of benefits. His study adopted a uniform specification and estimation strategy across four selected data sets. The objective was to generate a narrower range of UI effects as well as to ascertain how much of the differences in estimates can be attributed to differences in data bases. The four data sets used were (1) CWBH (1978–1983), (2) the Employment Opportunity Pilot Project (EOPP) (1979–1981), (3) the Federal Supplemental Benefits (FSB) Follow-Up Survey (1975–1977), and (4) the Newton–Rosen data set (1974–1976). Moffitt's study yielded estimated effects of an additional week of potential duration of UI benefits that ranged from 0.17 to 0.45 additional weeks of unemployment for males and 0.10 to 0.37 additional weeks of unemployment for females. There was some hint that these effects were somewhat larger when the unemployment rate was higher. Moffitt also found that the effect of an additional week of potential duration on combined weeks of unemployment and out of the labor force was 0.52 weeks for males and 0.66 weeks for females. He found no strong evidence that increased potential duration had any effect on the labor supply of other members of a UI recipient's family. Finally, Moffitt failed to find any convincing evidence of an effect of potential duration on postunemployment earnings.

Katz and Meyer (1988) used hazard rate analysis to estimate the impact of potential duration and UI benefit level on the duration of compensated unemployment and on the timing of exits from unemployment. They used two data sources for their study: (1) a sample of heads of households from the Panel Study of Income Dynamics (PSID) over the period 1980–1981, and (2) a sample of males from the CWBH data for 1978–1983 supplemented by an additional CWBH data set covering the period 1979–1984. They found sharp spikes among UI recipients in the escape rates from unemployment at about the duration when UI benefits were exhausted (26 and 39 weeks). No such sharp pattern in escape rates was found for non UI recipients. Although the statistical significances of the potential duration and UI benefit level in the hazard models were marginal, the estimated effects were in the anticipated directions. Katz and Meyer's results imply that a one-week increase in potential duration would raise compensated duration by 0.16 to 0.20 weeks and that a 20% rise in UI for an individual with a replacement ratio of 0.5 would increase the length of a compensated spell of unemployment by 1.5 weeks.

III. FIELD AND LABORATORY EXPERIMENTS

Applied econometric studies of the effects of UI, such as those reviewed in Section II, establish statistical associations between UI benefits and unemployment duration, wage gains, and other variables of interest to policymakers. However, interpretations of these results in terms of search intensity, labor/leisure substitutions, etc., require the use of theoretical models as maintained

hypotheses. Many econometric studies of UI employ either a finite- or an infinite-horizon search model as a maintained hypothesis (Devine and Kiefer, 1988). Therefore, if the job search/job acceptance behavior of economic agents is *not* consistent with the search models, then the interpretations of the results of the applied research can be misleading. Hence, it is essential that search models be subjected to direct empirical testing to learn whether they can be falsified.

The literature contains some very ingenious studies that use econometric techniques designed to test search models with data from the historical record. Among these are Kiefer and Neumann (1979a,b), Warner et al. (1980), and Lancaster and Chesher (1983). However, the properties of search models pose inherent limitations on what can be learned with this approach. Consider some of the difficulties in attempting to use nonexperimental data to test job search theory. The models imply that the feasibility of recalling past wage offers, the length of the search horizon, and/or agent information about the distribution of wage offers are central determinants of an optimal search strategy. But possibilities of wage offer recall, the length of search horizons, and agent information on wage offer distributions are not observable in nonexperimental data sources. Hence such data are not very useful for learning whether job search models can be falsified by observations of job search behavior. In contrast, controlled experiments have some unique advantages for empirical evaluation of search models. The relative advantages of laboratory experiments and field experiments are somewhat different, and thus we will discuss both types.

Laboratory experiments designed to test finite-horizon search models were conducted by Cox and Oaxaca (1989a,b). In these experiments the researchers can control, and thereby observe, the possibility of recalling past wage offers, the length of the search horizon, and agent information about the wage offers distribution. Also under experimental control are theoretically hypothesized determinants of search behavior such as the discounting rate of interest and the cost or subsidy to search. Thus, such laboratory experiments are well-suited for learning whether people are capable of making choices in a dynamic, uncertain decision environment *as if* they were finding the optimal solutions to stochastic dynamic decision problems. This is the type of behavior that is modeled in job search theory and that is used as a maintained hypothesis in much econometric research on UI.

The Cox and Oaxaca (1989a) laboratory experiments were conducted with 60 subjects who participated in base line and various treatment trials. The experimental treatments consisted of variation of the rate of interest, the subsidy to search, the riskiness of the wage offers distribution, the probability of obtaining a job offer, and the length of the search horizon. The picture that emerges from these experiments is one of reasonably close agreement between the predictions of the risk-neutral search model and observed subject behavior. Overall, subjects terminated search exactly at the point predicted by the risk-neutral model in 77% of 600 trials. However, there was significant evidence of risk-averse behavior.

The risk-averse or risk-neutral (concave) model survived the experimental tests remarkably well. Fully 94% of the search terminations in 600 trials were consistent with this concave search model. The accuracy of the concave search model in predicting search behavior is supported by several parametric and nonparametric tests reported in the paper.

The theory of optimal job search focuses on reservation wages. But the typical message space of both naturally occurring labor markets and job search experiments includes admissible statements of job offer acceptance and rejection, not statements of (binding) reservation wages. Cox and Oaxaca (1989b) present the results of experiments designed for direct tests of the reservation wage property of a finite-horizon sequential search model. Since precommitment may frame the acceptance/rejection decision in an unfamiliar way, precommitment is introduced as an experimental treatment, with base line control, in order to test for any framing effects on search decisions. Overall, the results of the Cox and Oaxaca (1989b) precommitment experiments confirm the findings from the no-precommitment search experiments reported in Cox and Oaxaca (1989a). That is, the linear (risk-neutral) model and especially the concave (risk-neutral or risk-averse) search model are good predictors of both reservation wages and search terminations. Precommitment effects are initially present but they attenuate in subsequent experimental trials. The precommitment effect takes the form of earlier (than in base line) search terminations. The data base generated from the paired no-precommitment/precommitment experiments is also used to evaluate various econometric procedures for estimating reservation wages from job acceptance data.

There are six field experiments with the UI program that have either recently been completed or are currently at some stage of planning or implementation. The first of these was the completed Illinois reemployment bonus experiment. Further experiments with alternative bonus formulas are currently in progress in Washington. The completed New Jersey experiments involved several treatments that included combinations of reemployment bonuses, job search assistance, job training, and relocation assistance. The New Jersey experiments were targeted on structurally unemployed workers. Experiments currently in progress in Pennsylvania involve treatments that use alternative reemployment bonus formulas and job search assistance. Two other experiments that are currently in progress are the Washington and the Three State Self-Employment Demonstration Projects. These experiments involve treatments that consist of self-employment allowances and various support services to assist UI recipients who want to become self-employed. The Illinois and New Jersey experiments are the only ones for which results are currently available; hence we will focus our discussion on these two.

Results from the Illinois experiment are presented in Spiegelman and Woodbury (1987) and Woodbury and Spiegelman (1987). In this experiment, individuals in a selected subset of UI claimants were randomly assigned to one of

two treatment groups or a control group. An individual who was assigned to the claimant treatment group was eligible for a $500 reemployment bonus if he or she returned to work with either his or her old employer or a new employer within 11 weeks of filing the UI claim and remained on that job for at least four months. If an employer hired an individual who was assigned to the employer treatment group then the employer was eligible for a $500 bonus if the worker met the 11-week and four-month filing and employment conditions. Bonus-qualifying jobs for both treatment groups had to provide at least 30 hours per week of employment.

The results of the Illinois experiment support the conclusion that reemployment bonuses can significantly affect the job-finding behavior of UI recipients. Individuals in the claimant treatment group had an average of 1.37 fewer weeks of unemployment than those in the control group during the first spell and 1.15 fewer weeks during the benefit year. The average differences between the employer treatment and control groups were 0.67 weeks in the first spell and 0.36 weeks during the benefit year. All of these figures except the 0.36 benefit year figure are significantly different from zero at the 5% significance level. Furthermore, the three significant reductions in weeks of unemployment also involve significant reductions in total UI benefits paid out, inclusive of the $500 bonuses. In addition, the lower average number of weeks of unemployment for the treatment groups does not appear to have been achieved at the cost of lower post-unemployment earnings.

Further analysis of data from the Illinois experiment is presented in Meyer (1988). He also discusses some ways in which the results of this field experiment might not be indicative of the effects of a permanent national reemployment bonus program. The Illinois experiment lasted only 17 weeks and was not publicized; hence it is unlikely that it induced firms to change their layoff and recall policies. However, if recalled workers were eligible for bonuses in a permanent program, this would provide a substantial subsidy to temporary layoffs. The responses by firms and workers to the incentive provided by this subsidy might lead to a substantial increase in UI claims. In contrast, if recalled workers were not eligible for bonuses (as in the New Jersey and Washington experiments) this would provide an incentive to break up employer/employee matches. Any response to this incentive by workers would increase UI claims and impose other costs on the economy. However, there could be an offsetting effect on firms in that they might reduce layoff frequency in order to avoid having their employees respond to the bonus incentive to join other firms.

Results from the New Jersey experiment are reported in Corson et al. (1988). In this experiment, individuals in a selected subset of UI claimants were randomly assigned to one of three treatment groups or a control group. The three experimental treatments were (1) job search assistance, (2) job search assistance combined with training or relocation assistance, and (3) job search assistance combined with a reemployment bonus. The eligibility screens that were used in

selecting individuals for inclusion in the experiment were intended to select displaced workers (the target group). This was partially successful although other UI recipients were included. Not surprisingly, the experimental treatments were more effective for individuals not in the target group.

The reemployment bonus formula in the New Jersey experiment was different than the one used in the Illinois experiment that is described above. The New Jersey bonus formula offered individuals one-half of their remaining UI entitlement if they started work by the end of the second full week following the assessment/counseling interview. This implies that bonus eligibility began about the seventh week after an eligible claimant filed for UI. The amount that could be claimed during the first week of bonus eligibility averaged $1644. In subsequent weeks the bonus declined by about 10% of the original amount each week.

Data from the New Jersey experiment indicates that each of three experimental treatments significantly reduced both the number of weeks that claimants collected UI and the amount that they collected. Furthermore, the treatment with the reemployment bonus had the largest effect in reducing both weeks of UI duration and dollars of UI paid. The results also indicate that all three treatments increase both employment and earnings in the year following the UI claim. Thus the experimental treatments do not appear to have lowered reservation wages. Finally, various benefit–cost analyses are reported. They indicate that none of the treatments led to positive net benefits for the Labor Department. However, two of the treatments yielded positive net benefits for the government sector and, most importantly, all three of the treatments yielded positive net benefits to society as a whole and to claimants.

Although the results of the Illinois and New Jersey experiments appear to be favorable, Meyer (1988) explains why these results do not support policy conclusions. Both of these experiments were short-lived and were not widely advertised to workers and firms. Hence, it is reasonable to suppose that neither workers nor firms made strategic responses to *the existence* of the experimental treatments. However, the possibility of such strategic responses to a permanent program implies that the results of the experiments might not be a good predictor of the impacts of a permanent program.

Consider the possibility of strategic responses by workers to a permanent reemployment bonus program. The bonus formulas from the New Jersey and Illinois experiments provide good examples to illustrate the problem. The New Jersey bonus became available in the seventh week after a UI claim was filed. The average initial bonus of $1644 was about five-weeks' average wages. Thus, anyone who was planning to start a job after two weeks of UI could increase his or her income by waiting a few more weeks to become eligible for the bonus. In contrast, the Illinois bonus was available immediately after a UI claim was filed. Immediate availability of a bonus would provide an incentive for some people to file UI claims who otherwise might not file. The most obvious example of this would be someone who had a new job lined up upon termination of the previous

job. With either type of strategic response, the larger number of benefit payments caused by added claimants might eliminate any cost savings of a bonus program. Therefore, the findings of positive net benefits for reemployment bonuses in the Illinois and New Jersey experiments do not support the prediction that a permanent bonus program of either type would yield positive net benefits.

Another question that was not addressed by the Illinois and New Jersey experiments is the possibility of displacement effects. That is, individuals in the experimental treatment groups who found jobs more quickly may have done so at the expense of others who took longer to find jobs. Any such displacement effects would detract from the calculated net benefits of the experimental treatments. The Pennsylvania experiment will attempt to examine this question by comparing the experiences of the control groups to similar groups in other labor markets. It remains to be seen whether this can be done effectively.

IV. IMPLICATIONS FOR RESEARCH AND POLICY

For all of the many differences in techniques, model specifications, time periods, and data sets that characterize empirical studies of UI effects on individual workers, it is remarkable that these studies all point to the same qualitative effect of UI benefits on work incentives: UI prolongs spells of unemployment and lowers employment among its recipients. Unfortunately, the magnitudes of these estimated effects vary more than one would like. Consider a 20% increase in WBA for a UI claimant with a wage replacement rate of 0.5. Based on the studies we reviewed, the estimated duration effect is substantially less than a week for females and young workers, more than a week for older males, exactly one week for all workers according to two studies, and 1.4 to 1.5 weeks according to two other studies. In the light of these findings a single best estimate at this point is that the duration effect is about a week. Another source of work disincentives is the maximum potential duration of UI benefits. Consider a one-week increase in the maximum potential duration of UI benefits. Four of the studies reviewed by us estimate that this increase would extend unemployment (or in one of the studies, reduce employment) by 0.1 weeks to 0.45 weeks. Two studies that found larger effects of 0.61 and 0.8 weeks used compensated weeks of unemployment, which may have overstated the effects on total weeks of unemployment. Given the preponderance of evidence for modest effects, a best guess at this point is that an additional week of potential duration increases a spell of unemployment by no more than 0.5 weeks. If one regards weeks of employment as a better measure of work disincentives, then the evidence thus far shows that at least extended benefits beyond the exhaustion of regular UI benefits has very small negative effects on employment.

The empirical evidence on the wage gain aspects of UI is not even in agreement on the existence of such an effect, let alone on its magnitude. This goes to

the heart of the issue of whether the job search or the leisure subsidy hypothesis is the most appropriate for understanding the effects of UI on spells of unemployment. While some studies show a wage gain, others do not. At the present time one can find no compelling evidence in support of the proposition that UI increases wages because of better job matches and increased job stability. This does not necessarily mean that there are no such effects, but only that significant data problems prevent the research community from being able to test for their presence properly.

It should be appreciated that different estimated UI effects that merely reflect demographic differences are not necessarily a statistical problem. Different worker responses to UI can be anticipated when workers differ in their personal characteristics. This does, however, present a policy problem because of the political and legal difficulties in adopting UI legislation that treats potential UI recipients differently depending on their non–job-related personal characteristics.

What are the implications of previous research for the future UI research agenda? These implications fall into two categories: (a) appropriate data bases, and (b) research topics. Virtually all of the empirical evidence on UI to date is derived from nonexperimental sources. These include administrative records and household surveys. Few of the authors of these studies have been too inhibited to point out severe limitations of the data bases used by others (and sometimes even their own!). It seems clear that there is a consensus that using data based on only compensated spells of unemployment introduces unacceptable estimation biases when analyzing *total* duration of unemployment spells. But even with household survey data that yield information on completed spells of unemployment, there are too many important factors that go unobserved. These include the length of a worker's search horizon, the worker's discounting rate of interest, and a worker's search costs.

An essential concept of the search paradigm that provides the theoretical basis for most of the UI studies on individual worker behavior is the reservation wage. Yet this theoretical construct is not observed in the data. What about the question in the May 1976 CPS that asked unemployed workers to state the lowest wage they would accept for the kind of job they were seeking? We maintain that there is no basis for interpreting the answers to this question as corresponding to the theoretical notion of a reservation wage. In their actual job acceptance decisions, workers are in no sense bound by their answers to the reservation wage question. One might more plausibly argue that the answer to this question reflects a hoped-for or desired wage. Consistent with this view is the evidence found in Feldstein and Poterba (1984) that shows little or no decay in the ratio of stated reservation wages to the previous wage with the number of weeks the individual had been unemployed. Accordingly, we are not prepared to draw any policy conclusions from the magnitude of the estimated UI effect on the reported reservation wage in

the Feldstein and Poterba study. This is an important issue because in the search paradigm it is the effect of UI on the reservation wage that produces the association between UI and the duration of unemployment. The absence of data on this variable could potentially be managed if one could observe all offers received by an unemployed worker instead of only the accepted offer. Unfortunately, traditional data sources do not provide information on all offers received.

One of the significant advantages of controlled experiments is that they can make some variables observable that would otherwise be unobservable. This is especially true of laboratory experiments in which such factors as the feasibility of recalling past wage offers, the length of the search horizon, and agent information about the distribution of wage offers are controllable and therefore observable. These theoretical determinants of job search behavior are inherently unobservable in nonexperimental data sources and difficult or impossible to observe in field experiment data sources. Although fewer variables are observable in field experiments than in laboratory experiments, the former have the obvious advantage of being conducted in an environment that is closer to the naturally occurring economy in which UI programs actually operate.

The UI research agenda of the future should include the topics described below.

The effects of UI on postunemployment earnings and on the quality of postunemployment job matches is still very much an open question. The answer to this question bears on how much we regard UI in its traditional role as income maintenance for workers on temporary layoff as opposed to its potential role for improving the efficiency of job search.

More research is needed to determine what effects UI has on search intensity. Changes in the UI system to provide monetary incentives to shorten the duration of UI claims may possibly work through some combination of lowering the reservation wage and increasing job search intensity. The fear some may have about the former is that poorer job matches may be encouraged.

If UI is found to raise postunemployment earnings through better job matches, then a cost–benefit analysis should be performed to determine whether the social gains offset the social costs of longer job searches.

The incentive effects of UI over the business cycle need to be examined more systematically. It seems reasonable to suppose that an additional dollar of UI benefits or an additional week of maximum potential duration will have different effects on search outcomes depending upon where the economy is in the business cycle.

Moffitt (1985) makes a convincing case for the value of improved modeling of the dynamics of the search process of unemployed workers as time-varying variables such as potential duration and the unemployment rate change during the search process. Changes in these factors can influence the efficacy of UI policy. This topic is related to the immediately preceding one.

More research is needed on the formal modeling and empirical testing of simultaneous search by firms for workers and search by workers for jobs. How does the UI system simultaneously affect both sides of the labor market?

Without awaiting the definitive work on the effects of UI on work incentives, there are some policy implications that can be raised at this point. If the UI system were found to produce better job matches, then why should there be a public subsidy to productive job search? A divergence between private and social gains could be one justification. The existence of imperfect capital markets might be a justification for a public subsidy. As argued in Classen (1977), such a justification need not require outright UI grants. Rather, one could make an argument that a subsidized unemployment loan program could address the problem of imperfect capital markets.

Policymakers are continually faced with the task of limiting the benefit costs of the UI programs. To the extent that the costs of the UI program are ultimately borne by employers and by workers in the covered sector, both parties have an interest in cost-effective UI plans. Aggregate benefit costs could be lowered by legislating reduced weekly benefit amounts and/or maximum weeks of UI entitlement. However, such measures would degrade the adequacy of UI benefits as an effective form of income maintenance during temporary spells of unemployment. Another cost-saving measure would be to tighten up on eligibility requirements in the nonmonetary determinations. This already occurs to some extent because claims deputies can give closer scrutiny to UI claims without the need for formal legislative changes.

The desire to reduce both the transfer payments of UI and its administrative costs can be accommodated by making the program function more like insurance rather than as income maintenance. The reemployment bonus payment plan is a step in this direction. There are of course the usual moral hazard problems to be concerned with. Furthermore, the results from completed UI bonus field experiments have not provided adequate evidence to support the conclusion that a permanent reemployment bonus program would be cost-effective.

Given the likelihood that the effects of the UI system are not invariant over the business cycle, it is unfortunate that the current UI system offers little flexibility in dealing with this issue. About the only automatic response is the Extended Benefits (EB) program, which is triggered by a state's insured unemployment rate reaching 6% for states with such provisions or a 5% rate that is at least 20% higher than the average for the corresponding period in the previous two years. One could imagine different UI payment schedules being triggered by a high unemployment rate. For example, consider a UI bonus plan in which a job acceptance bonus declines the longer a UI claimant goes without accepting a job. One possibility is that the UI bonus payment decreases at an increasing rate. This plan would provide the strongest incentives to accept an offer early in the spell of unemployment. This seems to be appropriate for periods of relatively low unemployment when the probabilities of receiving job offers are high. In periods of

high unemployment the incentive for early job acceptance would be frustrated by the low probabilities of receiving job offers. During these periods it might be more effective to implement a UI bonus payment schedule that provided more incentive farther along in a worker's spell of unemployment. Such a plan would offer a UI bonus schedule that declined at a decreasing rate during a spell of insured unemployment.

We believe that such plans as we have discussed above are feasible and could meet the objective of adequate income maintenance assistance for the involuntarily unemployed without unacceptable work disincentives. They would be good candidates for inclusion in future field experiments.

Other future field experiments should be designed so as to not be subject to the major shortcoming of the present experiments that we discussed in detail in Section III. This shortcoming is that the results of the completed and in-progress experiments with reemployment bonuses, etc., may not be good predictors of the effects of a permanent policy because of differences in the feasibility of strategic responses by workers and firms. There is no low-cost solution to this experimental design problem. In order to incorporate in an experiment the same possibility for strategic responses that would exist with a permanent program the experiment must continue for a longer time period than has previously been tried with UI experiments. Of course, the incentive features of the experimental treatments would also need to be widely advertised.

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