Bonuses to Workers and Employers to Reduce Unemployment: Randomized Trials in Illinois

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New claimants for Unemployment Insurance were randomly assigned to one of two experiments that were designed to speed up the return to work. In the first experiment, a $500 bonus was offered to eligible claimants who obtained employment within 11 weeks. This experiment reduced the number of weeks of insured unemployment, averaged over all assigned claimants whether or not they participated, by more than one week. In the second experiment, the $500 bonus was offered to the subsequent employer of the eligible claimant. This experiment reduced the weeks of insured unemployment for only one important group — white women — by about one week.

Between mid-1984 and mid-1985, the Illinois Department of Employment Security conducted two controlled social experiments designed to test the effectiveness of cash bonuses in reducing the duration of insured unemployment. In one experiment, called the Claimant Bonus Experiment (or simply *Claimant Experiment*), a random sample of new claimants for Unemployment Insurance (UI) were instructed that they would qualify for a cash bonus of $500 if they found a job (of 30 hours or more per week) within 11 weeks of filing the UI claim, and if they held that job for four months. The intent was to create an incentive for claimants to search more intensely for work and to become reemployed more rapidly than they would otherwise.

In the other experiment, called the Employer Bonus Experiment (or *Employer Experiment*), a second random sample of new UI claimants were told that their *next employer* would qualify for a cash bonus of $500 if they, the claimants, found a job within eleven weeks of filing the UI claim, and if they retained that job for four months. The intent here was to provide a marginal wage-bill subsidy, or training subsidy, that might reduce the duration of insured unemployment.

The impetus for the Illinois Unemployment Insurance experiments was a decade of criticism of the UI system and the inability of economists to establish clearly how the behavior of UI recipients differs from what it would be in absence of the UI system, if it differs at all.

The most benign view of the UI system would be that it provides unemployment benefits to carry a worker through a spell of unemployment resulting from involuntary layoff. In such a benign view, UI benefits are taken to be a nondistortionary transfer the

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size of which cannot be affected by an individual's behavior. But there are at least two reasons why the presence of unemployment benefits may prolong a jobless spell beyond what it would be in the absence of unemployment benefits. First, as job search models such as those pioneered by Dale Mortensen (1970) and J. J. McCall (1970) suggest, UI benfits may act as a subsidy to additional job search. Indeed, nearly all empirical work to date on the relation between UI and the duration of unemployment has been interpreted in the context of one or another job search model. Second, as Orley Ashenfelter (1978a) has observed, UI benefits are also a subsidy to the consumption of nonmarket time (or leisure). If labor is not supplied perfectly inelastically (i.e., if workers place a positive value on their nonmarket time), then the availability of UI benefits may increase unemployment duration to the point where the marginal utility of nonmarket time equals the difference between the wage and the UI benefit level. Recently, Jerry Kingston et al. (1986) and Robert St. Louis et al. (1986) have suggested that many UI beneficiaries search for work less than the law requires, and that UI benefits subsidize leisure rather than job search.

The Illinois experiments provide the first opportunity to explore, within a controlled experimental setting, whether bonuses paid to UI beneficiaries or their employers would reduce the unemployment of beneficiaries relative to a randomly selected control group.

The experiments yield strong evidence on whether the UI system is simply a benign income transfer that changes no worker or employer behavior. However, these experiments alone do not resolve the question of how best to characterize the behavior of unemployed workers. In Section I, we describe the design and operation of the experiments, and sketch our data sources. Section II discusses the incentives facing experimental participants. Section III is a presentation of the responses to the experimental treatments, and Section IV a development of experimental benefits and costs. Section V is a discussion of the implications of the experiments.

I. Experimental Design and Operations

A. *Treatment Design*

Both treatments consisted of a $500 bonus payment. In the Claimant Experiment, $500 was paid to a *claimant* who had found a job within 11 weeks and held that job for four months. In the Employer Experiment, $500 was paid to an *employer* who hired an eligible claimant within 11 weeks of the initial claim and employed that claimant for four months. The size of the bonus, $500, reflected a balancing of the experiments' budget constraint (a maximum of $750,000 in bonus payments) against a somewhat arbitrary judgment about how small a bonus could still be expected to generate a response. For the average UI claimant, $500 was on the order of 5 percent of annual wage and salary payments, and represented about four weeks of UI benefit payments. Such an amount was believed large enough to provide an incentive to at least some claimants to accept employment more quickly (or search more intensely), and to at least some employers to hire a claimant.[1]

To qualify for the $500 bonus (or to make their employer qualify), claimants had to find a job of 30 hours or more per week within 11 weeks of filing their initial claim, and to hold that job for at least four months. The period of 11 weeks was chosen arbitrarily to be about 40 percent of the potential duration of benefits in Illinois, which is 26 weeks. It was also chosen to be less than the median duration of insured unemployment experienced by Illinois UI beneficiaries in the months preceding the experiment.[2]

[1] The $500 bonus was low compared with the marginal wage-bill subsidy provided under the Targeted Jobs Tax Credit, which was up to $3,000 in the first year after hiring an eligible worker. Because UI claimants would seem to be more attractive prospective employees than the disadvantaged workers who were eligible for the Targeted Jobs Tax Credit, it seemed likely that a smaller incentive might evoke a response from employers.

[2] Note that the 11-week period implies 10 weeks of benefit payments, because of the 1-week waiting period in Illinois.

Employment for four months was required to avoid the possibility of fraudulent hire, undertaken solely to obtain a bonus. Also, four months was regarded as the shortest period that would avoid payment of bonuses to seasonal workers and employers.

B. *Sample Design*

To be eligible for either the Claimant Experiment or the Employer Experiment, an individual had to 1) file an initial claim for UI between July 29, 1984 and November 17, 1984; 2) be eligible for 26 weeks of UI benefits; 3) register with one of 22 Job Service offices in northern and central Illinois; and 4) be at least 20 years old, but less than 55.

Imposing these eligibility criteria increased the homogeneity of the sample and excluded claimants whose behavior might be influenced by complicating factors. For example, enrolling claimants filing claims other than initial claims (i.e., additional, reopened, or transitional claims) would have meant admitting claimants eligible for anywhere from 1 to 26 weeks of UI benefits, and would have complicated the evaluation unnecessarily. Requiring enrollees to be Job Service registrants restricted the experiments to claimants who could be expected to obtain a job through usual market channels.[3] Excluding younger and older claimants was an attempt to reduce the number and kinds of complicating factors—special programs for young people, incentives to retire early for older workers—that might influence the job-finding behavior of those enrolled in the experiments.

Each claimant was assigned to one of three groups—the control group, the Claimant Experiment treatment group, or the Employer Experiment treatment group—by simple random assignment, based on the last two digits of his or her Social Security number. Hence, claimants had an equal probability of assignment to each of the three groups.

[3] Hence, workers on layoff with a definite recall date and union members who find jobs through a hiring hall were excluded from the design. In addition, recent veterans and federal employees were excluded.

C. *Site Selection and Sample Size*

Three variables could have been used to control the size of the sample: the number (and type) of sites, the length of the enrollment period, and the proportion of claimants selected at any given site. Of the 22 Job Service offices chosen as sites, 11 were in metropolitan Chicago, 2 were in the outlying metropolitan area, and 9 were in outlying northern and central Illinois. Limiting the experiments to these 22 offices, rather than enrolling claimants at every Job Service office in the state, lowered the costs of monitoring the experiments and of communicating with Job Service personnel. On the other hand, having a fairly large number of sites helped assure that experimental results would not be specific to one or few offices, and that the results would represent the response of the diverse demographic and industrial mix of Illinois. Also, having more sites permitted a shorter enrollment period, which was viewed as desirable in order to obtain results in a reasonable period of time.

The duration of the experiments (originally designed to be 13 weeks, ultimately 16) was selected with an eye to 1) the size of the bonus budget, and 2) achieving a sample large enough to allow detection of experimental responses that would be relevant to policy. As it turns out, the sample was large enough to detect, at the 5-percent significance level, a 0.017 change in the proportion of claimants who found a job within 11 weeks (for example, a change from 25.6 to 27.3 percent). Such a small response is at the lower bound of what could be regarded as economically significant to UI reform (see our report, 1987a, ch. 2).

D. *Operational Issues and Data*

Although the experimental design was unusually simple, implementing it within an existing agency posed a variety of complexities (our report, 1987a, chs. 2 and 3). In particular, several special instruments had to be created to monitor and track the experience of enrolled claimants. When a claimant registered at the Job Service office, a base-line

survey was administered to monetarily eligible claimants aged 20 through 54, and a special log was started on each claimant to record the claimant's treatment assignment and experience in the experiment. Claimants who were assigned to the Claimant Experiment or Employer Experiment groups were asked to sign an "agreement to participate," and those who agreed received a packet of administrative and instructional materials by mail once their nonmonetary eligibility for benefits had been determined. Participants in the Claimant Experiment who found a job within 11 weeks submitted a Notice of Hire (countersigned by the employer) to the Illinois Department of Employment Security (the Department), and those in the Employer Experiment who found a job within 11 weeks gave a Notice of Hire to their employer for submission to the Department. The Department then returned a voucher to the claimant or the employer, and the voucher was in turn submitted for $500 cash after the claimant had been employed continuously for four months. All of these transactions were recorded in the Job Service office logs.

To analyze treatment responses, we had access not only to the base-line survey and office logs, but also to the administrative data bases of the Department. Most important were 1) the Benefits Information System, which records the dates of claims filed and the amount and timing of benefits received, among other items, and 2) the Wage Records data base, from which we have drawn each claimant's quarterly earnings in UI-covered employment for the third quarter of 1983 through the third quarter of 1985 inclusive. Access to these administrative data frees us from the problem of selective attrition and associated nonresponse bias. That is, we have complete data on each claimant's experience in the experiment, on benefits received, and on earnings in covered employment before, during, and after the experiment.[4]

Table 1 displays basic data on experimental enrollment and use. Roughly 4,000 claimants who were eligible for UI benefits and eligible to participate in the experiments were assigned to each of the three experimental categories. Because the procedures used to construct these three samples were identical, each can be treated as a random sample from the population of fully eligible initial claimants for UI benefits who were aged 20 through 54.

The second row of Table 1 shows that there were important differences between the Claimant Experiment and Employer Experiment in claimants' willingness to participate in each. Whereas 84 percent of the eligible claimants who were offered the chance to participate in the Claimant Experiment agreed to participate, only 65 percent of those offered the chance to participate in the Employer Experiment agreed. Table 1 further indicates that actual use of the programs—as shown by return of Notices of Hire and actual cashing of bonuses—differed greatly between the Claimant Experiment and Employer Experiment. Whereas 14 percent of those assigned to the Claimant Experiment received a bonus, only 3 percent of those assigned to the Employer Experiment were responsible for a bonus payment to their employer.

We have explored participation in and use of the experiments in detail elsewhere (1987a, ch. 7), and for now note only two points about the Employer Experiment. First, the Employer Experiment was a more complicated treatment than the Claimant Experiment because it required the understanding and participation of both the worker and the employer. Second, the limited use of the Employer Experiment suggests that it had limited scope for reducing UI benefits paid or weeks of insured unemployment.

Table 2 displays some descriptive statistics of each of the three subsamples. Although these data on sex, race, and other variables may be of interest in their own right, their

[4] The data do impose an important limitation: We do not know the labor force status of claimants who terminated their benefits (particularly those who exhausted benefits) but did not reenter UI-covered employment. These claimants could have found a job in the uncovered sector, left the labor force, or remained unemployed (i.e., continued to seek work).

TABLE 1—ILLINOIS UNEMPLOYMENT INSURANCE EXPERIMENTS: PROGRAM PARTICIPATION AND USE

	Control		Claimant Experiment		Employer Experiment	
	N	Proportion	*N*	Proportion	*N*	Proportion
Eligible[a,b]	3,952	1.00	4,186	1.00	3,963	1.00
Agreed to Participate[c]	–	–	3,527	0.84	2,586	0.65
Submitted Notice of Hire[d]	–	–	765	0.18	199	0.05
Bonus Paid	–	–	570	0.14	112	0.03

Sources: Eligibility from Illinois Department of Employment Security, Benefits Information System; other data from office logs kept during the experiments.

[a]Eligible for UI benefits by both monetary and nonmonetary criteria, met the age and initial claim restrictions of the experiments, and were located in the Benefits Information System.

[b]A total of 17,306 claimants completed the base-line survey and were assigned to one of the three groups; 1,857 of these were monetarily ineligible, were not initial claimants (that is, were filing additional, reopened, or transitional claims), or could not be located in the Benefits Information System. An additional 3,348 (1,171 controls, 1,104 in the Claimant Experiment, and 1,073 in the Employer Experiment) were nonmonetarily ineligible (i.e., failed to meet separation and availability requirements, as determined by our constructed nonmonetary eligibility code), or failed to meet the age restrictions of the experiments.

[c]Agreed to participate according to Job Service office records.

[d]Includes participants who ultimately received a bonus but never submitted a Notice of Hire.

TABLE 2—CHARACTERISTICS OF CLAIMANTS ASSIGNED TO EXPERIMENTAL GROUPS

	Control		Claimant Experiment		Employer Experiment	
	N	Proportion	*N*	Proportion	*N*	Proportion
Total	3,952	1.000	4,186	1.000	3,963	1.000
Male	2,162	0.547	2,357	0.563	2,131	0.538
White	2,497	0.632	2,723	0.651	2,565	0.647
Black	1,072	0.271	1,050	0.251	1,014	0.256
Hispanic, Native American, Other	383	0.097	413	0.099	384	0.097
Age 20–29	1,680	0.425	1,827	0.436	1,679	0.424
Age 30–39	1,315	0.333	1,357	0.324	1,292	0.326
Age 40–49	708	0.179	776	0.185	740	0.187
Age 50–54	248	0.063	226	0.054	252	0.064
Weekly Benefit Amount:						
$51	347	0.088	355	0.085	333	0.084
$52–$90	794	0.201	887	0.212	861	0.217
$91–$120	666	0.169	738	0.176	711	0.179
$121–$160	749	0.190	822	0.196	716	0.181
$161	1,396	0.353	1,384	0.331	1,342	0.339
Dependents' Allowance	1,834	0.323	1,955	0.345	1,883	0.332

Source: Illinois Department of Employment Security, Benefits Information System.

Notes: "Weekly Benefit Amount" refers to weekly payment for which each claimant was eligible at the time of filing the initial claim. The sample excludes claimants who were ineligible for UI benefits for monetary and nonmonetary reasons (as determined by our constructed nonmonetary eligibility code), and who failed to meet the initial claim and age restrictions of the experiments. Hence, all initial claimants who met the program criteria and were eligible for UI benefits are included in the sample.

main import lies in the support they lend to the randomness of the three subsamples. Indeed, none of the differences in proportions between any pair of groups is statistically different from zero at conventional significance levels.[5] The randomness of the three subsamples along the lines of observable characteristics suggests that each of the three subsamples was indeed randomly drawn from the same population. It follows that comparisons between the Claimant Experiment group and the control group (or between the Employer Experiment group and the control group) implicitly control for all observed and unobserved variables that may have contributed to the outcomes that are of interest—duration of insured unemployment and post-reemployment earnings. Thus, a simple comparison of the mean weeks of insured unemployment for members of either experimental group with the mean weeks of unemployment for members of the control group will show the impact of the treatment in question on the duration of insured unemployment.

II. Incentives Facing Participants

The incentives facing UI claimants assigned to the Claimant Experiment may be viewed within the context of either a job search model or an income-leisure model of labor supply. In a job search context, the $500 bonus creates an incentive for unemployed workers either to lower their reservation wage during the 11-week qualification period and accept a job sooner than they would otherwise, or to search more intensely for a job in order to find a job sooner than they would otherwise. In a labor supply context, the $500 bonus raises the opportunity cost of leisure consumed during the period of time immediately following the initial claim, hence creating an incentive to substitute income for leisure. The basic results of the Claimant Experiment cannot distinguish whether one of these approaches (or some other approach) is the appropriate way to view how the Claimant Experiment worked; such an enterprise requires imposition of a structural model on the data and we do not propose to do that here.

To the extent that leisure or job search can be substituted easily from one time period to another, some workers may alter their behavior both during the period of the experiment and during the period after reemployment. We shall test for this possibility in what follows by examining the behavior of the treatment group relative to the control group before, during, and after the experiment.

Marginal wage-bill subsidies resembling the Employer Experiment have been implemented in the United States before. As Ashenfelter (1978b) has observed, the evaluation of such subsidies is complicated by the fact that the employers who would add workers to their payrolls even without the subsidy have the greatest incentive to participate in such programs.[6] If an employer pays the same wage rate to all workers of a given skill class, then an employer who hires an Employer Experiment enrollee will simply receive a subsidy for hiring the enrollee rather than someone else. Clearly, then, a worker assigned to the Employer Experiment has an increased probability of being hired, but it does not follow that the Employer Experiment must increase total employment. The higher the turnover rate of workers in a labor market, given the ratio of Employer Experiment enrollees to total job seekers in that market, the less likely it is that the Employer Experiment will raise total employment. Nevertheless, if the demand for labor is sufficiently elastic, the Employer Ex-

[5] We have explored differences between the control group and the experimental groups in several other observable variables, and have found no statistically significant differences.

[6] There is of course a similar possibility in the Claimant Experiment. Some workers assigned to the Claimant Experiment—i.e., those whose planned unemployment durations were shorter than 11 weeks—could receive a lump sum bonus without altering their behavior at all. Nevertheless, if there is uncertainty about whether one can find a job to fulfill precisely one's plan, even these workers (if they are risk averters) may be induced by the $500 bonus to become reemployed sooner than they otherwise would.

periment bonus may induce an employer who was not planning to hire additional workers to increase total employment.[7]

A final consideration is that a worker assigned to the Employer Experiment may bargain with an employer to receive a wage payment that is (in expected present value terms) higher than would otherwise be the case by precisely the $500 bonus that the employer will receive. If workers could strike such bargains, which could be thought of as "Coasean deals," then the Employer Experiment and the Claimant Experiment would establish precisely the same incentives for workers, and the two experimental treatments should have precisely the same effect on workers' unemployment and earnings (net of the bonus).[8]

III. Responses to the Treatments

A. *Effects on Benefit Receipt and Duration of Insured Unemployment*

Table 3 displays the means of several program variables by experimental group. These means are based on the sample of all fully eligible claimants—the same sample that underlies Tables 1 and 2. We stress that this sample *includes* eligible claimants who refused to participate in one of the experiments, so that the Claimant Experiment and Employer Experiment groups are fully comparable with the control group. (Examining only Claimant Experiment and Employer Experiment group members who agreed to participate would involve a comparison of self-selected groups—Claimant Experiment and Employer Experiment *agreers*—with all controls, some of whom would have refused participation had they been offered the opportunity. The result could be biased estimates of the experimental effects.)

Row 1 under "Benefits Paid" ("State Regular, First Spell") shows that the mean dollar amount of state regular benefits received by members of the control group during the spell of unemployment immediately following the initial claim was $2,267. For eligible claimants assigned to the Claimant Experiment and Employer Experiment, the comparable figures are $2,074 and $2,159. Row 2 under "Benefits Paid" shows the mean of the sum of state regular benefits and Federal Supplemental Compensation received during the spell of unemployment immediately following the initial claim. Rows 3 and 4 show state regular benefits received, and the sum of state regular and Federal Supplemental Compensation received, but this time for the entire benefit year.

The full benefit year is the appropriate time period to examine in determining the impact of the experiments on benefit receipt, rather than just the spell of insured unemployment following the initial claim. As noted in Section II, it is possible that the experimental treatments created incentives to redistribute insured unemployment over the benefit year, with insured unemployment dropping immediately following the initial claim, but increasing in the latter part of the benefit year to compensate. We can capture this effect, if it exists, by examining benefit receipt and weeks of insured unemployment over the full benefit year.

The two rows under "Weeks of Insured Unemployment" show the means—for the spell of unemployment immediately following the initial claim, and for the full benefit year—of the number of weeks of insured unemployment for each group. It is worth emphasizing that statistical tests performed on these means are valid if interpreted as tests on the number of weeks of *insured* unemployment, because weeks of insured unemployment are a censored measure of total unemployment.

The entries at the bottom of Table 3 show the proportion of claimants who exhausted their state regular benefits, and the proportion of claimants who terminated benefits within 11 weeks of filing their initial claim.

Table 4 displays differences between the mean values of the control group and the

[7]For example, John Bishop (1987, ch. 4) has recently concluded that the Targeted Jobs Tax Credit did result in a net increase in employment in participating firms.

[8]See Ronald Coase (1960). Of course, it is also possible for the Claimant Experiment and the Employer Experiment to have identical effects without such Coasean deals occurring.

TABLE 3—MEANS OF PROGRAM VARIABLES BY EXPERIMENTAL GROUP[a]

	Control		Claimant Experiment		Employer Experiment	
	Mean	*SE* of Mean	Mean	*SE* of Mean	Mean	*SE* of Mean
Benefits Paid ($):						
1) State Regular, First Spell	2,267	27.5	2,074	26.7	2,159	27.4
2) Total, First Spell	2,558	33.8	2,329	32.9	2,446	33.8
3) State Regular, Benefit Year	2,487	27.0	2,328	26.3	2,426	27.0
4) Total, Benefit Year	2,786	33.1	2,592	32.2	2,725	33.8
Weeks of Insured Unemployment:						
1) First Spell	18.3	0.205	17.0	0.199	17.7	0.205
2) Benefit Year	20.1	0.194	18.9	0.188	19.7	0.194
	Proportion	*SE* of Proportion	Proportion	*SE* of Proportion	Proportion	*SE* of Proportion
Proportion of Claimants Who:						
1) Exhausted Benefits	0.478	0.008	0.446	0.008	0.464	0.008
2) Ended Benefits within 11 weeks	0.353	0.008	0.408	0.008	0.384	0.008
N	3,952		4,186		3,963	

Sources: Tabulations from Illinois Department of Employment Security, Benefits Information System, and office logs.
Notes: "First Spell" refers to the spell of unemployment immediately following the initial claim for UI. "Total Benefits Paid" refers to the sum of state regular benefits and Federal Supplemental Compensation. "Benefit Year" refers to benefits paid or weeks of benefits paid during the full benefit year for each claimant. "Ended Benefits within 11 Weeks" refers to termination of benefits within 11 weeks of filing the initial claim (equivalently, 10 or fewer weeks of benefit payments, because of the 1-week waiting period). Sample is the same as that underlying Table 2.
[a]*SE* denotes standard error.

Claimant Experiment group, and between the control group and the Employer Experiment group. The differences are calculated from Table 3, and the standard error of each difference is shown. The most striking results shown by the table pertain to the Claimant Experiment: Average benefit receipt was lower in the Claimant Experiment group than in the control group by $158 to $194 over the full benefit year (depending on whether Federal Supplemental Compensation benefits are included in benefits received). The differences are statistically different from zero at the 1-percent level (using a two-tailed test). Further, the average number of weeks of insured unemployment was lower in the Claimant Experiment group than in the control group by somewhat over a week, again measured over the full benefit year.

The Claimant Experiment results are quite strong in that the $158 to $194 benefit reduction, and the 1.15-week reduction in the duration of unemployment, were attained on average over all eligible workers who were assigned to the Claimant Experiment, whether or not they agreed to participate, and whether or not they actually cashed a voucher for $500. Further, compared with the control group, 5.5 percent more of those assigned to the Claimant Experiment ended their spell of insured unemployment within 11 weeks of filing, and 3.2 percent fewer exhausted their UI benefits. These last findings suggest that the Claimant Experiment

TABLE 4—DIFFERENCES BETWEEN CONTROL GROUP AND EXPERIMENTAL GROUP MEANS

	Claimant Experiment minus Control		Employer Experiment minus Control	
	Difference of Means	*SE*	Difference of Means	*SE*
Benefits Paid ($):				
1) State Regular, First Spell	−193[a]	38.30	−108[a]	38.82
2) Total, First Spell	−229[a]	47.15	−112[b]	47.79
3) State Regular, Benefit Year	−158[a]	37.70	−61	38.21
4) Total, Benefit Year	−194[a]	47.17	−61	46.80
Weeks of Insured Unemployment:				
1) First Spell	−1.37[a]	0.29	−0.67[b]	0.29
2) Benefit Year	−1.15[a]	0.29	−0.36	0.27
	Difference of Proportions	*SE*	Difference of Proportions	*SE*
Proportion of Claimants Who:				
1) Exhausted Benefits	−0.032[a]	0.011	−0.014	0.011
2) Ended Benefits within 11 Weeks	+0.055[a]	0.011	+0.031[a]	0.011

Source: Calculations based on Table 3. See Notes and fn. a, Table 3.
[a]Rejection of the hypothesis that the difference of means is zero using a two-tailed 1-percent significance level.
[b]Rejection of the hypothesis that the difference of means is zero using a two-tailed 5-percent significance level.

reduced the duration of insured unemployment of workers throughout the distribution of unemployment spell lengths.

The results of the Employer Experiment are quite different. Although there was an initial reduction in benefits received by the Employer Experiment group in the spell of unemployment immediately following the initial claim, the reduction in benefits paid to the Employer Experiment group over the full benefit year is statistically insignificantly different from zero. The evidence of an impact of the Employer Experiment on the number of weeks of insured unemployment is similar. Although there was a reduction in unemployment during the first spell, no statistically significant difference between the control group and the Employer Experiment group exists over the full benefit year.

In view of the comparatively low rate of use of the Employer Experiment, it may seem surprising that there was an impact even in the first spell of unemployment. The results suggest that, to the extent the Employer Experiment reduced the length of the initial spell of unemployment, this effect did not persist over the full benefit year. However, evidence presented in Section III, Part C, shows that the Employer Experiment did reduce the benefits paid and the weeks of insured unemployment of white women over the full benefit year. Hence, these overall results mask an effect of the Employer Experiment on at least one major group of UI claimants.

B. *Effects on Earnings after Reemployment*

The above results suggest strongly that the Claimant Experiment reduced the duration of job search for claimants who participated in it. It is possible—indeed, Mortensen's and McCall's early models of job search suggest

TABLE 5—MEAN PRE- AND POSTPROGRAM EARNINGS OF ELIGIBLE CLAIMANTS WITH EARNINGS IN QUARTER AFTER BENEFIT TERMINATION, BY EXPERIMENTAL GROUP

Mean Earnings in:	Control	Claimant Experiment	Employer Experiment
Base Period (Average of Four Quarters)	$3,226 (44.7) ($N = 2,531$)	$3,243 (44.4) ($N = 2,786$)	$3,203 (45.3) ($N = 2,550$)
Quarter before Initial Claim	3,995 (53.1) ($N = 2,357$)	3,965 (48.0) ($N = 2,591$)	3,916 (50.0) ($N = 2,386$)
Quarter after Benefit Termination	3,121 (47.3) ($N = 2,531$)	3,129 (46.7) ($N = 2,786$)	3,066 (46.7) ($N = 2,550$)

Notes: Standard error of each mean is shown in parentheses. The sample is constructed as follows: Starting with fully eligible claimants who met the initial claim and age restrictions of the experiments, samples of those who showed positive earnings in the quarter after they terminated benefits (2,531 controls, 2,786 Claimant Experiment enrollees, and 2,550 Employer Experiment enrollees) were used to compute mean earnings in the base period, the quarter before the initial claim, and the quarter after benefit termination. Note that all means are computed conditional on positive earnings; thus, N used to compute mean earnings in the quarter before the initial claim is lower than elsewhere because not all claimants in the sample showed earnings in the preclaim quarter.

—that the shorter search time induced by the $500 bonus may result in a less-favorable match between worker and job, which would manifest itself in lower earnings in the subsequent job. If a Claimant Experiment participant who submitted a Notice of Hire (or received a bonus) effectively lowered his or her reservation wage and simply accepted the first job that presented itself, then the claimant's earnings after reemployment and the efficiency of the labor market would both be reduced.[9]

Table 5 addresses the concern that Claimant Experiment participants may have sacrificed earnings in their postprogram job in order to obtain the $500 bonus. The table displays data on the pre- and postprogram earnings of claimants in each of the three groups. All figures are based on the subsample of claimants who terminated benefits (at some point following the initial claim that brought them into the experiment), and had positive earnings in the first full quarter following benefit termination. That is, claimants who exhausted benefits and failed to find a new job, and claimants who dropped out of the labor force, are excluded from consideration here. Since our concern focuses on the earnings of those who found a new job, and whether these earnings are lower for the Claimant Experiment group, this is clearly the appropriate group to examine.

The first row of Table 5 shows average base period earnings of claimants in each of the three groups, and the second row shows earnings in the quarter before the initial claim was filed. Note that there is no statistically (or otherwise) significant difference across groups in either of these preprogram earnings measures.[10] (The sample on which earnings in the quarter before the initial claim is based is smaller than the sample used to calculate the other figures in the table because not all claimants had earnings in the quarter before they filed for benefits.)

The third row of Table 5 shows, for each of the three groups, earnings in the first full quarter after benefit termination (for those claimants who had earnings after benefit termination). The figures suggest strongly

[9] Kathleen Classen (1979) has shown that, in search models that relax some of the assumptions of the early Mortensen and McCall models, the relation between unemployment duration and earnings after reemployment is ambiguous. See also Kenneth Burdett (1979).

[10] For mean earnings in the base period, the standard error of the difference between either experimental group and the control group is about $63, which overwhelms either of the mean differences ($17 and $23). The standard error of the differences is about $72 for mean earnings in the quarter before the initial claim, and about $67 for mean earnings in the quarter after benefit termination.

TABLE 6—DIFFERENCE BETWEEN CONTROL GROUP AND EXPERIMENTAL GROUP MEANS BY RACE AND SEX

Race/Sex	Difference in Benefits Paid ($): Control vs.		Difference in Weeks of Benefits: Control vs.	
	Claimant Experiment	Employer Experiment	Claimant Experiment	Employer Experiment
White Women	−262.0[a]	−164.1[b]	−1.623[a]	−1.008[b]
(1,113; 1,170; 1,166)	(70.7)	(70.7)	(0.503)	(0.503)
White Men	−185.1[a]	−9.8	−1.125[b]	+0.192
(1,384; 1,553; 1,399)	(62.4)	(63.6)	(0.444)	(0.455)
Black Women	−65.9	−49.3	−0.978	−0.539
(527; 512; 504)	(104.7)	(105.1)	(0.745)	(0.748)
Black Men	−72.7	−33.2	−0.518	−0.211
(545; 538; 510)	(115.6)	(104.3)	(0.730)	(0.742)

Notes: A negative difference implies that the experimental mean is less than the control mean. Standard error of each difference is in parentheses under the difference. Number of observations in the control, Claimant Experiment, and Employer Experiment groups for each race and sex category is shown in parentheses in the first column. Sample sizes for Hispanics, Native Americans, and others are too small to allow detection of experimental effects of the magnitude found in the overall sample; hence, these groups are not considered separately.

[a,b]See Table 4.

that there is no difference between the post-program earnings of controls and of Claimant Experiment workers—the average for the controls is $3,121, whereas the average for the Claimant Experiment group is $3,129. The difference, $8, is swamped by the standard error of that difference, which is $67. We conclude that the relatively rapid reemployment of Claimant Experiment participants did *not* come at the expense of lower earnings. Rather, the data are consistent with the idea that the faster reemployment of Claimant Experiment workers resulted from more-intense job search efforts by Claimant Experiment workers, and not from overly rapid acceptance of job offers.[11]

C. *Effects by Race and Sex*

Those enrolled in the experiments compose a diverse group, and it is possible that certain groups of workers responded more strongly than others to the experimental incentives. We have explored elsewhere (1987a, ch. 6) the effects of the experiments on several different categories of workers and we believe one of these breakdowns to be particularly significant.

Table 6 displays the effects of the experiments on 1) state regular benefits paid to claimants during their full benefit year, and 2) the number of weeks of benefits paid to claimants during the full benefit year, broken down by four race and sex categories.[12] These results are dramatic because they show that, for white women, the Employer Experiment unambiguously reduced UI benefit payments and weeks of insured unemployment. Moreover, we have found that no loss of earnings accompanied the response of white women and their employers to the Employer Experiment (1987a, ch. 6, Table 6–9). White women stand out sharply from the other three race and sex categories, each of whom showed a response to the Employer Experiment that is not statistically significantly different from zero.

Table 6 also shows that the effects of the Claimant Experiment appear to have varied

[11]An interpretation within the framework of labor supply is also possible. If a single wage offer (constant over time) faced a worker, then increased search intensity would be unnecessary in order to obtain a job at a given wage. Participating claimants could then cut short their spell of unemployment by substituting income for leisure in the current period.

[12]Race and sex subgroups other than the four shown are too small to allow detection of experimental effects of the magnitude found in the overall sample.

by race. The Claimant Experiment brought about a larger reduction in the UI benefits and weeks of insured unemployment of both white women and white men than of black women and black men.[13] Indeed, the effect of the Claimant Experiment on the insured unemployment of blacks is not statistically significantly different from zero.

Some insight into the differences by race and sex in the effects of the experiments can be obtained from Table 7. The first panel shows the proportion of each race and sex group in each experiment that received a bonus. Here we see that whites were nearly three times more likely to receive a Claimant Experiment bonus than were blacks. It is also shown that whites' employers were roughly ten times more likely to receive an Employer Experiment bonus than were blacks' employers.

We can decompose the difference between any two groups in their propensities to receive a bonus into two components. One component reflects the difference between the groups in their probabilities of qualifying for a bonus (or in their probabilities of qualifying an employer for a bonus), and the other reflects the difference in the probabilities that qualifying workers (or their employers) will actually cash a voucher for $500. Define b_{it} as the proportion of race and sex group i (i = white women, white men, black women, and black men) in treatment group t (t = control, Claimant Experiment, and Employer Experiment) who actually receive a bonus. Also, define e_{it} as the proportion of all claimants in group i,t who qualify for a bonus (because they find a job within eleven weeks and hold it for four months), and r_{it} as the proportion of *qualifying* claimants in group i,t who actually receive a bonus (or whose employers do). (We will call this conditional probability r_{it} the "take-up rate" because it reflects the proportion of qualifying claimants who use the program.) We can then express the proportion of a group who receive a bonus (b_{it}) as the product of the proportion qualifying and the take-up rate: $b_{it} = e_{it}r_{it}$.

Since we are interested in differences between groups in their propensities to receive a bonus, it is natural to write the proportional difference between two groups as

$$(1) \qquad b_{it}/b_{js} = (e_{it}r_{it})/(e_{js}r_{js}).$$

Taking the natural logarithm of both sides and rearranging gives

$$(2) \quad \ln(b_{it}/b_{js}) = \ln(e_{it}/e_{js}) + \ln(r_{it}/r_{js}).$$

This is the desired decomposition. It attributes differences between two groups' propensities to receive a bonus to differences between the two groups in (a) their probabilities of qualifying for a bonus, and (b) their probabilities of cashing a voucher if they qualify.

The data required to compute any such decomposition are displayed in Table 7. The second panel shows the proportion of each race and sex group in each experimental group that (by finding a job within 11 weeks and holding the job for four months) qualified for a bonus. The third panel shows the take-up rate of each group. If we were interested in decomposing the difference between white and black women's probabilities of receiving a Claimant Experiment bonus, we would compute:

$$\ln(b_{wf,a}/b_{bf,a}) = \ln(0.1761/0.0625) = 1.036;$$

$$\ln(e_{wf,a}/e_{bf,a}) = \ln(0.300/0.158) = 0.641;$$

$$\ln(r_{wf,a}/r_{bf,a}) = \ln(0.5870/0.3951) = 0.396.$$

(Subscripts *wf* and *bf* denote white and black females, and subscript *a* denotes the Claimant Experiment.) The decomposition suggests that 62 percent of the difference between white and black women in their propensities to receive Claimant Experiment bonuses can be attributed to the higher probability that white women qualified for a bonus, and that 38 percent of the difference can be attributed

[13] A case could be made that black women showed a stronger response to the Claimant Experiment treatment than did black men.

TABLE 7—PROPORTION OF CLAIMANTS WHO RECEIVED A BONUS, QUALIFIED FOR A BONUS, TAKE-UP RATE, AND EXPERIMENTAL COSTS AND BENEFITS, BY RACE AND SEX

	Control	Claimant Experiment	Employer Experiment
1) Proportion Receiving a Bonus:			
Total	–	0.1362	0.0283
		(0.0053)	(0.0026)
White Women	–	0.1761	0.0463
		(0.0111)	(0.0062)
White Men	–	0.1700	0.0358
		(0.0095)	(0.0050)
Black Women	–	0.0625	0.0040
		(0.0107)	(0.0030)
Black Men	–	0.0632	0.0039
		(0.0105)	(0.0028)
2) Proportion Qualifying for a Bonus:			
Total	0.207	0.250[a]	0.228[a]
	(0.006)	(0.007)	(0.007)
White Women	0.237	0.300[a]	0.280[a]
	(0.013)	(0.013)	(0.013)
White Men	0.244	0.277[a]	0.258
	(0.012)	(0.011)	(0.012)
Black Women	0.139	0.158	0.153
	(0.015)	(0.016)	(0.016)
Black Men	0.127	0.165	0.120
	(0.014)	(0.016)	(0.014)
3) Take-up Rate:			
Total	–	0.5448	0.1241
		(0.0077)	(0.0052)
White Women	–	0.5870	0.1654
		(0.0263)	(0.0109)
White Men	–	0.6139	0.1388
		(0.0235)	(0.0092)
Black Women	–	0.3951	0.0261
		(0.0543)	(0.0071)
Black Men	–	0.3821	0.0325
		(0.0515)	(0.0079)
4) Bonus Cost per Assigned Claimant ($):			
Total	–	68.1	14.2
		(2.7)	(1.3)
White Women	–	88.1	23.2
		(5.6)	(3.1)
White Men	–	85.0	17.9
		(4.8)	(2.5)
Black Women	–	31.3	2.0
		(5.4)	(1.5)
Black Men	–	31.6	2.0
		(5.3)	(1.4)
5) Ratio of Benefit Payment Reduction to Bonus Cost per Assigned Claimant:			
Total	–	2.32	4.29
		(0.46)	(2.90)
White Women	–	2.97	7.07
		(0.61)	(2.95)
White Men	–	2.18	0.55
		(0.61)	(3.48)
Black Women	–	2.11	24.65
		(2.98)	(34.06)
Black Men	–	2.30	16.60
		(3.27)	(40.50)
6) Size of Group (N):			
Total[b]	3,952	4,186	3,963
White Women	1,113	1,170	1,166
White Men	1,384	1,553	1,399
Black Women	527	512	504
Black Men	545	538	510

Note: Standard errors in parentheses. The proportion of each group qualifying for a bonus (second panel) is the proportion of the group that found a job within 11 weeks of filing the initial claim *and* held the job for four months. A group's take-up rate (third panel) is the proportion of the group receiving a bonus divided by the proportion qualifying for a bonus. A group's bonus cost per assigned claimant (fourth panel) equals total bonus payments to the group divided by the size of the group (N). The ratio of benefit payment reduction to bonus cost per assigned claimant (fifth panel) states the reduction in UI benefit payments for each dollar of bonus paid. The standard error of each ratio in the fifth panel is approximated by taking a Taylor expansion for the ratio.

[a] Rejection of the hypothesis that the difference of proportions between the experimental and control groups is zero using a two-tailed 5-percent significance level.

[b] The sum of white women, white men, black women, and black men is less than the total because the total includes Hispanics, Native Americans, and others.

TABLE 8—DECOMPOSITION OF DIFFERENCES IN PROBABILITIES OF BONUS RECEIPT, SELECTED GROUP COMPARISONS

Comparison	Difference in Proportions Receiving Bonus	Percentage of Difference Attributable to: Difference in Proportions Qualifying	Percentage of Difference Attributable to: Difference in Take-up Rates
Women in Claimant Experiment with Men in Claimant Experiment	0.0009 (0.0100)	[a]	[a]
Whites in Claimant Experiment with Blacks in Claimant Experiment	0.0979 (0.0112)	58	42
Women in Employer Experiment with Men in Employer Experiment	0.0073 (0.0100)	[a]	[a]
Whites in Employer Experiment with Blacks in Employer Experiment	0.0327 (0.0113)	29	71
Women in Claimant Experiment with Women in Employer Experiment	0.1045 (0.0073)	3	97
Men in Claimant Experiment with Men in Employer Experiment	0.1109 (0.0067)	8	92
Whites in Claimant Experiment with Whites in Employer Experiment	0.1241 (0.0057)	5	95
Blacks in Claimant Experiment with Blacks in Employer Experiment	0.0590 (0.0098)	6	94

Note: Standard error of the difference in proportions receiving a bonus is shown in parentheses below the difference. Equation (2) in the text defines the decomposition displayed in the table.

[a] The difference in proportions receiving a bonus is statistically insignificant; therefore, no decomposition is shown.

to the higher probability that qualifying white women actually cashed a $500 voucher.

Some further decompositions are shown in Table 8. To keep Table 8 of reasonable length only four groups—all women, all men, all whites, and all blacks—are used in the comparisons (rather than the less aggregate race and sex categories shown in Table 7). The top four rows of Table 8 display comparisons of different groups within each experiment, whereas the bottom four rows display comparisons of the same group between experiments.

The differences between women and men in bonus receipt within each experiment are statistically insignificant and hence no decompositions are shown (first and third rows of the table). But the differences between whites and blacks within experiment are more revealing (second and fourth rows). These decompositions show that differences between whites and blacks in bonus receipt

can be attributed to white-black differences both in the probabilities of qualifying for a bonus and in take-up rates. In the Claimant Experiment, 58 percent of the higher propensity of whites to receive a bonus can be attributed to whites' higher probability of qualifying, and 42 percent to whites' higher take-up rate. In the Employer Experiment, only 29 percent of the higher propensity of whites to receive a bonus can be attributed to whites' higher probability of qualifying, and 71 percent to the higher take-up rate of whites and their employers.

Differences between the Claimant Experiment and the Employer Experiment in bonus receipt are decomposed in the bottom four rows of Table 8. The decompositions show clearly that the difference between the experiments in bonus receipt must be attributed mainly to higher take-up rates in the Claimant Experiment.

IV. Benefit-Cost Ratios and Take-up Rates

The fourth and fifth panels of Table 7 display information on the costs and benefits of the experiments to the state. The fourth panel shows the bonus cost per assigned claimant for each experimental and race-sex group. For example, the figure \$68.10 for bonus cost per assigned claimant in the Claimant Experiment equals the dollar amount paid in Claimant Experiment bonuses (\$500 times 570 bonuses) divided by the number of claimants assigned to the experiment (4,186). Since the Claimant Experiment was more heavily used than the Employer Experiment, its costs per assigned claimant were higher.

The cost figures displayed in the fourth panel of Table 7 can be used to derive a benefit-cost ratio for each experimental and race-sex group. These ratios are shown in the fifth panel. They are obtained by dividing the reduction in UI benefit payments per claimant (that is, the average treatment responses shown in Tables 4 and 6) by the bonus cost per assigned claimant from the fourth panel of Table 7. For example, the benefit-cost ratio for the Claimant Experiment, 2.32, equals the average treatment response (\$158) divided by the bonus cost per assigned claimant (\$68). This benefit-cost ratio is statistically significantly different from zero. A straightforward interpretation of this benefit-cost ratio is that, for the Claimant Experiment overall, state regular benefit payments were reduced by \$2.32 for each \$1.00 of bonus payments made. It follows that a program modeled on the Claimant Experiment would be extremely attractive from the state's point of view if the presence of the program did not increase unemployment among workers who were not participants in the program.

The fifth panel also shows that the overall benefit-cost ratio for the Employer Experiment is 4.29, but it is not statistically different from zero. The benefit-cost ratio for white women in the Employer Experiment, however, is 7.07, and is statistically different from zero. Hence, a program modeled on the Employer Experiment also might be attractive from the state's point of view if the program did not increase unemployment among nonparticipants. Since, however, the Employer Experiment affected only white women, it would be essential to understand the reasons for the uneven effects of the treatment on different groups of workers before drawing conclusions about the efficacy of such a program.

It is important to consider these benefit-cost ratios in relation to the take-up rates developed in the previous section and displayed in the third panel of Table 7. Those figures show that only 54 percent of the Claimant Experiment workers who qualified for a \$500 bonus actually took the steps to claim the bonus. It is also striking that only 12 percent of the employers who could have received a \$500 bonus simply by claiming it actually did so.

If a program modeled on either the Claimant or Employer Experiment were implemented, it is possible that claimants' or employers' take-up rates would be substantially higher than those observed in the experiments. Futher, if increased take-up rates were unaccompanied by additional reductions in UI benefits and unemployment, then the benefit-cost ratio would decline. For example, in the Claimant Experiment, if 100 percent of the qualifying claimants had claimed

a bonus, then the benefit-cost ratio would fall from 2.32 to 1.26. In the Employer Experiment, if 100 percent of qualifying employers had claimed a bonus, then the benefit-cost ratio would be only 0.53. (For employers of white women in the Employer Experiment, a 100 percent take-up rate would lead to a decrease in the benefit-cost ratio from 4.29 to 1.17.) Again, these calculations assume an increase in the take-up rate without any accompanying behavioral response. How take-up rates would change in an actual program, and whether there would be further reductions in UI benefit payments and unemployment, are important topics for further research and experimentation.

V. Conclusion

The results of the Claimant Experiment are unequivocal and strong. The incentive created by the $500 bonus, which was actually paid to only 570 out of 4,186 UI claimants assigned to the experiment, reduced state regular benefits paid to the randomly selected treatment group by an average of $158, and reduced average weeks of insured unemployment by more than one week (over the full benefit year), compared with the randomly selected control group. We reemphasize that these reductions in average benefit payments and weeks of unemployment were achieved over all 4,186 eligible claimants in the Claimant Experiment sample, whether or not they agreed to participate or acted on the incentive.

Some of the results of the Employer Experiment are also unequivocal and strong. White women who were randomly assigned to the Employer Experiment received $164 less in UI benefits, and experienced one week less of insured employment (over the full benefit year), than did white women who were randomly assigned to the control group. These reductions were achieved on average over all 1,166 white women who were assigned to the Employer Experiment. Only 54 of these women were responsible for a $500 bonus being received by the employer who hired them. White women are the only race and sex group who experienced a statistically significant effect of the Employer Experiment.

It is clear that the members of the experimental treatment groups experienced less unemployment than the randomly selected control group. Thus, we may unambiguously reject the hypothesis that the unemployment insurance benefit system is only a benign or nondistortionary income transfer. The results reported here are the first experimental demonstration of this proposition, and we believe them to be quite convincing.

It is also clear that the members of the experimental groups were, on average, made no worse off by their assignment to the experiments. On the other hand, it is not possible to be certain whether the control group's experience was identical to what it would have been in the absence of any experimental treatments. This is a problem shared by all controlled social experiments. Moreover, we have not addressed whether a full-scale program modeled on either the Claimant or Employer Experiment would have significant displacement effects—that is, would result in improvements for program participants at the expense of nonparticipants. Hence, we cannot conclude unambiguously what the net social benefits of such a program might be.

Another limitation of our results stems from our finding that many claimants, and most employers, failed to take the steps to obtain a $500 bonus even when they qualified to do so. If the take-up rate in the Claimant Experiment had been 100 percent (that is, if all claimants who qualified for a bonus had claimed one), and UI benefit reductions had remained unchanged, then the benefit-cost ratio of the experiment would have fallen to 1.26 from 2.32. If the take-up rate in the Employer Experiment had been 100 percent, then the benefit-cost ratio would have fallen to 0.53 from 4.29. (For white women in the Employer Experiment, the benefit-cost ratio would have fallen to 1.17 from 4.29.) Thus, even with a 100 percent take-up rate, the Claimant Experiment would have reduced UI benefit costs by considerably more than the bonus costs incurred; however, the Employer Experiment (overall) would not. Because the take-up rates observed in the experiments could understate those that would occur in a program modeled on one of the experiments, these findings pose im-

portant questions for further research and experimentation: Would take-up rates change in an actual program, and if so, how? To what degree would changes in take-up rates be accompanied by further reductions in UI benefit payments and unemployment?

Our analysis of the experimental data does not allow us to determine conclusively why the experimental treatments changed the participants' behavior. We do have three clear findings, however, that may be of considerable significance for evaluating alternative models of worker behavior: 1) Offering unemployed workers the option of assigning bonuses to employers who hired them had a far smaller effect on employment than offering unemployed workers the same bonus directly. 2) The lower unemployment of members of the Claimant Experiment group occurred for workers at all parts of the distribution of unemployment spell lengths. 3) The earnings of members of the treatment groups after they became reemployed did not differ from the earnings of members of the control group. It seems clear that additional analysis of these data will be needed before further conclusions can be reached, particularly conclusions about which of various models of worker and firm behavior are supported.

Because such strong inferences can be derived from experimental work, the desirability of further experimentation seems unambiguous. A significant improvement in the design of the Illinois experiments over the design of the income maintenance experiments of the 1960's and 1970's is the reliance on administrative records rather than surveys for the collection of information about the treatment and control groups.[14] Use of administrative records is an important development in field experimentation in economics. Experiments that rely solely on survey records are vastly more expensive than those that rely on administrative data. Moreover, experiments that rely on survey data to measure responses run the risk of confounding measures of the experimental treatment with measures of attrition from the survey, as Ashenfelter and Mark Plant (1987) have recently noted.

Although the Claimant and Employer Experiments in Illinois have yielded strong results about the effects of experimental bonuses to workers and employers, additional research and experimentation along the lines of these experiments could usefully address three different and important issues. First, it would be desirable to measure the response to variation in (a) the size of the bonus provided to successful job seekers and to employers, and (b) the length of the period within which a UI claimant must become reemployed in order to qualify for a bonus. Second, it is important to learn how a longer and more widespread experimental bonus program would influence employee and employer take-up rates as well as UI benefit reductions. Third, analysis and experimentation that would shed light on the extent to which differences between treatment and control groups in unemployment experience represent costs to the control group (or other nonparticipants) would be of special importance. Further experiments hold out the possibility of designing new programs that may effectively reduce unemployment at low or even negligible cost to unemployed workers and to society.

[14] In our earlier article (1987b, Sec. I.C), we discuss the advantages of administrative data, particularly in avoiding the problems associated with selective attrition. For analyses of attrition in the income maintenance experiments, see Harold Watts et al. (1977), and Philip Robins and Richard West (1980).

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[Footnotes]

[8] **The Problem of Social Cost**

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[14] **Program Participation and Labor-Supply Response**

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