

The Illinois Unemployment Insurance Incentive Experiments

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EXECUTIVE SUMMARY

From mid-1984 to mid-1985, the Illinois Department of Employment Security conducted an experiment designed to test the effectiveness of bonus offers in reducing the duration of insured unemployment. This experiment was funded under the Wagner-Peyser Act, which allows states to use funds "for exemplary models for the delivery of services." Illinois Governor James R. Thompson reviewed this proposed project and approved use of his ten percent (10%) discretionary money under Wagner-Peyser 7(b). The experiment had two distinct treatments.

The first, called the Job Search Incentive Experiment (JSIE), offered new UI claimants a cash bonus of \$500 if they met the following conditions:

- *Filing initial claims, and eligible to receive benefits.
- *Obtained employment before receiving 11 weeks of benefits.
- *Employed continually for 4 months.
- *Worked on the job 30 or more hours per week.

The second, called the Hiring Incentive Experiment (HIE), offered an employer of a new UI claimant a cash bonus of \$500 for hiring a participating UI claimant who met the same conditions as stated above.

The purposes of the experiment were to determine the following:

1. Do incentive payments to claimants influence their job search behavior?
2. Do incentive payments to employers influence their hiring behavior?
3. Do incentive payments paid either to claimants or employers reduce the duration of insured unemployment?
4. Are incentive payments an efficient means of reducing the flow of benefits from the UI Trust Fund?

The experiment was conducted in 22 designated Job Service offices in northern and central Illinois, including the City of Chicago. About 17,000 new UI claimants who registered with the Job Service were randomly assigned to one of the two treatments or the control group according to the last two digits of their Social Security numbers. The success of the randomization process in generating identical samples for assignment to each of the two experiments and the control group is shown in Table 5-2. The Job Service, rather than the UI office, was the place of enrollment in order to limit the experiment to those UI claimants who were legally required to register with the Job Service and actively engage in job search. (Excluded categories include

recent veterans, federal employees, members of unions with associated hiring halls, and claimants on layoff with recall dates within 4 weeks.)

For each claimant assigned to a treatment, a Job Service specialist described the treatment and asked the claimant to sign an agreement to participate. Each claimant who agreed to participate and had a valid UI claim was enrolled in the experiment and sent a packet of materials. After this point, the process for the two treatments differed somewhat.

For the JSIE, the packet included a "Notice of Hire" form that the claimant signed and returned to the Department of Employment Security (DES) upon obtaining full-time employment within the 11-week period. The DES verified that benefit payments had stopped within the designated period, and sent the participating claimant a voucher, which the claimant submitted for payment of \$500 after completing 4 months of continuous employment. Upon receipt of the voucher, the DES verified that UI benefit payments had not been resumed within the 4-month period, and issued a check for \$500.

For the HIE, the packet included multiple copies of a letter to employers explaining the bonus offer, which the participating claimant could use in his or her job search. Upon becoming employed, the claimant gave the employer the "Notice of Hire" form, which the employer and the claimant signed. The employer sent the Notice of Hire to the DES, which verified that the participating claimant was not receiving UI benefits and sent a voucher to the employer. The voucher was signed by both the employer and the claimant at the end of the 4-month employment period, and submitted to the DES for payment. Upon verification by the DES, a check for \$500 was issued to the employer.

Approximately 12,000 of the originally assigned group of 17,000 claimants were determined to be eligible to participate in the experiment. Table 5-1 shows that 84 percent of those eligible to participate in the JSIE signed the agreement to participate, but only 65 percent of those eligible for the HIE agreed to participate. As indicated by the low proportion of participants submitting Notices of Hire or receiving bonuses, and as further shown by the results of a Follow-Up survey of 2,000 experimental claimants, the "real" participation rates, especially in the HIE, were considerably lower. The limited use of the HIE suggests that this experiment had little scope for reducing UI benefit payments or weeks of unemployment.

The effects of the experimental treatments were determined by examining the benefits paid to claimants over a full benefit year, and the number of weeks of insured unemployment experienced over a full benefit year. A full benefit year (rather than just the spell of unemployment immediately following the initial

claim) is required to determine the true effects of the treatments, because claimants could have redistributed their unemployment experience over a benefit year, with unemployment dropping immediately following the initial claim, but increasing later on.

The JSIE had a large and statistically significant effect on the benefit receipts and weeks of insured unemployment of eligible claimants. Claimants eligible for the JSIE received \$158 less in state regular benefits over the full benefit year than did members of the control group. Also, JSIE eligibles experienced 1.15 fewer weeks of insured unemployment over the benefit year than did controls. Both the \$158 benefit reduction and the 1.15-week unemployment reduction were achieved on average over the full group of claimants eligible to participate in the JSIE. Finally, the JSIE reduced the probability that a claimant would exhaust his or her benefits by 3.2 percent. These results are displayed in Table 5-4. Computation of the net benefits of the JSIE shows that for every \$1 spent on bonuses, expenditures from the UI Trust Fund were reduced by \$2.30.

The results of the HIE are more complex. The HIE clearly reduced benefit payments and weeks of insured unemployment in the spell of unemployment immediately following the initial claim for all eligibles. This in itself is remarkable, given the small number of claimants who participated in the experiment. But, over the full benefit year, the HIE had no statistically significant effect on the benefits paid to HIE enrollees taken as a whole, or on the number of weeks of insured unemployment they experienced.

An important question to be addressed is whether the reduced weeks of unemployment induced by the JSIE were purchased at the expense of reduced effective job search time and acceptance of a less satisfactory job. We have tested this hypothesis by examining the post-experimental earnings of claimants who found employment. Table 5-5 demonstrates that there was no earnings loss for JSIE eligibles in the first full quarter of earnings after benefit termination. (The first post-claim quarter is ignored, because it may be contaminated by the effects of the experiment on timing of job acquisition.) Both before and after the experiment, earnings of the controls and JSIE eligibles who obtained post-claim employment were virtually identical.

We have also examined the effects of the treatments broken down by sex, age, race, occupation, industry, and benefit payment levels. The disaggregations by sex and race expose some important results not evident from the aggregate analysis. As shown in Table 6-8, the HIE had large and statistically significant effects on benefits paid and weeks unemployed of white women, but had no effect on men or black women. The reasons for this strong sex-race differentiation of the results are not totally clear, but investigation of participation in the HIE indicates clearly that whites were more prone than blacks to participate in the experiment, as shown in Table 7-2. We speculate that white women were affected by the HIE, whereas white men were not, because the

jobs white women obtain tend to involve less on-the-job training. If so, then the \$500 hiring bonus would offset a larger proportion of the training costs incurred by an employer who hired a woman.

We conclude that the JSIE demonstrates that bonus payments to UI claimants are a remarkably efficient means of reducing UI benefit payments and insured unemployment, and at a minimum should be further tested. Additional experiments with the JSIE should provide variation in bonus levels and weeks of elapsed time over which the voucher is valid.

The conclusions on the HIE are less clear. The fact that it was effective for one group--white women--indicates the potential efficacy of a voucher program paying benefits to employers. However, the participation rate in the experiment was so low that no conclusion can be drawn as to how effective a program would be, if modelled on the experiment.

Chapter 1

INTRODUCTION

In January 1984, the Illinois Department of Employment Security (then called the Bureau of Employment Security) decided to launch a demonstration of a new program aimed at reducing the costs of the Unemployment Insurance program. This experiment was funded under the Wagner-Peyser Act, which allows states to use funds "for exemplary models for the delivery of services." Illinois Governor James R. Thompson reviewed this proposed project and approved use of his ten percent (10%) discretionary money under Wagner-Peyser 7(b). The availability of these funds was crucial because the states are prohibited from using unemployment insurance funds for research projects involving Job Service programs even if the end results would be to have a positive effect on these funds. The Department contacted the W. E. Upjohn Institute about the Institute's interest in assisting the Department in conducting the demonstration. In February, the Institute submitted a proposal to assist the Department in the design, implementation and evaluation of what was initially called the Demonstration Claimant Placement Program. This initial proposal was formally resubmitted in March in response to an RFP issued by the Department. The contract was awarded to the Institute, and work on the design of the demonstration commenced in May of 1984.

As initially proposed, the demonstration was to consist of a single treatment in which employers would be the recipient of a bonus for hiring UI claimants under specified conditions. The experiment was to be conducted in two or three offices in the Chicago area. To evaluate the demonstration, the unemployment experiences of claimants in the designated offices were to be compared with those of selected claimants in comparison sites.

Initially, use of a true experimental design, using random assignment to treatment or control status, was rejected. Although we recognized that only random assignment guarantees matched control and experimental samples, random assignment was rejected because of concern about the possibility of displacement. Displacement would occur if, because of the bonus offer, employment of claimants in the demonstration is increased at the expense of claimants in the control group. If displacement does occur to any substantial degree, measures of the effects of the demonstration would have an upward bias.

In May, work on project design began, and several important changes were made and incorporated in a revised contract between the Department and the Institute. There were three crucial changes: (1) a second experimental treatment in which a bonus would be paid to a claimant, rather than to an employer, was added; (2) the experiment was to be conducted in 22 offices, rather than the two or three originally proposed; and (3) a true

experimental design was to be used, incorporating random assignment of equal proportions of eligible claimants to a control group and to each of the two treatments.

The second experimental treatment was introduced in order to compare the benefits of offering bonuses to employers (thus affecting the demand for labor) with the benefits of offering them to claimants (and thus affecting the supply of labor). The employer experiment would permit us to determine if a bonus, by decreasing hiring costs and raising the costs of further search for suitable employees, would increase the likelihood of a job seeker obtaining employment. The claimant experiment would determine if a bonus would encourage more intense job search in the period of eligibility, or would encourage more rapid acceptance of job offers. The relative benefits to the UI trust fund of paying bonuses to employers versus paying bonuses to claimants would be determined.

The number of offices in which to conduct the experiment was increased from two or three to 22 for three reasons. First, estimates of the likely flow of new UI claimants demonstrated that the desired sample size would take too long to attain if enrollment were confined to only two or three offices. Second, using a large number of offices assured that the results would not be unduly influenced by the particular characteristics of any one office. Third, spreading the bonus-carrying claimants over many labor markets made it less likely that the job opportunities of members of the control group would be affected by the experiment. With 22 experimental sites, bonus-carrying job seekers could fill at most 3 percent of the job vacancies in the relevant labor markets, making it unlikely that the bonus offer would operate to reduce the employment opportunities of members of the control group, who could fill no more than 1.5 percent of job openings.

The remainder of the summer was spent designing in detail the two experiments (see Chapter 2), developing the operating procedures and forms that would be used in the experiments (see Chapter 3), and selecting the specific sites. Once the design was approved by the Department, staff members of the Institute made a presentation to the DES regional managers at their annual meeting in Belleville, Illinois on July 18, 1984. At that meeting we explained the experiment, and solicited their support. All the regional managers gave their support to the experiment and agreed to request that the office managers in the 22 designated Job Service offices assign appropriate agency personnel to carry out the enrollment procedures. Before the experiment began in late July, the Institute and DES staff conducted a full day training session in Chicago for the local office personnel who had been assigned by the office managers in the 22 designated Job Service offices to carry out the experiment. On July 29th, the first enrollments were conducted.

The remainder of this report describes in detail the design, program operations, and results of the experiments. Chapter 2 presents the design of the experiment. The design encompasses the structure of the two experimental treatments, the determination of eligibility, sample design, and operational design. Chapter 3 describes the operations, including the enrollment procedures, descriptions of the Job Service offices in which the experiments were conducted, and the role of the various agencies in the operations.

Chapter 4 presents a description of the data used to evaluate the experiments. It describes both the administrative and special experimental data bases to which we had access, and discusses the procedures used to construct the analytic data bases. It also includes a brief discussion of the limitations of the data we have relied on.

Chapter 5 presents the main experimental results. It starts with a statistical profile of the claimants who were enrolled in the experiments, and goes on to discuss the effects of the two experimental programs on benefits received by claimants, weeks of insured unemployment experienced by claimants, and post-experimental earnings. The chapter concludes with a discussion of the reliability of these results. Chapter 6 explores the effects of the two treatments on various subgroups of the claimant population. Experimental results disaggregated by age, race, sex, labor market experience in the base period, and industry of reemployment, among others, are reported.

Chapter 7 presents an analysis of program participation. This chapter analyzes the results of the follow-up survey of 2,000 randomly selected claimants offered enrollment in either of the two treatments. Chapter 8, the concluding chapter, summarizes the results and discusses the implications of the experimental results for the operations and outcomes of actual programs that would be modelled on the experimental treatments.

Chapter 2

EXPERIMENTAL DESIGN

The employer-bonus experiment was given the title "Hiring Incentive Experiment" (HIE), and the claimant-bonus experiment was titled the "Job Search Incentive Experiment" (JSIE). The motivation for the HIE was to test the hypothesis that the offer of a bonus would encourage employers to hire eligible UI claimants more rapidly. The motivation for the JSIE was to test the hypothesis that a bonus offer to UI claimants would alter their job search behavior and lead to their more rapid reemployment. Together, the two experiments permit simultaneous testing of the effects of the bonus offer on the demand for labor and the supply of labor.

I. Eligibility for the Experiment

In order to be eligible to participate in either the HIE or the JSIE, an individual had to meet the following criteria:

- (1) File an initial claim for UI between July 29, 1984 and November 17, 1984;
- (2) Have a valid UI claim, that is, a claim that would result in 26 weeks of benefit payments;
- (3) Register for job search in one of 22 designated Job Service offices and not be in one of the following excluded groups: on layoff with a definite recall date within four weeks, recently separated veteran (UCX), federal employee (UCFE), and or a member of a registered union who obtains employment through a hiring hall), and
- (4) be at least 20, but less than 55, years of age.

Since the experiment was operated by the Job Service, a de facto requirement for eligibility is that the claimant actually register with the Job Service. The main goal of the experimental program is to reduce the costs of the UI program, it therefore follows that only claimants eligible to receive UI benefits should be eligible to participate in the experiment. Further, eligibility was limited to those filing new claims in order to increase the degree of homogeneity of the participants. Those filing additional, transitional or reopened claims could be eligible for different number of weeks ranging from 1 to 26, whereas new claimants all have 26 weeks of eligibility. Aside from the bonus offer, job search behavior can be expected to differ among individuals as a function of the remaining period of UI eligibility.

Making registration with the Job Service a condition for eligibility to participate in the experiment, meets several objectives. First, it serves to reinforce the general provision that UI claimants, not in special categories, must register with the Job Service. At present, not all UI offices rigorously enforce this requirement.

Secondly, special procedures had to be developed in order to carry out the experiment. Most important was the need to carefully explain the nature of the experiment to prospective participants, and convince them of its value and authenticity. Eligible UI claimants had no other source of information about the experiment. This strongly suggested that the persons conveying the information about the experiment be trained and experienced in communicating with claimants. Job Service counselors were viewed as the best qualified to carry out this mission.

The third reason for requiring Job Service registration was to enhance the homogeneity of the group included in the experiment. Each of the UI beneficiary groups not required to register with the Job Service have special characteristics: Temporarily laid-off workers with known recall dates are clearly not seeking other employment; recent veterans have legal provisions that assist them in special ways, such as veteran hiring preferences; persons working through union hiring halls have specially targeted job opportunities and are unlikely to be influenced by a small bonus; and government employees have special channels for obtaining new positions and have particular retirement provisions that are likely to make their behavior different from others. Thus, excluding these groups from the experiment increases the homogeneity of the eligible population. Most important, the eliminated groups are those with less incentive to search for work, and are less likely to have their job search behavior affected by the bonus offer.

Age constraints also were imposed to create a more homogeneous population. Youth under 20 were eliminated, because there are several support programs, such as the Targeted Jobs Tax Credit that provide special employment inducements for youth. Thus, we believe that this age group could not be combined with the older group without special consideration for these other programs. It was easier to simply eliminate them from the sample. Those over 54 were eliminated, because this group is nearing retirement, and indeed may already be eligible for retirement benefits under some plan. Job search is clearly affected by retirement decisions and by pension eligibility for this age group. Thus, we believe that this group also could not be merged with younger age groups without special consideration of their pension eligibility, information not available to those conducting the research.

II. Treatment Design

The two experiments had very similar designs, except that the bonus recipient differed between the two. In the HIE, an employer was eligible to receive a \$500 cash payment by hiring a worker who:

- was an initial claimant for UI benefits, who was eligible to receive benefits, and had agreed to participate in the experiment;
- was hired before receiving 11 weeks of benefits;
- remained employed continuously for 4 months; and
- worked on the job for 30 or more hours per week.

In the JSIE, a UI claimant meeting the same four criteria was eligible to receive a \$500 cash payment.

Thus, the conditions for payment are the same in both experiments, differing only in that the payment is made to the employer in the HIE and to the claimant in the JSIE. The next chapter describes how these two experiments are operationalized. In this chapter, we will describe the rationale behind the design.

Although the amount of the bonus of \$500 was arbitrarily selected, it reflected both the total financial constraint under which the project was operating, and the desire to have a bonus of sufficient magnitude to have some reasonable expectation that it will generate a response. \$500 represented about 4 weeks of UI benefits payments.

The period of insured unemployment over which the bonus offer was valid was arbitrarily selected to be 11 weeks from the week in which the claimant filed for benefits. However, this time period could have been delineated in one of two ways: (1) elapsed calendar time of 11 weeks, or (2) a period of time necessary to acquire 10 weeks of benefits (plus a waiting week). The two will differ if there are partial payments due to part-time employment, or if there is an interruption in the benefit flow. Although in theory the second definition creates a stable, and therefore more predictable, ratio of the bonus to the total available benefits, it was judged that the requirement to end the experiment at a particular point of calendar time required that the first definition be used.

The second time-related condition that must be fulfilled before a bonus would be paid either to the employer or the claimant is that the claimant must be retained in the position for which he/she was hired (or an equivalent or better position) for a period of four months. The four-month waiting period is required to avoid the possibility of a fraudulent hire, undertaken solely to obtain the bonus without the claimant intending to remain

working or the employer intending to retain the worker. The four-month waiting period was also regarded as the shortest possible period that would avoid payment of a bonus for strictly seasonal hiring.

The last condition is that the claimant work for at least 30 hours per week. This implies that the job at least approaches a regular full-time position, and will result in termination of benefit payments.

III. Sample Design

Three issues are addressed under sample design. The process of selecting members of the sample, which in this case is by random assignment, the decisions regarding the size of the sample, and the process of selecting sites (i.e., Job Service offices) in which to conduct the experiments.

A. Randomization

Randomization is a process of blind selection of a sample from a population. The key point in randomization is that each member of the population has an equal chance of being selected for the sample. In a social experiment, randomization is accomplished by assigning each member of the population of persons eligible to take part in the experiment a unique number, and then establishing a system whereby each number has the same chance as any other number of being selected for inclusion in the experiment. If the experiment is comprised of more than one treatment, or has a control group, then the process of random selection assures that each member of the eligible population has an equal chance of being assigned to each of the treatments or to the control group.

In the case of the Illinois UI experiment, randomization was accomplished by using the last two digits of each individual's Social Security number to assign eligible UI claimants to one of the two "treatments" and to the control group. It is well known that the last two digits of an individual's Social Security number is a random number. The last two digits of the Social Security number were used for assignment in the following way:

If the last two digits were 00 to 33 the claimant was assigned to the control group and received no treatment;

If the last two digits were 34 to 66, the claimant was assigned to the HIE;

If the last two digits were 67 to 99, the claimant was assigned to the JSIE.

This procedure assured that, on the average, the characteristics of individuals in each of the three groups would be the same, thereby making it likely that in a sample as large as that used in

the Illinois experiment, any systematic differences in behavior of the members of the three groups would be due to the experimental treatment.

B. Determination of Sample Size

Determination of sample size started from the premise that the State of Illinois had \$750,000 available for payment of bonuses, and that we were to select a sample that would result in bonus payments totalling this amount. This task required determination of the number of claimants to be enrolled that would likely result in the cashing of 1,500 vouchers, which would just exhaust the \$750,000 budget (1,500 vouchers x \$500 = \$750,000). Determining the number of vouchers that would be cashed required determination of the proportion of claimants filing initial claims who would be found eligible for UI benefits, agree to participate in the experiment, find a job within 11 weeks of filing, and retain that job for 4 months.

This algorithm can be expressed as a simple algebraic formula:

- (1) No. of UI claimants offered participation in the experiment, times
- (2) the proportion of (1) that are found eligible to receive UI benefits, times
- (3) the proportion of (2) that agrees to participate in the experiment, times
- (4) the proportion of (3) that obtains a job within 11 weeks of filing, times
- (5) the proportion of (4) that retains the job for 4 months, equals
- (6) 1,500

This equation was used to determine the number of eligible UI claimants to enroll in the experiment. Of course, the proportions were only best guesses based on historical experience, and thus the number of UI claimants to enroll was predicted with error. An important consideration in designing the sampling procedure was that error in estimating the proportion could lead to actual payment greater than or less than the budgeted \$750,000. Although the state agreed to accept some overrun (because other funds could be tapped if the budgeted amount was exceeded by a small amount), the Institute was admonished to make every effort to devise a system with as low a chance as possible of overrunning the budget, without jeopardizing the integrity of the experiment. Of course, one option was to enroll that number of claimants presenting no chance of overrunning the budget, i.e., 1,500. But this small sample would be unlikely to lead to reliable estimations of experimental effects, as will be demonstrated below.

In fact, the whole exercise was academic as the real problem turned out to be enrolling a large enough number of claimants to

exhaust the budget. The reasons for the large overestimation of the number of eligibles who would claim bonuses are discussed in Chapter 7.

The procedure followed to calculate the desired sample is presented in logical sequence, even though the actual decision process is reversed. The data provided by the Illinois Department of Employment Security used to determine the enrollment sample are presented in Table 2-1. In the period for which the data were available, July-September 1983, the 19 UI offices had 11,183 new valid claims processed. It was estimated that statewide, initial claims in June of 1984 were running 65 to 75 percent of the level a year earlier.¹ Thus, we assumed that in the enrollment period, claims would be about 70 percent of the level in the summer of 1983, leading us to expect that the 19 offices would generate a flow of 7,828 claims in the three months starting August 1984. Calculations of expected participation are shown in Table 2-2.

Two-thirds of these claimants would be offered an experimental treatment, split evenly between HIE and JSIE. On the basis of past experience with enrolling subjects in similar experiments, a refusal rate of 10 percent could be expected. Refusal occurs because of the inability to communicate properly, or because of innate suspicion that there is "no free lunch." Thus, we expected that 4,720 claimants would be offered and would accept enrollment into the two experiments.

The next step was to estimate the expected percentage of enrollees that would receive a bonus. This involved three steps: (1) determining the percent of claimants that would normally be expected to obtain a full-time job within 11 weeks of filing a claim; (2) determining the percent of those in (1) that could normally be expected to retain that job for four months; and (3) estimating the likely effect of the experiment on (1) and (2). As shown in Table 2-1, in the observation period, 3,540 of the 11,183 new claimants terminated before receiving 10 weeks of benefits. (Since there is a waiting week in Illinois, this represents 11 weeks from date of filing.) This number is greater than the number of claimants obtaining full-time employment, because benefits may also be terminated because of withdrawal from the labor force. However, we had no readily available information on these reasons, and therefore took the conservative step of assuming that all terminations were for full-time employment. The proportion of claimants who terminated benefits within 11 weeks in the 1983 period (i.e., .317) was multiplied by the expected number

1. Letter to Harry Hardwich, Director, Office of Economic Information and Analysis, from Erwin Cohen, Asst. Director, Office of Research and Information, dated June 15, 1984, Subject: Status of the EB Program Trigger and Status of FSC Eligibility as of claims week ending 06-09-84.

of enrollees to obtain an estimate of the expected number of experimental subjects that would become employed and become provisionally eligible for the bonus.

Estimating the expected number that would keep their jobs for four months and thus become fully eligible for the bonus was more difficult. Information in Table 2-1 on the number of those who had terminated benefits within the 11-week period and refiled for benefits within the benefit year shows that .377 of those who terminated in the initial 11-week period refiled. Some of those, however, would have been eligible for the bonus, because they would have retained their jobs for at least 4 months. On the basis of conversations with UI staff, we estimated that about half of the refilers would in fact have lost eligibility for the bonus; thereby reducing the bonus recipients to about .81 $[1-.5(.377)]$ of those provisionally eligible, resulting in an estimate of 1,212 bonus recipients.

However, this calculation does not take into account possible experimental effects, which would have induced some claimants and employers, otherwise not eligible for the bonus, to change their behavior so as to make themselves eligible. To estimate a possible experimental impact, we assumed that a portion of the group receiving benefits for a period of 11 to 15 weeks would reduce the length of their unemployment spell sufficiently to qualify for the bonus. The \$500 bonus represented approximately three-and-one-third weeks of bonus payments. Assuming that terminations occur evenly throughout the 5-week period between the 11th and 15th week after filing, implies that two-thirds of those terminating benefits within 11 to 15 weeks after filing will respond to the experiment by reducing their unemployment spell to less than 11 weeks. In the period July-September 1983, 1,288 claimants in the 19 offices terminated benefits between the 11th and 15th week. Two-thirds of the 1,288 claimants, or 859, are assumed to respond to the experiment by reducing their period of unemployment and obtain employment within the 11-week period for bonus eligibility.

Of these, 696 (.81 percent of the responders) would be expected to remain employed for four months, and earn a bonus. This represents 6.2 percent of the 11,183 claimants. Applying this proportion to the number expected to be enrolled in the experiment, i.e., 4,720, means that an additional 294 bonus payments could be expected due to the experiment, bringing the total of bonus payments to 1,506, a number very close to the goal of 1,500. These calculations are shown in Table 2-2.

The implication of this analysis is that offering the experimental treatment to eligible claimants in the 22 Job Service offices for a period of 13 weeks, starting the first week in August 1984, should have resulted in the payment of about \$750,000 in bonuses, thereby meeting the financial constraint for the project. As we shall see, the use of the bonus was far less than predicted.

C. Site Selection

Sample selection was carried out in 22 pre-selected Job Service offices in northern and central Illinois. Several factors were considered in determining the number of offices and their location. First, the number of offices was limited by the efficient monitoring scope of the DES central office and the Upjohn Institute's research staff. For the integrity of the experiments, it was necessary that all the agency personnel conducting the enrollments at the sites were fully knowledgeable about the experiments, that operating procedures were being carried out in accordance with the agreed upon design, and that they were being carried out in the same manner in each office. If treatments differ across offices, then the average effects of the experiment might go undetected, and "office" becomes an unwanted treatment.

On the other hand, another objective dictated having as many offices as feasible. Having a large number of offices helps assure that the results are not office-specific; that is, differences in performance across offices will tend to average out if there are enough offices. For this reason, it was desirable to have a large enough set of offices to reflect the demographic and industrial mix in the state and to minimize the possibility that the results were unduly influenced by the idiosyncratic behavior on the part of one or a few offices. Another reason for selecting as many offices as feasible was the desire to limit the duration of the enrollment period.

The requirements for monitoring set limits to the number of offices that could be handled by the DES and Institute staffs. To minimize variations in treatment across offices, it was essential to conduct training sessions for all those expected to be involved in the experiment and then for the monitors to make periodic visits to the sites while they were in operation. Seeking a compromise between the administrative necessity to keep the number and locational distribution of offices as small as possible, while obtaining diversity, led to a selection of 22 Job Service offices in northern and central Illinois, including Chicago.

To select individual sites, the research team obtained from the Illinois Department of Employment Security information for each office on the following variables for the period July-September 1983: The number of new claimants, the number of such claimants who terminated benefits before receiving the 11th week of payment, the number who terminated within 11 to 15 weeks, and the number in each of these two groups that subsequently refiled for benefits within the same benefit year. The second step in the process was to use a map of Illinois to designate areas in which the sample was to be selected. The decision was made to confine the area to northern and central Illinois, with Springfield being the southernmost area included.

Excluding the southern half of the state served two purposes: (1) limited monitoring to an area that could feasibly be visited two or three times during enrollment by the research team at reasonable cost; and (2) made the sample as homogeneous as possible--the southern half of the state was heavily into agriculture and mining, whereas the northern half had most of the manufacturing employment. It was felt that mixing these two populations would increase considerably the size of the sample needed for analysis.

Twenty-two Job Service offices, serving the UI applicants from 19 UI offices, were selected to take part in the experiment. These offices were grouped into four regions:

The Chicago area, with 7 UI offices served by 8 designated Job Service offices (Bedford Park, Chicago Heights, Evergreen, Prof/Cler/Sales, Ravenswood, South Chicago, West Town, Woodlawn);

The Metro-Outlying area with 4 UI offices served by 4 JS offices (Aurora, Mt. Prospect, Waukegan and Villa Park);

The Central area, with 3 UI and 4 JS offices; (Danville, Kankakee, Springfield North and South);

The Northwest area with 4 UI offices served by 5 JS offices (Galesburg, Moline, Peoria, Rockford East and West).

In part, these offices were selected because they were the largest offices in their regions. Further significant increases in sample size would have required a large increase in the number of offices. Therefore, it was decided that if these 22 offices were unable to generate the desired sample size, any additional increases would be accomplished by manipulating the length of the enrollment period.

IV. Adequacy of the Sample to Measure Experimental Impact

The question arises as to whether or not the sample size will be adequate to detect an experimental impact. Based on benefit payments of about \$140 per week, it would take about 3-1/3 weeks of foregone benefits to equal the \$500 bonus. Using this measure, we determined (calculation above) that the experimental treatment could raise the number of bonus payments from 1,212 to 1,506, or from .256 of those enrolled to .319. The following calculation demonstrates that the expected sample is more than adequate to detect an impact of this magnitude.

On the basis of the flow analysis described above, we would expect the 22 Job Service offices to generate a flow of 7,828 new eligible claimants in a three-month period. With a 10 percent refusal rate, this will generate a potential participant flow of about 2,360 claimants into each of the two experiments and a

control group (assuming one-third into each of the three groups). Assuming that .256 of the control group would earn a bonus, if available to them, the question arises as to whether or not an observed value of .319 for each of the experimental samples would be regarded as statistically significant.

For a sample of this size, we can say with a 95 percent degree of confidence that an observed difference of only .017 between the control group and an experimental group in the proportion of claimants earning a bonus is a true difference. On the basis of .256 of the control group earning bonuses, if .274 of an experimental group collected bonuses, we could say with a 95 percent degree of confidence that there is an experimental impact, and that the observed difference is not due simply to chance.²

Since this proportion is far lower than the .319 deemed possible, we conclude that the sample size is sufficient to test the hypothesis.

The sample is also adequate to detect an impact of this size on several subgroups of the population. For instance, if we subdivide the population into eight subgroups: two races (white and non-white), two sexes, and two occupations (blue collar and white collar), we would need to detect a change in the probability of collecting a bonus for a group about one-eighth the size of the total population. The upper limit of the 95 percent confidence interval for one-eighth the total group, assuming again that .256 of the control group collects a bonus, is .307 implying the ability to detect an impact on an experimental group with a bonus collection rate of .319.³

Thus, we entered the experiment with reasonable expectations that we would be able to detect an impact, if present.

2. Calculated as follows:

let p = the probability of collecting the bonus
 let q = the probability of not collecting the bonus
 let n = the number enrolled in each experiment and in the control group
 let s = the standard deviation of a proportion,
 then $p + 2s$ is the upper limit of 95 percent confidence range (using a two-tail test) for measurement of experimental impact, and
 $s = \sqrt{pq/n}$, thus,
 $s = \sqrt{.256 \times .744/2360} = .009$, and
 $p + 2s = .256 + 2 \times .009 = .274$

3. For $n = 2360$, $1/8$ th $n = 295$

$s = \sqrt{.256 \times .744/295} = .0254$
 $p + 2s = .256 + 2 \times .0254 = .307$

TABLE 2-1

Claimants, Early Selections, Refilings and Proportions of Totals
Illinois UI Program, July-September 1983

Region (1)	Total Claimants (2)	Claims Terminated 1-10 Weeks (3)	Claims Terminated 11-15 Weeks (4)	No. in Col. 3 Refiled
<u>North</u>				
Rockford (25+61)	451	174	58	90
Mt. Prospect (7)	452	102	42	45
Aurora (22)	810	309	72	114
Lombard (31)	354	105	30	58
Moline (30)	1,192	403	177	115
Waukegan (20)	761	261	78	77
<u>Central</u>				
Kankakee (24)	473	172	48	64
Peoria (33)	359	105	39	61
Springfield (41)	497	164	48	86
Galesburg (32)	542	196	61	104
Danville (37)	501	185	52	106
<u>Chicago</u>				
Chicago (10)	242	42	26	12
Chicago (11)	655	138	59	52
Chicago (12)	501	149	71	52
Cicero (17)	719	213	99	56
Chicago (71)	968	267	89	96
Chicago (5)	1,021	380	138	91
Chicago (13)	<u>685</u>	<u>175</u>	<u>101</u>	<u>54</u>
Totals	11,183	3,540	1,288	1,333

SOURCE: Illinois Department of Employment Security.

TABLE 2-2

Expected Number of Participants

1.	JS Registrants Eligible to Enroll, July-September 1983	11,183
2.	Estimated Registrants, Fall 1984 ($(1) \times .7$)	7,828
3.	No. Assigned to Experiments ($.67 \times (2)$)	5,245
4.	No. Expected to Enroll ($.9 \times (3)$)	4,720
5.	No. Expected to Obtain Job in 11 Weeks ($.317 \times (4)$)	1,496
6.	No. Expected to Retain Job for 4 Months ($.81 \times (5)$)	1,212
7.	No. Expected to Receive Bonus ($(6) + (.062 \times (4))$)	1,506

SOURCE: Table 2-1 is source for proportion used in lines 5, 6, and 7.

Chapter 3

OPERATIONS

Chapter 2 set forth the joint design of the HIE and JSIE experiments. In this chapter, we will describe how the project was implemented. The first section will present the operational design, which describes how claimants were enrolled in the experiments and tracked through the various stages in the process. This section also describes how the project was monitored for compliance to the design and what forms and instruments were used in the experiment. Following the first section are three sections that describe in more detail important aspects of the operations, namely: (1) JS office organization and procedures; (2) Operations of the central DES office in connection with the experiment; and (3) The flow of participants, the monitoring process, and the adjustments made in participant flow and timing.

I. The Operational Design

Although the experimental design is basically simple, the fact that it must be operationalized within an existing agency structure, and essentially imposed on that structure, creates considerable complexity in the operations. The operational design, described below, is comprised of five parts: first, the flow of claimants through the phases of the experiment; second, the experimental clock and responsibilities of each of the agencies involved (the experimental clock is the timing of events and actions in terms of the length of the experiment, in which time zero on the experimental clock is the week or day in which the experiment started); third, the various forms and data collection instruments; fourth, the system for monitoring participant flow, which is important because of the need to be assured that an appropriate number of eligible claimants participate in the experiment; and finally, the methods used to transmit information to the eligible claimants and prospective employers, crucial because an experiment cannot be said to have taken place if those eligible for the experiment are unaware of its existence or its rules of operation. Each of these issues will be discussed below.

A. Flow of Eligible Claimants

Figure 3-1 shows the flow of persons who file an initial claim for UI benefits and who register with the Job Service. Figure 3-1 also shows the flow of individuals who are offered the JSIE. Figure 3-2 shows the flow of individuals who are offered the HIE, which takes off from point "A" in Figure 3-1.

All monetarily eligible UI claimants who are filing an initial claim and who are in the appropriate age bracket and who register with the Job Service are eligible to be offered an opportunity to enroll in the experiment. Eligible claimants are asked to complete a baseline survey and then are assigned to an experimental or control group on the basis of the last two digits of their Social Security number, as shown in Figure 3-1.

The JS counselor who has been assigned as specialist for the experiment proceeds to inform the eligible claimant as to the nature and purpose of the experiment, and then asks the claimant if he/she would sign an "Agreement to Participate" in the experiment. The survey and Social Security number are retained for all eligible claimants, including those who refuse to participate in the experiment. Those who do agree to participate, sign the agreement.

The central office of the Department of Employment Security in Chicago is informed of the Social Security number of all those claimants who agree to participate in the experiment, and a determination is made as to whether or not the individual is eligible for UI benefits. Once eligibility is established, the individual is sent an instruction sheet and multiple copies of the "Notice of Hire" form.

If the individual is not hired within 11 weeks of the time of filing a claim, the Notice of Hire expires, and the individual's eligibility for the bonus ends. In the JSIE, if the individual does obtain a full-time job within the specified time limit, then the four-month clock starts and the participant sends an employer-validated copy of the Notice of Hire to the Department of Employment Security (DES) office in Chicago. DES sends a letter of acknowledgment with a voucher form back to the participant. At the end of four months, if the claimant is still on a full-time job that he/she had obtained within 11 weeks of filing, then the voucher is submitted to the DES. After the DES verifies that the participant has not been receiving UI benefits for the four-month period, the bonus payment is made.

The process for the HIE is the same as for the JSIE to point "A" in Figure 3-1. Figure 3-2 shows the participant flow for the HIE after the point of assignment. The JS specialist for the experiment informs the eligible claimant of the experiment and obtains his/her agreement to participate. The enrollment is processed at the DES in the same manner as the JSIE enrollment, with enrollment depending ultimately upon eligibility for UI benefits, as well as the other conditions of the experiment. The forms sent by the DES to the newly enrolled participant differ because of the differences in the experiments. In the HIE, copies of a letter informing prospective employers of their potential eligibility to receive the \$500 bonus are included in the packet. The participant is expected to give copies of this letter to prospective employers so that they can be informed about the experiment. As with the JSIE, the voucher expires if the participant has not been hired within 11 weeks of filing for benefits.

In the HIE, the claimant who agrees to participate in the experiment, and is declared eligible, informs prospective employers of the bonus. If the claimant is hired, he/she provides the employer with a copy of the Notice of Hire, which the employer then submits to the central office of the DES. If the claimant has met all the conditions for eligibility, then the employer is sent an acknowledgment and a voucher form that the employer will use to claim the bonus at the end of the four-month waiting period. If the claimant remains employed for that period, then the employer submits the voucher for payment. As noted in Figure 3-2, an individual claimant could obtain one job within the 11-week filing period, lose that job and obtain another before the 11 weeks have elapsed. In this case, the second employer becomes the one eligible to receive the bonus. As with the JSIE form, once the four months have elapsed, the employer submits the voucher, which is validated by the DES, and a payment is made to the employer. The validation process is a determination that the claimant had stopped filing for benefits within the 11-week period, and not resumed filing before the end of four months from the date filing had stopped.

B. Timing of Events and Responsibilities

Figure 3-3 captures the time sequence of events and shows the allocation of programmatic responsibilities among the four participating groups; namely, the participating UI claimant, the participant's employer, the local Job Service, and the Department of Employment Security (DES).

Figure 3-3 shows that the process starts at time zero when an unemployed individual files a UI claim. Most monetarily eligible claimants who are filing initial claims must register with the Job Service. Many offices are joint UI/JS offices, and the claimant will simply walk to a second desk to register with the Job Service. Other offices are separated, and it may be a few days (but legally not more than two weeks) before the claimant registers with the Job Service. By the end of the second week after filing, the claimant has registered with the Job Service and has seen a JS counselor, who has been assigned to carry out the experimental treatments. If the claimant is in the control group, then he/she is likely to have been asked to fill out the baseline survey by the JS receptionist. Otherwise, all claimants see the specialist, who makes the assignment to experimental treatment or control status.

The JS agent enrolls the claimant into the experiment and records the enrollment in a log, which is sent weekly to the project office at the central office of the DES in Chicago. Sometime before the end of the fourth week, the DES will have verified the enrollees eligibility for the bonus and will mail the appropriate forms to the newly enrolled participant. This process takes two to three weeks, because of the time necessary for the relevant information about UI eligibility to enter the agency data base (called the BIS file).

At this point, the two experiments part company. For the JSIE, upon verification that the participant is an initial claimant eligible for UI benefits, the DES officer sends a copy of the Notice of Hire, which will be filled out by the claimant and returned to the central office of DES if the claimant obtains full-time employment within 11 weeks of filing for the claim. The claimant is asked to obtain verification of employment by the employer.¹

Under both the JSIE and the HIE, the role of the Job Service is confined to the initial enrollment in the experiment. After that, participants or employers communicate directly with personnel in the central office of the DES. Under the HIE, the DES agent verifies UI eligibility, sends packets of letters of introduction to the participant that he can use to advertise the bonus to prospective employers, and Notice of Hire forms. Upon receipt of a completed Notice of Hire, the DES agent sends a voucher to the employer. Upon receipt of the signed voucher, marking the end of four months of employment, the DES agent verifies that no UI claim was filed in the interim, and then generates the internal paper work necessary to have DES pay the \$500 bonus to the employer.

Under JSIE, the roles of the Job Service and DES are much the same as under HIE, with regard to validation of eligibility for participation in the experiment, for forwarding vouchers and for making payments. The difference occurs because the payment will go directly to the participant and no communication with the employer is needed.

The role of the participant differs in the two experiments markedly. In the HIE, the participant must use the bonus offer to induce prospective employers to offer a job. Thus, the claimant must be able to articulate the nature and rules of the experiment to employers. In other words, utilization of the HIE bonus depends upon actions of employers as well as participants. Once a participant has obtained a job and turned over a Notice of Hire, his/her role in the experiment is essentially ended, except to verify that he has remained on the job for the four months when he is approached by the employer to countersign the payment voucher.

In the JSIE, the role of the participant is more direct and simple. He/she need only communicate with the employer to obtain a signature on the Notice of Hire in order to verify employment,

1. It should be noted that the duration of job search before the voucher expires will vary from 9 to 11 weeks, depending upon how soon after filing the UI claim the applicant registers with the Job Service. Since the participant has 11 weeks to establish eligibility for the bonus and must remain employed for four months, the total experimental clock can run from 17 weeks for an individual who finds a full-time job almost immediately, to 28 weeks for an individual who obtains a job in the 11th week.

and again on the voucher to verify continued employment for the four-month period. It is the obligation of the participant in JSIE to submit the Notice of Hire to the DES when becoming employed within the 11-week period, and to submit the voucher for payment at the end of 4 months. Payment of the bonus is made directly to the participant by the DES. The employer is not involved in the transaction.

C. Forms and Instruments

Appendix A3 contains copies of all the forms and instruments used in the experiment. The title, the general content and agent responsible for each form and survey instrument is listed below for each experiment in the proper experimental time sequence.

Base Line Survey. Administered by a Job Service intake person, and filled out by UI claimants who are eligible to participate in the experiment. This survey provides information about the structure of the individual's household, other household income, ownership of residence, and health condition.

Information Sheet. These single-page sheets describe the experiment and the conditions that must be fulfilled for the claimant (JSIE) or the employer (HIE) to receive the \$500 bonus. For the HIE, the information sheet also informs participating claimants how to approach prospective employers.

Agreement to Participate. Administered by a Job Service specialist assigned to the experiment and signed by the eligible claimant. It is a statement of agreement to participate and an acknowledgment that the claimant has read a one-page description of the experiment to which the claimant has been assigned.

Letter of Introduction. For the HIE only, this letter introduces the participant to prospective employers, explains the experiment, and what the employer must do to obtain payment of the bonus. It is signed by the Field Operations Coordinator (FOC) of the Illinois Department of Employment Security (DES).

Letter of Participation. After determining that the claimant is eligible to receive UI benefits and meets the other criteria for eligibility for the experiment, the FOC/DES sends a letter informing each applicant as to his/her eligibility to participate in the experiment.

Notice of Hire. This is the formal document that notifies the DES that the participant has been hired on a job for 30 hours per week or more. If the date of hire is within 11 weeks of the initial claim, and is confirmed by a break in payments of UI benefits, the FOC, DES sends a voucher form to the participant (JSIE) or to the employer (HIE). For the JSIE, the Notice of Hire provides the name and Social Security number of the participant, the name of the company that hired him/her, and the signature of the participant. The notice is to be certified by the employer (although this was not made a requirement for payment of the

bonus, since there were other means for verifying employment). The Notice of Hire for the HIE was similar, except that it is submitted and signed by the employer, and certified by the participant.

The Voucher. The voucher is the form submitted to claim the bonus. For the JSIE, it is signed and submitted by the participant, and certified by the employer. For the HIE, it is signed and submitted by the employer, and certified by the participant. It states that the participant has been continuously employed by the named company on a job of 30 hours per week or more for at least 4 months. The voucher is received by the FOC, DES, who verifies that UI payments did not resume within four months of the date of hire. Upon verification, the FOC, DES authorizes payment of the bonus.

D. Monitoring the Experiment

In any social experiment, there is a need to assure conformity to the experimental design in order to be certain that the results obtained can be attributed to the experimental treatment. If the experiment is not carried out according to the design, or worse, if the actual treatment administered is unknown, then the results cannot be interpreted as causally related to the experiment. Several means were utilized in this experiment to either (1) assure that the experimental design was carried out, or (2) to document the deviations from the design. These means are described below.

1. The Upjohn Institute staff provided in-service training to designated JS specialists from each of the offices in which the experiment was to be administered. Two specialists had been assigned from each of the 22 JS offices in the experiment. These assignments had been made by the office managers under the direction of their regional directors, and thus had the official support of the operating agency. The training was conducted in a joint all-day session at the Chicago office of the DES so that all of the specialists would receive the same information at the same time.

At the training session, written instructions, prepared by the Upjohn Institute, were disseminated along with oral presentation of the material and full discussion to assure understanding and agreement. The instructions set forth the procedures for identifying and handling eligible claimants, and for processing the forms and survey instrument. The discussion disclosed some procedures that needed to be modified to permit easier administration. These were allowed if they did not jeopardize the integrity of the experiment. For example, the instructions required there be two specialists, one for HIE and one for JSIE. The staff members pointed out that with two persons assigned, there could be no guarantee that two would always be present (e.g., in the case of vacations). Thus, we allowed offices to vary this procedure, with some offices having both specialists work with both experimental groups.

2. Upjohn Institute staff made at least two trips to each of the JS offices to observe operations and to discuss operational problems with the JS staff and the office managers.

3. The FOC, DES appointed to supervise the project maintained close liaison with the local offices.

4. Each JS office kept logs of the claimants seen and the results of the interviews, especially whether or not the claimant agreed to participate in the experiment. Copies of the log were sent each week to the DES. The FOC, DES would determine if the flow rate of claimants through each office and the proportion of claimants refusing to participate were unusual. If so, after conferring with the Upjohn Institute project leader, the FOC, DES would call the JS office to determine if there was an operational problem. In one case, an unexpectedly high flow rate resulted in a decision to cut the proportion of claimants assigned to the experiments in one of the offices. In another case, an unexpectedly high refusal rate led to a visit that resulted in further training for the JS staff.

5. Another purpose of the monitoring, besides assuring experimental integrity, was to fine tune the size of the sample to get the largest possible sample without exceeding the budget. The largest sample would be the number of experimentals that would cash a total of 1,500 vouchers. However, as already noted, there were several unknowns: (1) How many claimants would be eligible for UI payments? (2) how many of those eligible for UI payments would obtain jobs within 11 weeks? (3) how many of those obtaining jobs within 11 weeks would file "Notices of Hire?", and (4) how many of those filing Notices of Hire would retain their jobs for four months and submit a voucher for payment? Making these estimates was particularly difficult in the HIE, because of the largely unknown response of employers.

Estimates of these parameters had been made in order to determine a planned sample size. However, the performance of the experiment was monitored carefully to ascertain if these assumptions were valid. It was hoped that careful monitoring would disclose any discrepancy in the size of the assumed parameters and allow the DES to stop the experiment earlier than planned if it appeared that bonus payments were going to exceed the budget. On the other hand, if the estimates of utilization were excessive, then the experiment could be extended.

To monitor the experimental flow, the FOC, DES received weekly reports from the sites, delineating the number of claimants offered enrollment in the experiment, the number who enrolled, and the number refusing enrollment. She then prepared a weekly report to the Upjohn Institute with this information plus information on the number of enrollees determined to be eligible for the experiment. The number of eligible enrollees corresponded to the number of Notice of Hire packets sent to participants (JSIE) or employers (HIE). She also provided the number of Notice of Hire forms received. By monitoring these flows and ratios and compar-

ing them with the expected numbers, the Institute staff could determine whether or not the flow of participants and the submission of Notice of Hire forms were meeting expectations.

E. Information Provided to Participants

As stated previously, no experiment can be said to have taken place if the participants were unable to respond to the treatment, because they did not understand the rules of operation of the experiment and the gains and losses they would experience as a result of their behavior. Although they do not have to understand the purposes or goals of the experiment--in fact, it is better if they don't in order to avoid responses designed to please the experimenters--they do have to be able to modify their behavior in response to the treatment. Such a response has a precondition that the participants "know what they need to know," that is, the rules of operation and the resultant effects of their behavior.

Conveying this information is also important to assure that the decision as to whether or not to participate is made with full knowledge of what is being accepted or rejected. Refusal to participate often results because people want to avoid the risk of becoming involved in something they do not understand, or are skeptical regarding the reliability of the information they receive. Americans generally believe that there is no free lunch; therefore, an apparent "gift" of \$500 for doing what they are supposed to do anyway may be greeted with skepticism. Thus, the claimant must believe in the legitimacy of the experiment and must be informed about what will not happen as well as what will happen as a result of his/her actions. For example, it is important that a claimant understand that he/she will not lose regular UI benefits by accepting the bonus, or that he/she is not required to intensify job search just by agreeing to participate in the experiment.

Most information is imparted to the potential claimant at the time he/she enrolls at the JS office by the JS interviewer assigned to the experiment. If the intake officer determines that the individual filing an initial claim is monetarily eligible for UI benefits, then he/she is asked to complete the Base Line Survey, which provides various information on the claimant, including age. If the claimant is within the appropriate age group, then the JS agent informs the claimant that he/she may be eligible to participate in the experimental program, and describes the program.

Besides the verbal explanation, the agent hands the claimant a single-page information sheet that informs the claimant of his/her eligibility to participate in the program (HIE or JSIE), and describes the conditions that must be fulfilled for the claimant (JSIE) or the employer (HIE) to receive the \$500 bonus. The instruction sheet informs the claimant that his/her UI benefits will not be affected by participation in this experimental program. The instruction sheet next explains how the program works, and for the HIE, provides an example of how the

participant may approach a prospective employer. Since use of the HIE depends upon the recipient informing employers about the availability of the bonus, it was deemed important to make the HIE participant comfortable with approaching prospective employers and telling them about the availability of the bonus. This was stressed in the oral instructions as well as the written instruction sheet. A second sheet of instructions, that provided a brief, step-by-step set of instructions to each participant, was also given to the prospective participant. Finally, the prospective participant is told to contact the FOC, DES by phoning collect, or call the JS specialist with any questions.

II. Job Service Office Organization and Procedures

The procedures for handling UI claimants who register with the JS are quite simple. Most new UI claimants, unless in an exempt category, are required to register with the JS and are required to engage in active job search. In the normal procedure, a UI claimant files his/her claim at the UI office. The claim is immediately reviewed by an intake officer, who accesses the BIS file to determine monetary eligibility, and sets an appointment for the claimant to return to the office for final processing. At the same time, the claimant is presented with a registration card that identifies the bearer as a UI claimant. Unless the card has a code that exempts the bearer from the requirement to register with the Job Service, the claimant must register with the Job Service prior to returning to the UI office for final processing.

The claimant presents the registration card to the receptionist in the JS office, and then waits to be called for an interview. The interviewer fills out the basic information sheets for the JS records, informs the claimants of his/her job search obligations and of the services available to him/her at the JS office. The interviewer at that meeting, or at any subsequent meeting, may make a job referral. In the sections that follow we will show how the experiment interacted with the normal operations of the UI and JS offices.

As previously noted, 22 JS offices in central and northern Illinois took part in the experiment. These offices were diverse in many respects. Those diversities believed important to the outcome of the experiment are described in the following subsections.

A. Office Location

Although the claimant is required to register with JS within two weeks of filing an initial claim, that is, before returning to the UI office for initial review, the elapsed time between filing the UI claim and registering with the JS will vary, at least partly as a function of the distance between the two offices. At one extreme, there are unified offices at which the receptionist serves both offices, and the claimant almost automatically moves

to the JS side of the office after filing his/her claim. At the other extreme, the offices may be many blocks apart, requiring an automobile or bus trip to move from one office to the other. As will be discussed, these differences may affect the experimental clock as well as the selection of claimants who participate in the experiment.

To show the effect of distance on the offices, the offices were classified in the following manner: (1) unified offices; i.e., offices that share the same premises and have a unified office structure that provides for a natural flow from the UI to the JS office; (2) offices in the same building, which may be on different floors, or in different parts of the building, such that there would be separate receptionists for UI and JS, and the claimant would not necessarily move directly from filing a UI claim to registering at the JS office; (3) offices in separate buildings, no more than three blocks apart, such that most claimants can be expected to walk from the UI office to the JS office; and (4) offices that are more than three blocks apart, such that a claimant would normally be expected to need a car or some other form of vehicle transportation to move from the UI to the JS office.

Moving from type (1) to type (4) offices, the average time lapse between registering at the UI and the JS offices can be expected to increase. This increase in elapsed time may have an experimental effect, because the 11-week period during which the claimant must acquire a job to qualify for the bonus starts with the date of filing the UI claim. However, enrollment in the experiment takes place at the JS office and the claimant will be unaware of the experimental program until he/she registers with the JS. Thus, claimants registering in type (4) offices are likely to have less time to act on the experimental incentive and thus are less likely to be affected by it.

The following tabulation shows the number of offices in each of the four distance categories by area:

	Total	Chicago	Metro-Out	Central	Northwest
(1) Unified	8	0	1	3	4
(2) Same bldg.	2	2	0	0	0
(3) <3 blocks	6	3	1	1	1
(4) >3 blocks	6	4	2	0	0

It can be seen that there is a strong correlation between UI/JS office relationship and area. Most of the unified offices are in the areas outside of the Chicago Metropolitan Area, while most of the widely separated offices are in the City of Chicago.

B. JSIE and HIE Office Assignment

In the experiment, the JS receptionist (also the UI receptionist in unified offices), has one or two responsibilities. In all of the experimental offices, the receptionist directs poten-

tially eligible UI claimants to the JS interviewer assigned to the experiments. In the original operational design, two JS specialists were to be assigned to the experiment, one for JSIE and one for HIE, thereby assuring that there would be an agent knowledgeable about each of the experiments and capable of answering questions about their specialty. However, to minimize the need to impose constraints on the individual offices, each office was allowed to structure its use of personnel in other ways to meet their needs. Some of the offices held to the original design, while others modified the design in two ways. In some offices both specialists handled both experimental groups, splitting them 50-50, while in other offices, one specialist handled all of the experimental claimants, with a second specialist acting as a backup for overflow, or during the absence of the first specialist.

A second operational feature that varied among the offices was the disposition of the Base Line Survey. In some offices, the survey was issued by the receptionist and filled out by the claimant while waiting to see the specialist, and then checked by the specialist. In other offices, the specialist issued the survey and helped the claimant fill it out. The following table shows the number of offices in each of several operational categories:

	<u>Offices with Two Specialists:</u> <u>Separate Handling</u> <u>of JSIE & HIE</u>	<u>Joint Handling</u> <u>of JSIE & HIE</u>	<u>Offices with One</u> <u>Specialist and</u> <u>a Backup</u>
Survey by Receptionist	2	2	1
Survey by Specialist	7 1/2	4	4 1/2

Thus, most of the surveys were issued and supervised by the specialists, but in 5 of the offices, the surveys were handled more casually. The offices were fairly evenly divided as to how the specialists dealt with the different experimental groups, with only 8 to 9 of the 22 offices sticking to the original concept in which the two specialists would each handle one of the experiments. We don't, however, have any a priori assumptions as to how these differences may have affected the experimental results.

C. Office-Specific Issues

Although the description of office operations above covers the general operations and is applicable to most offices, there were special circumstances that affected office operations, and perhaps outcomes, to varying degrees. These events, or circumstances are briefly sketched on the following pages:

1. Bedford Park. Above average number of Spanish speaking clients, and some language barriers to understanding the experiment.

2. Chicago Heights. On Mondays and Tuesdays, the specialist for the experiment was at the UI office doing enrollment; the other days of the week enrollment was at the JS office.

3. Danville. The JSIE enrollees were receiving help from the specialist to fill out Notices of Hire. In September 1984, a threatened GM strike was causing some increase in JS load, but no special activities.

4. Evergreen. September 1984 saw an influx of persons on indefinite layoff due to a potential plant closing; this led to a particularly high refusal rate. This office also experienced a batch of misassignments due to having the assigned specialist on vacation and the substitute unaware of procedures. Because of misassignments, about 50 of the 100 participants at this office were removed from the experimental data base.

5. Mt. Prospect. A well-run office, experiencing twice the anticipated caseload. In a letter dated August 23, 1984, this office was instructed to cut its caseload in half by enrolling only those whose Social Security number ended with an odd number.

6. Moline. Before the beginning of the experiment, the procedure in this unified office differed from all other offices in that the claimants registered with the JS before filing a claim for UI. However, the JS could not distinguish eligible from ineligible claimants. This procedure was scheduled to change about the time the experiment was to start at the end of July. This office also had some difficulties with Spanish language barriers.

7. Ravenswood. Very high proportion of non-English speaking clients, mostly from southeast Asia. There were some significant plant closings early in the experiment, which increased the caseload on JS interviewers and caused significant increases in the amount of time claimants had to wait for an interview. Some of the claimants who would have been eligible to participate in the experiment did not wait to see the JS specialist. This was an office in which the regular JS interviewer conducted the JS interview and then asked the client to wait to see another specialist for the experiment. This undoubtedly caused some attrition, especially among those who were not serious job seekers.

8. Rockford East. The experimental specialist was gone one day a week for a field assignment. No experimental assignments occurred that day.

9. South Chicago. Experienced significant start up problems, because of the low level of understanding on the part of one of the assigned experimental specialists, the vacation schedule of the lead specialist, and the low level of support from the office

supervisor. Also, large influx of claimants early in the experiment because of U.S. Steel shutdown.

10. Springfield South. Closing of four IGA supermarkets in late July caused influx of claimants, many seeking part-time retail work.

11. West Town. Large Hispanic population created some language problems and a high refusal rate.

12. Woodlawn. U.S. Steel shutdown caused influx early in the experiment.

III. Operations of the Illinois Department of Employment Security

The State of Illinois appointed a staff member of the Office of Field Operations, Department of Employment Security (DES), to monitor the performance of the field offices and to perform the experimental functions required of central administration. Briefly, these functions were as follows:

- *to receive a weekly log of all enrollees from each office.
- *to check the Bureau Information System (BIS) file to determine which of the enrollees were eligible to receive UI benefits, and met the other eligibility requirements of the experiment.
- *to send packets of appropriate material to each eligible participant.
- *to receive Notices of Hire from participants in the experiment (or employers).
- *to verify that participants submitting Notices of Hire (or participating employees of employers submitting notices of hire) had terminated receipt of benefits within the 11-week period.
- *to mail to eligible participants (or employers) voucher forms.
- *to receive vouchers submitted for payment.
- *to verify that participants submitting the voucher (or participating employees of employers submitting voucher) had not resumed receipt of benefit payments prior to the end of the 4-month waiting period.
- *to submit request for payment of the bonus to the accounting office.

Mary Glusak, the appointed project monitor, and at different times one or two assistants, worked full time during the 17-week operational period to perform the above listed tasks. At the same time, she monitored the flow of eligible participants and receipt of valid Notices of Hire to enable the research team to determine if the quantitative goals of the project were being met. The detailed procedures were described previously in the first section of this chapter.

For an HIE enrollee, the material would be a packet containing letters of introduction that the participant could give to each prospective employer in order to explain the program and the procedures. The packet would also include several copies of the Notices of Hire to give to employers, so that the qualifying employer could mail a completed notice to DES if the participant is hired by the date specified in the letter. Upon receipt of the "Notice of Hire" submitted by the employer, the DES agent again enters the Social Security number of the participant into the BIS system to determine if the UI payments had ceased prior to the end of the 11th week. If the termination is verified, then the DES agent mails to the employer a voucher form, and instructions as to the conditions under which the form is to be submitted to receive payment of the bonus. At the end of four months of employment, the voucher is submitted for payment by the employer, and the DES agent again checks the BIS file to ascertain that the participant had not reinstated his/her claim for benefits prior to the end of four months.

One operational requirement that did introduce some unwanted experimental variation was the time necessary for DES to verify that the individual was eligible to receive UI benefits. This determination had two components, and it was the second that created the problem. First, monetary eligibility needed to be ascertained. This occurred quite rapidly. In fact, most cases of monetary ineligibility were determined by the intake officer at the UI office. However, many of the applications for benefits were disqualified for the duration of the spell of unemployment for nonmonetary reasons, primarily voluntary quitting a job, discharge for misconduct and refusal of suitable work. The problem arose because many claims were pending for several weeks as their claims were adjudicated. These claimants were not declared eligible to participate in the incentive experiment until their claim for UI benefits were approved. If the decision went in their favor, then they were declared eligible for UI payments for the entire period; however, the clock on the 11-week period within which employment must be obtained in order to receive the bonus continued to run. The reduced time available to earn the bonus was especially a problem for HIE participants because the voucher was expected to be used as an aid in obtaining employment.

IV. Participant Flow

Concern over participant flow arose because of the need to control experimental costs. The State of Illinois had a budget of \$750,000 for bonus payments and was anxious not to exceed this limit. Although no procedure, other than a cutoff of funds when the limit was reached, could guarantee against overpayments, the Institute devised a system of monitoring participant flows that would minimize the risk. The decision was made by the State that it would not cut off funds if an overrun occurred because of the recognition that such a procedure posed extreme danger to the

integrity of the experiment. To cut off the funds without informing the participants in advance that this might take place was not politically or ethically acceptable, and to explicitly announce such a policy at the start of the experiment would change the motivation structure so that the experimental results would lack external validity, i.e., confidence could not be placed in the replicability of the results if the program were to be implemented without such a fund limitation.

The compromise procedure involved establishing a monitoring system which involved estimation of expected flow of claimants into the experiment and expected flow of completed Notices of Hire and establishing enrollment goals based on these estimates. The process was monitored on a weekly basis to determine if the enrollment rates and submittal rates of Notices of Hire were conforming to expectations. The intent was to cut off the enrollment at such a point that it appeared likely that the budget would be overrun. A detailed description of the method used to estimate the likely flow of participants and Notices of Hire is provided above in Chapter 2, section on the enrollment period.

It became clear early in the experiment that the problem we were geared to address--terminating the experiment to avoid overrunning the budget--was not the problem we faced. The actual problem was a low rate of flow of eligible claimants into the experiments and an especially low rate of inflow of Notices of Hire. The refusal rate, especially in the HIE, was very high --34 percent rather than 10 percent that we had expected. The refusal rate of 14 percent in the JSIE was closer to that expected, but the overall refusal rate was much higher than anticipated, resulting in a lower than anticipated claimant flow.

The larger problem, however, was the low rate of return of Notices of Hire. In the HIE, only 25 to 30 percent of the number of Notices of Hire that we expected to receive were received. Even for the JSIE, only 65 to 70 percent of the expected Notices were received. By the end of the 13th week, when the enrollment had been expected to terminate, only 435 Notices had been received, one-fourth of the expected number. At that point, the DES agreed to extend the enrollment period another 4 weeks, until the start of the Thanksgiving holiday.

At the final count, 964 Notices of Hire had been received and 682 bonuses paid. All of the parameters used to predict participation (shown in Table 2-2) proved to be overestimates, especially for the HIE. In the 17-week enrollment period, 8,149 eligible claimants were processed, against a projected 7,828 for the originally planned 13 weeks. However, the nominal refusal rate was 25 percent instead of 10 percent, and "passive" refusers (i.e., non-users of the bonus offer who signed agreements to participate) added considerably more to the non-participation rate. The issue of participation in the experiments was deemed so important that a special follow-up survey was undertaken, the analysis of which is presented in Chapter 7.

Figure 3-1
Participant Flow: Job Search Incentive Experiment

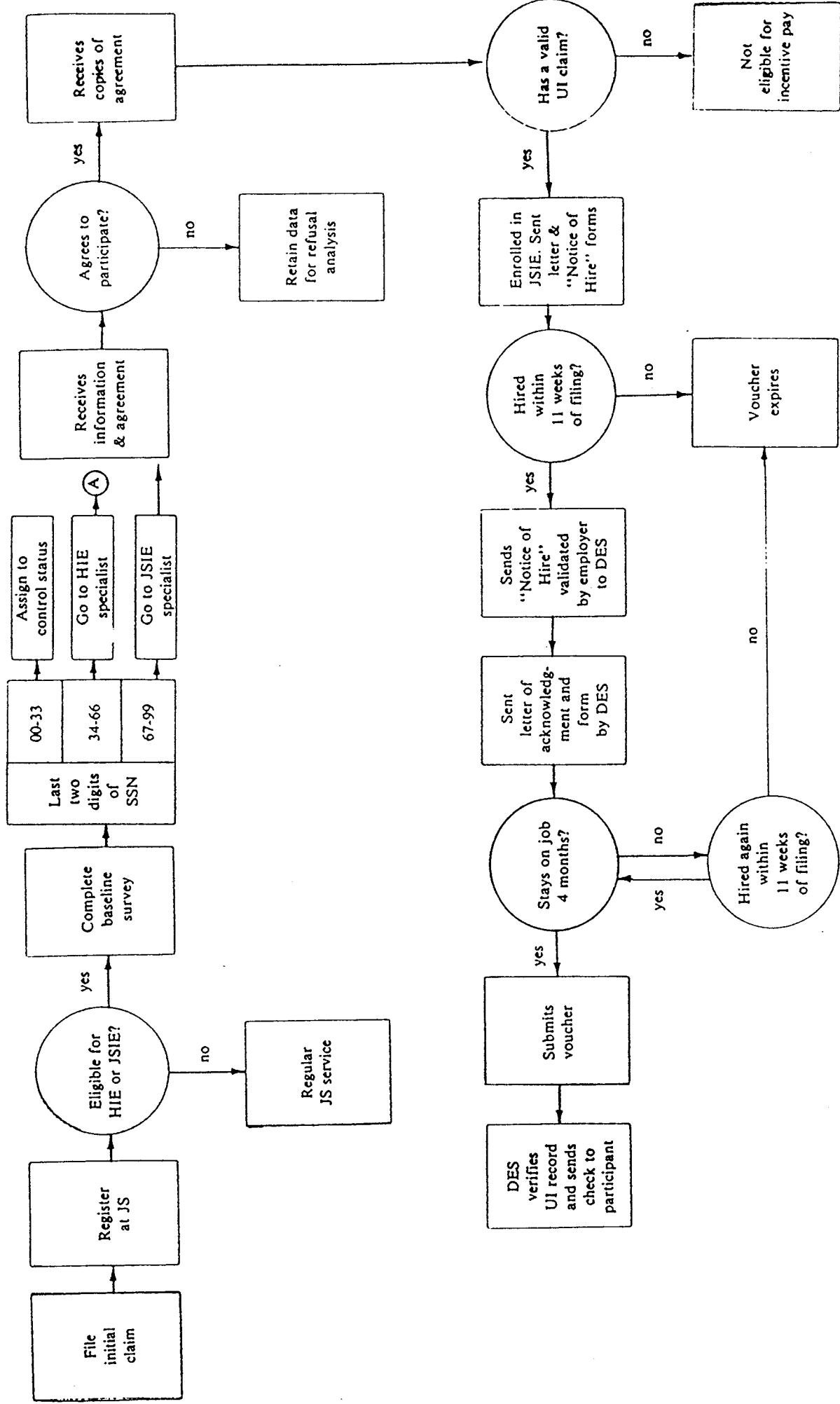


Figure 3-2
Participant Flow: Hiring Incentive Experiment

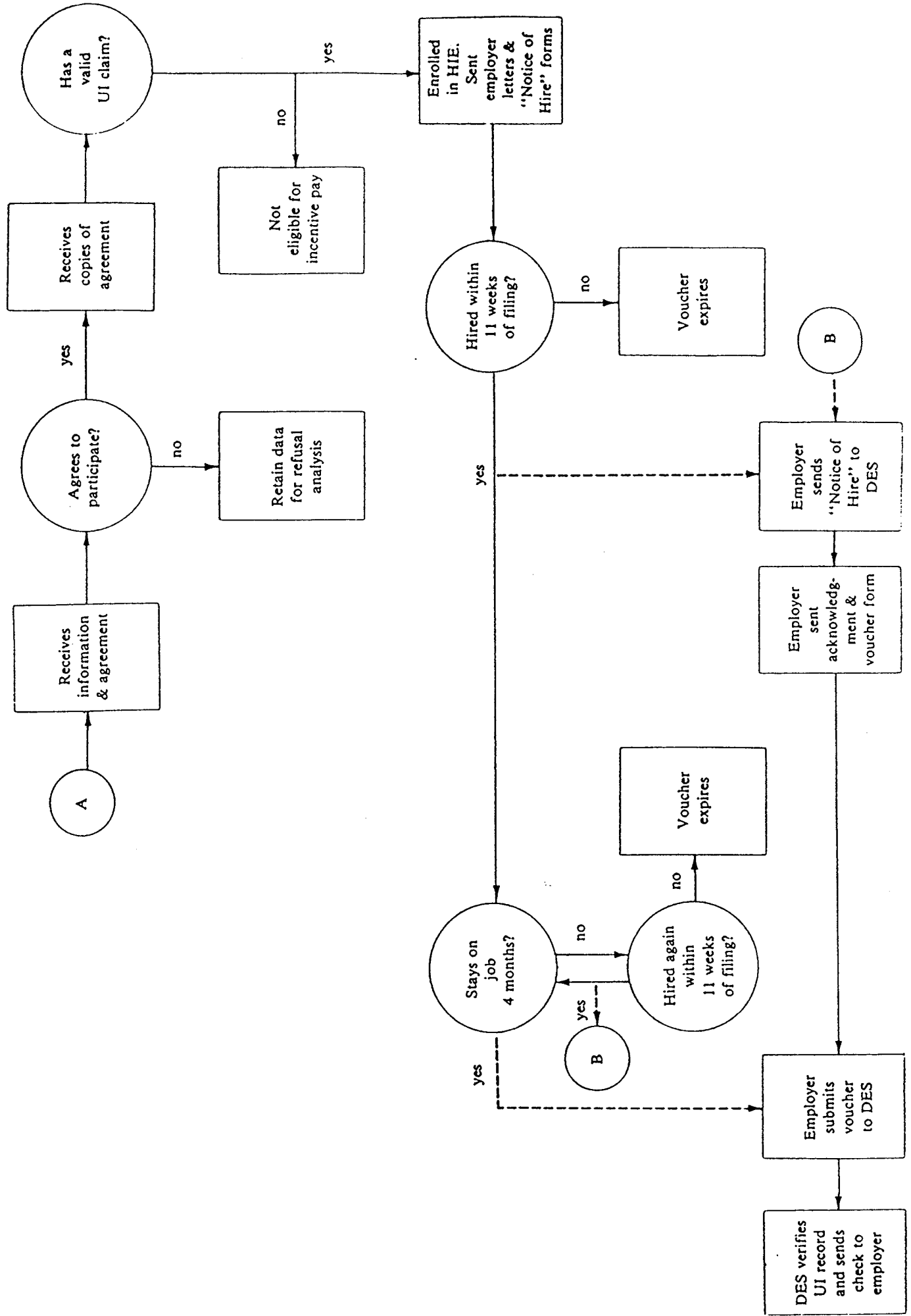


FIGURE 3-3

ACTIONS AND RESPONSIBILITIES
BY EXPERIMENTAL STAKEHOLDERS

ACTION	RESPONSIBLE AGENT				TIME IN WEEKS
	PARTICIPANT	JOB SERVICE	EMPLOYER	ILLINOIS DEPT. OF EMPLOYMENT SECURITY	
FILE UI CLAIM & REGISTER AT JS	X				0-2
CONDUCT BASE LINE SURVEY AND INSTRUCT ELIGIBLES ASSIGNED TO HIE OR JSIE		X			0-2
AGREE TO PARTICIPATE	X				0-2
DETERMINE ELIGIBILITY AND SEND LETTER				X	2-4
SUBMIT NOTICE OF HIRE	X(JSIE)		X(HIE)		2-11
VERIFY BENEFIT TERMINATION AND ISSUE VOUCHER				X	3-12
SUBMIT VOUCHER	X(JSIE)		X(HIE)		17-28
MAKE PAYMENT				X	19-30

X = Agent responsible for initiative.

APPENDIX A3

Forms and Instruments

State of Illinois, Department of Employment Security
910 South Michigan Avenue
Chicago, Illinois 60605

SPECIAL SURVEY OF JOB SERVICE REGISTRANTS

Social Security Number 1.	Name 2.	Date of Birth 3.
4. Are you married or living with someone as if you are married? Yes () No ()		
5. Are any children living with you? Yes () No ()		
6. If yes, how many _____		
7. Are any children living with you under the age of 6? Yes () No ()		
8. Are you living with your parents or another relative? Yes () No ()		
Check sources of other income for you and other household members last month		
	Yourself	Spouse
		Other Household Members
9. Earnings	X X X	()
10. Disability Payments	()	()
11. Dividends/Interest	()	()
12. AFDC	()	()
13. Food Stamps	()	()
14. Pension	()	()
15. Rental Income	()	()
16. Social Security	()	()
17. Other	()	()
18. Excluding your earnings, approximately how much was received from these and other sources by you and other household members?		
Less than \$250/month () \$250-\$499/month () \$500-\$749/month ()		
\$750 - \$999/month () \$1,000/month or more ()		
19. Do you own a house/condominium? Yes () No ()		
20. For how many years have you owned a house/condominium? _____ years		
21. Were you hospitalized anytime during the previous 12 months? Yes () No ()		
22. Have you had an illness/injury during the previous 12 months that prevented you from working for a week or more? Yes () No ()		
For Office Use Only		
Code	Job Service Office	Date
23.	24.	25.

State of Illinois
Department of Employment Security
910 South Michigan Avenue
Chicago, Illinois 60605
(312) 793-4930

You have been selected to take part in a HIRING INCENTIVE EXPERIMENT. Its purpose is to help you get a job faster. Under the experiment, the State of Illinois will pay \$500 to the employer who hires you under certain conditions.

FOR THE EMPLOYER TO RECEIVE THE \$500:

- . YOU MUST BE ELIGIBLE TO RECEIVE UNEMPLOYMENT BENEFITS.
- . YOU MUST START WORK BEFORE THE END OF 11 WEEKS AFTER YOU FIRST FILE FOR BENEFITS.
- . THE JOB MUST BE FOR AT LEAST 30 HOURS PER WEEK.
- . EMPLOYER MUST SEND NOTICE OF HIRE FORM TO DES.
- . YOU MUST STAY ON THE JOB FOR AT LEAST 4 MONTHS.

Participation in the experiment will not affect your claim for unemployment insurance benefits. All UI claimants are required to search for work. The incentive payment offer to employers is to assist your job search.

Here's how it works.

. A job service representative will answer any questions you may have. If you agree to participate, sign the AGREEMENT TO PARTICIPATE attached to this sheet. Read it carefully. You will receive copies.

. About 4 weeks from the date you filed your claim for unemployment benefits, you will receive in the mail a supply of form letters telling employers about the experiment. Each letter will contain your name, social security number and the last date you can start work for the employer to qualify. It will also contain a Notice of Hire form for the employer.

. Take these letters with you whenever you apply for a job. Give a copy of the letter to the employer (or the person who interviews you) and explain that you are in an experiment run by the State of Illinois. This is an example of what you might say:

"Hello. My name is _____. I would like to apply for a job here. Besides being an experienced worker, I am also in a special experiment run by the State of Illinois. The State is offering to pay \$500 to the employer who hires me. This letter explains how it works."

Before you receive these letters, you may give each employer you visit a copy of your Agreement and the attached Notice of Hire form. You may tell them that you will be in the experiment if you get unemployment benefits. In this case, the employer who hires you may be eligible for a \$500 payment.

Make use of the Job Service and any other leads to job openings. The job does not have to come through the Job Service in order for the employer to qualify for the incentive payment. If you need more copies of the letter, you can get an additional supply from the central office of the Department of Employment Security.

State of Illinois
Department of Employment Security
910 South Michigan Avenue
Chicago, Illinois 60605
(312) 793-4930

You have been selected to take part in a JOB SEARCH INCENTIVE EXPERIMENT. Its purpose is to provide you with a bonus if you get a job quickly. Under the experiment, the State of Illinois will pay you an extra \$500 if you become employed under certain conditions.

FOR YOU TO RECEIVE THE EXTRA \$500:

- . YOU MUST BE ELIGIBLE TO RECEIVE UNEMPLOYMENT BENEFITS.
- . YOU MUST START WORK BEFORE THE END OF 11 WEEKS AFTER YOU FIRST FILE FOR BENEFITS.
- . THE JOB MUST BE FOR AT LEAST 30 HOURS PER WEEK.
- . THE NOTICE OF HIRE FORM MUST BE SENT TO DES.
- . YOU MUST STAY ON THE JOB FOR AT LEAST 4 MONTHS.

Participation in the experiment will not affect your claim for unemployment insurance benefits. All UI claimants are required to search for work. The bonus payment offer to you is to assist your job search.

Here's how it works.

. A job service representative will answer any questions you may have. If you agree to participate, sign the AGREEMENT TO PARTICIPATE attached to this sheet. Read it carefully. You will receive a copy.

. About 4 weeks from the date you filed your claim for unemployment benefits, if you are eligible for those benefits, you will receive in the mail a letter telling you that you are enrolled in the experiment. The letter will contain your name, social security number and the last date you can start work for you to qualify for payment. It will also explain what you must do to file for the extra \$500 when you start work.

Make use of the Job Service and any other leads to job openings. The job does not have to come through the Job Service in order for you to qualify for the bonus.

Form: JSIE Instruction, July 10, 1984

State of Illinois
Department of Employment Security
910 South Michigan Avenue
Chicago, Illinois 60605
(312) 793-4930

I, 1., have read the description of the Hiring Incentive Experiment. I understand that if I am eligible to receive unemployment benefits a payment may be made to an employer who hires me. I understand that I must start work on a job of 30 or more hours per week before the end of the 11th week after I filed the first unemployment insurance claim. I further understand that the employer won't be paid unless I stay on that job for at least 4 months. I understand that taking part in this experiment does not affect my claim for unemployment benefits, or change the requirement to look for a job.

I agree to participate in the experiment.

2.

Signature

3.

Date

4.

Social Security Number

State of Illinois
Department of Employment Security
910 South Michigan Avenue
Chicago, Illinois 60605
(312) 793-4930

I, 1., have read the description of the Job Search Incentive Experiment. I understand that I may receive an extra \$500 if I am eligible to receive unemployment benefits. I understand that I must start work on a job of 30 or more hours per week before the end of the 11th week after I filed the first unemployment insurance claim. I further understand that for me to receive the \$500 I must stay on that job for at least 4 months. I understand that taking part in this experiment does not affect my claim for unemployment insurance benefits, or change the requirement to look for a job.

I agree to participate in the experiment.

2.

Signature

3.

Date

4.

Social Security Number

State of Illinois
Department of Employment Security
Field Operations, Third Floor
Attn: Mary Glusak
910 South Michigan Avenue
Chicago, Illinois 60605
(312) 793-4930

Dear Prospective Employer:

This will introduce 1. _____ /2. _____ who is seeking employment and receiving unemployment benefits under the Illinois law. He/she has been selected for an experiment designed to encourage the speedier reemployment of unemployed workers and to reduce outlays for benefits.

The Illinois Department of Employment Security will pay you, as the employer, \$500 if you hire 3. _____ to start work on a job of 30 or more hours per week before 4. _____ and if he/she continues in your employment for at least 4 months.

If you hire, or have already hired, this worker under the conditions specified, the attached Notice of Hire should be promptly completed and mailed in the enclosed self-addressed and stamped envelope. Shortly after receiving the Notice of Hire we will mail a voucher form to you. To receive payment, submit the voucher to the Department of Employment Security after the employee has completed 4 consecutive months of employment of at least 30 hours per week.

Further information about this experiment can be obtained by calling your local Job Service office and asking for the Hiring Incentive Experiment specialist, or by contacting me.

Thank you for your cooperation.

Sincerely,

5.

s/

Mary Glusak
Field Operations Coordinator

State of Illinois
Department of Employment Security
Field Operations, Third Floor
Attn: Mary Glusak
910 South Michigan Avenue
Chicago, Illinois 60605
(312) 793-4930

Dear 1. / 2. :

Please be informed that you are eligible to receive unemployment benefits under the Illinois law, and that you are enrolled in the Job Search Incentive Experiment.

The Illinois Department of Employment Security will pay you \$500 if you start work on a job of 30 or more hours per week before 3. and if you continue in that employment for at least 4 months.

If you start work under those conditions, have your employer promptly complete the attached Notice of Hire and mail it in the enclosed self-addressed and stamped envelope. Shortly after receiving the Notice of Hire, we will mail a voucher form to you. To receive payment, submit the voucher to the Department of Employment Security after you have completed four consecutive months of employment of at least 30 hours per week.

If you need further information about this experiment, call your local Job Service office and ask for the Job Search Incentive Experiment specialist, or contact me.

Thank you for your cooperation.

Sincerely,

4.

s/

Mary Glusak
Field Operations Coordinator

State of Illinois
Department of Employment Security
Field Operations, Third Floor
Attn: Mary Glusak
910 South Michigan Avenue
Chicago, Illinois 60605
(312) 793-4930

From 1. _____
Company Street City State Zip

This serves to notify the Illinois Department of Employment Security that

2. _____ / 3. _____ has been hired in a job for
Name Social Security Number

30 or more hours per week, starting work on 4. _____
Date

We supply the following requested information:

Number of employees currently on the payroll at this establishment (please
check appropriate box): 5.

Less than 20 () 20-49 () 50-249 () 250 or more ()

Type of business 6. _____

About the hired worker:

Wage or salary rate \$ 7. _____ per _____ (indicate hour/week, etc.)

Hours per week 8. _____

Type of work or occupation 9. _____

10. _____

Signature

11. _____

Print Name

12. _____

Print Title

13. _____

Telephone Number

I certify that I have been hired
for a job of 30 or more hours by
the above-named employer.

Employee 14. _____
Signature Date

State of Illinois
Department of Employment Security
Field Operations, Third Floor
Attn: Mary Glusak
910 South Michigan Avenue
Chicago, Illinois 60605
(312) 793-4930

From 1. _____
Company Street City State Zip

This serves to notify the Illinois Department of Employment Security that

2. _____ / 3. _____ has been hired in a job for
Name Social Security Number

30 or more hours per week to start work 4. _____.

Date

We supply the following requested information:

Number of employees currently on the payroll at this establishment (please
check appropriate box): 5.

Less than 20 () 20-49 () 50-249 () 250 or more ()

Type of business 6. _____

About the hired worker:

Wage or salary rate \$ 7. _____ per _____ (indicate hour/week, etc.)

Hours per week 8. _____

Type of work or occupation 9. _____

10. _____

Signature

11. _____

Print Name

12. _____

Print Title

13. _____

Telephone Number

I certify that I have been hired
for a job of 30 or more hours by
the above-named employer.

Employee 14. _____
Signature Date

State of Illinois
Department of Employment Security
Field Operations, Third Floor
Attn: Mary Glusak
910 South Michigan Avenue
Chicago, Illinois 60605
(312) 793-4930

VOUCHER FOR INCENTIVE PAYMENT

1. _____ hereby certifies that it has
employed 2. _____ / 3. _____ continuously in a
Name Social Security Number
job of 30 or more hours per week from 4. _____ to 5. _____.

6. _____

Signature

Name of Official:

7. Title:

Name of Company:

Address:

Following is the requested information about the employee as of 8. _____

Wage or salary rate \$ 9. _____ per _____ (indicate hour/week, etc.)

Hours of work per week 10. _____

Type of work or occupation 11. _____

Send incentive payment to (enter only if change of address): 12. _____

I certify that I was employed by
the above-named employer for
the above-named dates.

13. _____

Signature

Date

State of Illinois
Department of Employment Security
Field Operations, Third Floor
Attn: Mary Glusak
910 South Michigan Avenue
Chicago, Illinois 60605
(312) 793-4930

I, 1. / 2. hereby certify that I have been
Name Social Security Number
employed by 3. continuously in a job of 30
or more hours per week from 4. to 5.

6.

Signature

7.

Social Security Number

Following is the requested information as of 8.

Wage or salary rate \$ 9. per _____

(indicate hour/week, etc.)

Hours of work per week 10.

Type of work or occupation 11.

Send incentive payment to (enter only if change of address):

12.

I certify that 13.
was employed for the above-named dates.

Employer 14. _____
Signature Date

Name of Official:

Title:

Name of Company: 15.

Address:

Chapter 4

DATA BASE CONSTRUCTION

The data used to analyze the Hiring Incentive and Job Search Incentive Experiments are derived from a total of seven sources. In this chapter, we first describe each data source and discuss the information that is available from each. We then offer some general observations about the data base that has been constructed to perform the evaluation and analysis, and discuss its strengths and limitations.

I. Data Sources

Of the seven data sources available to evaluate and analyze the experiments, three derive from instruments that were constructed specifically for the purposes of tracking and evaluating the HIE and JSIE, and the other four are administrative data bases of the Illinois Department of Employment Security (IDES). The three instruments that were created specifically for the experiment--the Base Line Survey, the Office Logs, and the Telephone Follow-Up Survey--all have been touched on in earlier chapters. Discussion of these instruments is confined in this chapter to how each contributed to the data base and to the subsequent evaluation. The four IDES administrative data bases--the Benefits Information System (BIS), Wage Records (WR), the Employment Security Automated Reporting System (ESARS), and the Contributions Tax System (CTS)--are rich and varied sources of data whose use required the extensive cooperation and assistance of IDES personnel. These administrative data bases, which have not been treated earlier, are discussed fully here.

A. Sources Specific to the Experiments

Base Line Survey. The Base Line Survey is the special survey that was administered in the Job Service office at the time each claimant was informed of the experiments. All claimants who were considered potentially eligible to participate in the experiments (or who were potential control group members) were requested to complete a survey, a copy of which is included in Appendix A3. As can be seen, the survey requests the claimant to provide the following information: Social Security number, name, birthdate, marital status, presence of children in the household, other sources of household income and a total dollar sum for those other sources, whether the claimant owned his or her residence, and whether the claimant had been hospitalized or experienced a serious injury within the last year. In addition, the survey was used by JS office personnel, the project monitor and her staff in the Office of Field Operations in the IDES central office in Chicago, and W. E. Upjohn Institute personnel, to record the group to which the claimant was assigned (control, HIE, or JSIE),

whether the claimant agreed to participate in the experiment (for potential HIE and JSIE enrollees only), the four-digit numeric code for the JS office in which the Base Line Survey was taken, and the date the survey was taken.

From the point of view of constructing the analytic data base, the Base Line Survey was used in conjunction with the Office Logs (see below) to obtain the Social Security numbers, group assignments, willingness to participate, and office of enrollment of each claimant. These are clearly critical pieces of information: The Social Security number was used to request administrative data on earnings, insured unemployment duration, and UI benefit receipt on enrollees from the IDES administrative data bases. Further, our ability to ascertain program assignment, agreement to participate, and office of enrollment is central to the validity of the experimental results. Considerable effort, both in the Office of Field Operations in Chicago and at the Upjohn Institute, was devoted to cross-checking and verifying these data.

Although the above data from the Base Line Survey are central to evaluation of the experiments, the other data available from the Base Line Survey--that is, the data on household composition, other sources of income, and so on--are probably less useful than are data from the administrative data bases. The main reason is that the Base Line Survey was voluntary and all the data self-reported. Many surveys were not completed, or were completed in a haphazard manner. Even the Social Security number was often reported inaccurately and had to be cross-checked or obtained by special inquiry into the Bureau Information System. (Indeed, approximately 200 observations--about 1.2 percent of the total--were lost because claimants' Social Security numbers could not be ascertained.) Hence, our use of the self-reported data contained in the Base Line Survey is limited to situations where no administrative data are available.

Office Logs. For the duration of the experiment, each JS office maintained two logs--one for all HIE enrollees and another for all JSIE enrollees. In these logs, JS personnel recorded the Social Security number of each enrollee, his or her name, the date on which the claimant came to the JS office, and whether the claimant agreed to participate in the experiment (either HIE or JSIE) that was presented to him or her. All this information was recorded on the enrollment day--the day the claimant appeared in the JS office.

After the enrollment day, the Office Logs were used to keep track of each enrollee's progress in the experiment. The eligibility of the enrollee for UI benefits (and hence for the experimental bonus) was recorded in the log as soon as it could be determined. The date on which the packet of HIE or JSIE forms and instructions was mailed to eligible participants was also recorded. If a Notice of Hire was submitted by a claimant (or by an employer who had hired an eligible claimant), the date on which

it was received, and the date on which the acknowledgment was mailed in response, were both recorded. Finally, if a voucher were paid, the date on which the completed valid voucher was received in Chicago was recorded.

Before discussing how the Office Logs were used, it is useful to emphasize that they recorded data only on HIE and JSIE enrollees. The omission of controls from the Office Logs posed two problems. First, checks on the identity of controls were difficult to perform, because no Office Logs existed to back up the controls' Base Line Surveys. Hence, problems resulting from illegible handwriting in the Base Line Survey could not be resolved by reference to a second source (that is, the Office Logs). As a result, a few more control group members were lost to the evaluation than were HIE or JSIE enrollees due to inability to identify them. In principle, if some unobserved variable influenced both a claimant's UI reciprocity behavior and the probability of being unidentifiable, then the control group might not be fully comparable to the HIE and JSIE groups (that is, there would be some nonrandomness introduced into the construction of the control group). Since, however, the difference between the proportions of the control, HIE, and JSIE groups that could be identified were negligible, there would appear to be no such problem. Further, the strong similarity in the distribution of observable characteristics across the three samples gives us confidence that assignment to the three groups used to make experimental comparisons was truly random (see Chapter 5, Table 5-2).¹

The second problem raised by the absence of Office Logs for controls was that we had no simple method of determining with complete certainty which controls were nonmonetarily ineligible for UI benefits (and hence ineligible to participate in the experiments). The determination of nonmonetary ineligibility is complicated--in practice it is made on a case-by-case basis by experienced IDES personnel. Although all the data needed to determine nonmonetary ineligibility are available in BIS, no single flag in BIS indicates nonmonetary ineligibility. Hence, we could not be certain whether a claimant assigned to the control

1. The omission of controls from the Office Logs also made it harder to verify the assignment of claimants to the control group (that is, to confirm that they received no treatment), again because the Office Logs did not exist to back up the Base Line Survey. We were, however, able to confirm the assignment of HIE and JSIE enrollees to their respective treatments by checking the Base Line Survey against the Office Logs. Claimants who were recorded as controls in the Base Line Survey, and who in addition did not appear in the Office Logs as HIE or JSIE enrollees, were assigned to the control group in our data base.

group would have been eligible for a bonus had he or she been assigned to one of the experiments.²

This second problem was handled in the following way. The IDES Management Information Systems personnel were able to construct (by a computer algorithm) a rough indicator of whether a claimant was nonmonetarily ineligible. We have used this indicator to remove claimants from the control, HIE, and JSIE groups. Because this indicator is imperfect, we have included in all three groups some claimants who were in fact nonmonetarily ineligible for UI benefits. Note that this poses no problem for the experimental results--the HIE, JSIE, and control groups we compare were constructed identically and are truly comparable in that the only difference among the three is that two of the groups were assigned to experimental "treatments." We simply note that the three groups are larger than is necessary, in that there are individuals in all three who were ineligible for UI benefits.

The Office Logs were used for two main purposes. First, they were used as a check on the most important data that were provided by the Base Line Survey; that is, the Social Security number and name of the claimant, the group to which each claimant was assigned, whether he or she agreed to participate in the experiment, and the office in which enrollment took place. Any conflict that arose between the Office Logs and the Base Line Survey (regarding the identity or group assignment of a claimant) was noted and could usually be resolved. Where identity could not be determined, no administrative data could be obtained, so of course the claimant was dropped from the sample; as already noted, a few more controls than HIE or JSIE enrollees had to be dropped because we could not identify them--that is, no back-up (in the form of an Office Log) existed for the controls. In 182 cases, the group assignment of a claimant could not be determined with certainty, and the only alternative was to discard the claimant from the evaluation. Controls, HIE enrollees, and JSIE enrollees were equally affected by this latter discarding process; hence, it should result in no bias in the results of the experimental evaluation. Further, since 17,306 claimants took the Base Line Survey, our inability to determine the experimental assignment of 182 claimants (about 1 percent) is not especially troubling.

The second use of the Office Logs was to obtain data about which claimants submitted a Notice of Hire and received a voucher payment. (We also know from the Office Logs which HIE and JSIE enrollees were determined eligible for UI benefits and the voucher payment. As already noted, however, comparable data on controls

2. The nature of the problem can be illustrated by an example. When a monetarily eligible claimant in the control group received no benefits, we could not be sure whether we were dealing with a fully eligible claimant who had returned to work (or stopped receiving benefits for some other reason unrelated to eligibility), or a nonmonetarily ineligible claimant.

were not available and required us to use other means to construct a sample of fully eligible claimants.)

Follow-Up Survey. As was noted in Chapter 2, a random sample of HIE and JSIE enrollees was interviewed by telephone in December of 1985 and January of 1986 to obtain further information on reasons for participation in the HIE and JSIE. This Follow-Up Survey is treated separately in Chapter 7 as part of our analysis of participation in the HIE and JSIE. Since the Follow-Up data are not used in the basic evaluation that appears in the following two chapters, discussion of the Follow-Up Survey is deferred to Chapter 7.

B. Administrative Data Bases

The administrative data bases maintained by the Illinois Department of Employment Security are the central source of data on the earnings, employment, and insured unemployment of all who were enrolled in the Hiring Incentive and Job Search Incentive Experiments. Their use by the W. E. Upjohn Institute for the purposes of experimental evaluation required the extensive cooperation of many Management Information Systems and other IDES personnel.

Benefits Information System (BIS). The Benefits Information System is the data base used by IDES to store information on UI claimants and the benefits they receive. It is a complicated data base whose structure is sequential or "layered," with different "layers" containing different aspects of each claimant's claim history.

For each claimant who was a member of the control group or of either experimental group, four kinds of variables were provided directly to the Upjohn Institute from the BIS--date variables, personal and demographic data, earnings data, and data on benefits. Each of these groups of variables is discussed in turn.

Five date variables were made available to us: (1) The initial claim date; that is, the date between July 29 and November 17 inclusive on which each claimant filed for UI benefits; (2) The date of the last day worked by each claimant; (3) The date (if any) on which the claimant resumed work (as best could be determined within the limits of the BIS data base); (4) The date (if any) on which the claimant refiled for UI benefits (following the initial claim date); and (5) The last certification date; that is, the last date on which a certification for a benefit payment was made. We have used these date variables in several ways, usually in conjunction with other data items from BIS and elsewhere. For example, the date variables were essential to determining whether a claimant stopped receiving UI benefits within 11 weeks of filing the initial claim, and whether a claimant who did appear to become reemployed within 11 weeks in

turn retained the new job for four or more months (as opposed to filing an additional claim for UI benefits).

A variety of personal and demographic data were available to us from the BIS. First, the claimant's name, address, telephone number, and county of residence were provided. (The name was used as a back-up for the Social Security number of each claimant, the address and telephone number were required to conduct the Follow-Up Survey, and the county of residence was used to match local labor market data to each claimant's record.) Demographic data available from BIS include: (1) The claimant's date of birth; (2) The claimant's sex; and (3) The race of the claimant (white, black, hispanic, native-American, or other). These variables are important because of the need to know whether the HIE and JSIE had different effects on old and young claimants, on men and women, and on different race groups.

Data on each claimant's earnings were available from both the BIS and Wage Records data bases. From BIS, we obtained the total earnings in each of the four quarters of the base period. (The base period in Illinois is the first four of the last five completed quarters. Hence, for enrollees who filed their initial claims between July 29 and September 30, 1984, we have total earnings in each quarter from 1983:II through 1984:I. For enrollees who filed their initial claims between October 1 and November 17, 1984, we have total earnings in each quarter from 1983:III through 1984:II.)

We were also able to obtain the earnings each claimant received in the base period from each of up to five employers. These employer-specific data are not broken down by quarter, but apply to the whole base period. These data allow us to determine which of up to five employers was the main source of earnings in the base period, and may possibly tell us something about the employment stability of each claimant. Further, since the employer account number of each employer is given, the characteristics of the employer who was the main source of earnings for each claimant in the base period can be matched to the claimant record. (See the discussion of the CTS data below.)

Finally, data on the benefits received by each claimant are available to us. We know: (1) The weekly benefit amount for the claimant; (2) The dependents' allowance (if any) for each claimant; (3) The number of weeks of benefits paid, and the dollar amount of benefits paid, in the spell of insured unemployment immediately following the initial claim; and (4) The number of weeks of benefits paid, and the dollar amount of benefits paid, in the full benefit year. These benefits data are clearly central to an evaluation of the experiments, since the intent of the experiments was to induce shorter spells of insured unemployment and to reduce benefit payments.

In addition, a "stop code," which equals one if a so-called stop was issued on the payments following from the initial claim,

is available to us. This variable was used to approximately determine nonmonetary ineligibility for UI benefits. As noted above, we could not tell with complete certainty which of the claimants assigned to the control group were declared ineligible for benefits on nonmonetary grounds. However, we have used the stop code to cull out many who were nonmonetarily ineligible. A claimant who was monetarily eligible but who received no benefits was considered nonmonetarily ineligible if the stop code was activated. This procedure removed from our sample many--although not all--claimants who were nonmonetarily ineligible, without removing any eligible claimants from the sample.

The BIS data are clearly at the heart of our evaluation of the experiments--without them we would have no means of determining the number of weeks of insured unemployment or the amount of benefits paid to claimants. We have constructed numerous variables based on BIS data elements for the purpose of analyzing the experiments, and these variables will be defined and discussed as they arise.

Wage Records Data Base. Illinois is a wage reporting state, meaning that all covered employers report the wages of their employees to the Illinois Department of Employment Security on a quarterly basis. The Wage Records Data Base is the repository of these earnings data, and when an individual files an initial claim for UI, the Wage Records Data Base is drawn upon to construct an earnings history. This earnings history in turn is used to determine the claimant's monetary eligibility and Weekly Benefit Amount (WBA).

The existence of Wage Records affords us the opportunity to observe earnings in covered employment not only before the initial claim that resulted in enrollment in the experiment, but during and after the experiment as well. The Wage Records of each member of the control, HIE, and JSIE groups were provided to us in the following way. For each of the six quarters starting with 1984:II and ending with 1985:III, we know the earnings of each claimant broken down by employer. Thus, if a claimant had three employers in the third quarter of 1985, we know the identity of the three employers (which allows matching of the claimant record with characteristics of the employer), and how much the claimant earned with each employer in 1985:III.

The third quarter of 1985 (1985:III) was the last quarter of data available in the Wage Records Data Base at the time our extract was created in January 1986. This is because there is a one quarter lag in the posting of wages in the data base. Although more data are always better, 1985:III should be sufficiently recent data in two senses. First, it gives us at least one full quarter of post-experimental covered earnings experience for each control and experimental participant. (Since the last enrollments were made on November 17, 1984, the last enrollee in the experiment would need to obtain a job by February 2, 1985, and hold that job until at least June 1, 1985, in order

to qualify for a \$500 bonus. This implies that 1985:III was the first full "post-experimental" quarter for late enrollees.) Second, it gives us a full quarter of covered earnings experience for late enrollees who exhausted their 26 weeks of state regular UI benefits in a single spell of unemployment. (That is, someone who filed for benefits in mid-November of 1984 would have exhausted his or her benefits by mid-May of 1985 if he experienced a single continuous spell of unemployment.) Such data are useful because they allow us to gauge the post-experimental earnings of HIE and JSIE enrollees relative to controls, which is in turn important because it is possible that job-hunters who foreshorten their job search--as HIE and JSIE participants may have done--would by so doing accept lower-paying jobs and sacrifice earnings in the long run.

Employment Security Automated Reporting System (ESARS).

ESARS is the data base that is used by the Job Service to store data about and track the experience of Job Service clients. Data maintained on ESARS may be used to judge the effectiveness of the Job Service in placing unemployed workers in jobs.

From the viewpoint of experimental evaluation, ESARS is attractive because it provides certain data that are unavailable from any other source. The chief drawback of ESARS for our purposes is that ESARS records do not exist for about 15 percent of the sample we use to analyze the experiments. Thus, if we wish to use education or occupation in our analysis, we are forced to reduce the size of our sample by 15 percent (roughly, from 12,000 to 10,000).

The data provided by ESARS that are unavailable elsewhere include the following: (1) The Dictionary of Occupational Titles occupational code of the claimant (we have used only the first two digits in our evaluation, although the full nine-digit code is available); (2) The highest grade of school completed; (3) Whether the claimant was classified as economically disadvantaged; (4) Whether the claimant was classified as a dislocated worker; (5) Whether the worker was a migrant or seasonal farm worker; (6) Whether the worker was handicapped or disabled; (7) Whether the claimant was a veteran; and (8) The dates on which each claimant received referrals from the Job Service in the weeks following the initial claim.

The data on occupation, education, and referrals received are particularly useful to the evaluation of the experiments. Both occupation and education are indicators of the skills possessed by claimants, and we strongly suspect that the effect of the HIE and JSIE should vary according to skill, with claimants possessing a high level of skill or specialized skills being less influenced by the experimental incentive programs. Referrals received are important in two senses. First, they may indicate the intensity of job search of claimants, allowing us to ascertain whether the experiments motivated claimants to look harder for jobs. Alternatively, they could serve as an indicator of differential

attention paid by Job Service personnel to experimental participants, allowing us to detect any favoritism that might have been shown toward HIE or JSIE participants.

Contributions Tax System (CTS). The CTS data base is where the Illinois Department of Employment Security stores data on the payroll tax contributions that each employer makes to the UI system, on the disbursements to former workers that each employer is responsible for, and hence on the insured unemployment experience of each covered employer. The existence of CTS gives us access to a variety of data on the employers of participants in the experiments. These employer-specific data were made available to us already matched to each participant's Social Security number. Thus, for each participant, we received a number of CTS-employer records equal to the number of employers that the participant had during the period of 1984:II through 1985:III. (Note that this is the same period for which we have an earnings history from Wage Records.) We were then able to select the characteristics of the employers who were responsible for the greatest portion of each claimant's earnings in the periods before and after the initial claim, and to use these two sets of employer characteristics in our analysis.

The data available on each employer are as follows: (1) The seven-digit employer account number; (2) The four-digit code indicating the Standard Industrial Classification (SIC) of the employer; (3) The UI payroll tax rate faced by the employer in each of the six quarters (1984:II through 1985:III); (4) The total wages paid by the employer in each of the six quarters; (5) The taxable wages paid by the employer in each of the six quarters; and (6) The total employment of the employer in each of the six quarters.

In order to keep items (3) through (6) above manageable, we took a six-quarter average of each and have used that average to characterize the tax rate, wages, and employment of each employer over the period in question. Since these data are relatively static and unchanging, this averaging results in little, if any, distortion in the characterization of an employer. In some cases, one or more quarters of data were missing--particularly at the beginning or end of the six-quarter period--presumably because employers came into existence (or became covered by the UI system) or went out of business. In these cases, we averaged over the existing quarters of data. Note that our procedure does not match with each claimant the characteristics of the employer at the time he or she was working for that employer. Rather, it matches with each claimant the average tax rate, wages paid, and employment of that claimant's employers over the full six-quarter period.

II. General Considerations, and the Strengths and Limitations of the Data

The result of matching and merging the four administrative data bases and the data gathered specifically for the experiments is a large data file in which the claimant is the unit of observation. The file includes data on the experimental status of each claimant, on whether each experimental participant submitted a Notice of Hire and received a \$500 bonus, on the timing and duration of each claimant's spells of insured unemployment, on the benefits received by each claimant, on the demographic characteristics of each claimant, on the earnings of each claimant, and on the essential characteristics of each of the employers for whom each claimant worked.

The data base is clearly a rich one, and fully adequate to evaluate and analyze the impact of the Hiring Incentive and Job Search Incentive Experiments. In particular, the availability of the IDES administrative data is advantageous in at least three ways.³ First, it gives us access to a very large sample of UI claimants--over 12,000 who turned out to be fully eligible for the experiments--at relatively low cost. To survey these 12,000 claimants so as to obtain all the measures that are available to us in the IDES data bases would be prohibitively expensive. Second, these administrative data are relatively free of the problems of nonresponse and attrition that tend to plague survey data. (Consider, for example, the problems of nonresponse that beset the Base Line Survey.) Third, we have considerable confidence in the accuracy of the IDES administrative data. To be sure, there are bound to be occasional errors in these data, but the more central are the data to the conduct and monitoring of the UI system, the more likely it is that these data have been checked for accuracy. Many of the variables that are at the heart of the UI system--such as benefit payments and number of weeks of insured unemployment--are also variables that are of central importance to evaluating the HIE and JSIE.

Nevertheless, like all data, the data we use here are subject to limitations that must be acknowledged if results derived from them are to be properly interpreted. The most evident limitation, perhaps, is that we cannot always ascertain from these data the true labor force status of a claimant. That is, we can distinguish only three states of participation in the labor force:

3. For a useful discussion of the relative advantages and disadvantages of survey and administrative data, see Orley Ashenfelter and Gary Solon, "Longitudinal Labor Market Data: Sources, Uses, and Limitations," in What's Happening to American Labor Force and Productivity Measurements? Proceedings of a Conference Sponsored by the National Council on Employment Policy (Kalamazoo, MI: W. E. Upjohn Institute for Employment Research), pp. 109-126.

(1) Employed in the UI-covered sector of the labor market; (2) Unemployed and receiving UI benefits; and (3) Neither employed in the UI-covered sector nor receiving UI benefits. It is this third classification that is troublesome. An individual who is neither employed in a UI-covered job nor receiving UI benefits could be in any of the following states of participation in the labor force: (1) Out of the labor force (that is, neither employed nor seeking employment); (2) Unemployed but not receiving UI benefits (this would be the situation, for example, of an individual who had exhausted his or her benefits but was still seeking a job); and (3) Employed in the non-UI-covered sector of the labor market. These latter three categories of labor-force status cannot be distinguished in the administrative data because the distinctions are irrelevant to the conduct of UI as a program. They are, however, important in an analytical sense--it would be useful, for example, to examine the impact of the HIE and JSIE not just on insured unemployment, but on unemployment and labor force participation as well. To sum up: Because the UI system is in effect blind to an individual unless he or she is employed in a UI-covered job or unemployed and receiving UI benefits, we are unable to appraise the effects of the HIE and JSIE on unemployment (broadly defined) or on labor force participation.

Although we cannot examine unemployment other than insured unemployment, and although we are unable to detect when a person drops out of (or enters) the labor force, the data base we use is still very powerful. It enables us to address the questions that are most relevant to changes that could be made in the UI system: What are the effects of programs like the HIE and JSIE on the duration of insured unemployment and on benefit payments? Which groups of the labor force are most influenced by incentives such as those provided by the HIE and JSIE? Which employers are most likely to make use of a Hiring Incentive program? Answers to these questions allow one to infer answers to broader questions about how the changes in the UI system would affect the performance of the system. In the following chapters, we concentrate on the questions that are most important to potential changes in the UI system.

Chapter 5

EXPERIMENTAL EFFECTS: OVERALL RESULTS

In Chapters 5 and 6, we examine the impacts of the Hiring Incentive and Job Search Incentive Experiments on the duration of insured unemployment, on the benefits received by claimants, and on other variables of interest. Our basic strategy in these two chapters is to take full advantage of the random assignment of initial claimants to the control, HIE, and JSIE groups. In examining experimental effects, we will compare the mean of a variable of interest across each of the three groups, and report appropriate statistical tests of whether differences among those means are significant. Such comparisons yield correct measures of the impact of the HIE and JSIE as long as claimants were in fact assigned randomly to each of the three groups (control, HIE, and JSIE), as evidence we present below strongly suggests is the case.

We begin this chapter with a statistical profile of the UI claimants who were enrolled in the two experiments and in the control group. This statistical profile entails a discussion of figures on enrollment and participation in the experiments. In the second, third, and fourth sections of the chapter we present the basic results of the HIE and JSIE, focusing on the experiments' effects on duration of insured unemployment, benefits received, and post-experimental earnings. The fifth section of the chapter discusses possible biases in the experimental results --that is, the "external validity" of the experiments. A final section summarizes the chapter's findings.

Recall that the basic design of the experiments was as follows. In the Job Search Incentive Experiment (JSIE), a random sample of new claimants for unemployment insurance were instructed that they would receive a cash bonus of \$500 if they found employment (of 30 hours or more per week) before the end of the eleventh week following their initial claim, and if they held that employment for four months. The intent was to create an incentive for claimants to become reemployed more rapidly than they would otherwise.

In the Hiring Incentive Experiment (HIE), another random sample of new claimants was told that their next employer would be eligible for a cash bonus of \$500 if they, the claimants, found employment within 11 weeks of filing the initial claim, and if they held that employment for four months. Once the eligibility of these claimants was determined, each was mailed a packet of materials instructing him or her to advise prospective employers of the experiment and the possibility of receiving a bonus. The intent here was to provide a limited wage-bill subsidy, or perhaps a training subsidy, that might again reduce the duration of insured unemployment.

In the final experimental design, individuals aged 20 to 54 (inclusive) who filed new claims for UI at one of 22 selected UI offices in northern and central Illinois were randomly assigned to one of the experimental groups or to a control group. (Details of the design and operations of the two experiments are treated in Chapters 2 and 3.)

I. Profile of Claimants Enrolled in the Experiments

A total of 17,306 claimants completed the Base Line Survey and were assigned to one of the three groups. However, not all of these claimants were eligible to participate in the HIE or JSIE: 1,857 were monetarily ineligible for UI benefits, were not initial claimants (that is, were filing an additional, reopened, or transitional claim), or could not be located in the Benefits Information System. These 1,857 were deleted from our sample. An additional 3,348 claimants (1,171 who were assigned to the control group, 1,073 who were assigned to the HIE, and 1,104 who were assigned to the JSIE) either failed to meet the age requirements of the experiments, or were nonmonetarily ineligible for UI benefits (that is, failed to meet the separation and availability requirements of the Illinois UI system), as determined by our constructed nonmonetary eligibility code.¹ These 3,348 have also been deleted from our sample.

As a result, we are left with a sample of 12,101 claimants who are both eligible for UI benefits and eligible to participate in the experiments--3,952 assigned to the control group, 3,963 claimants who were offered the opportunity to participate in the HIE, and 4,186 claimants who were offered the opportunity to participate in the JSIE. Note that the procedures used to construct these three sample groups were identical, so that they can be treated as three random samples from the population of fully eligible initial claimants for UI benefits who were aged 20 through 54 (inclusive).

The first row of Table 5-1 displays these data on the number of claimants in each of the three samples. Table 5-1 also shows that there are important differences between the HIE and the JSIE in the willingness of claimants to participate in each. Whereas

1. As noted in Chapter 4, we know the true eligibility status only of participants in the HIE and JSIE, because Office Logs were kept only on experimental participants, not on those assigned to the control group. IDES provided us with a variable flagging most--but not all--of those who were nonmonetarily ineligible for benefits. We refer to all claimants in the remaining sample of 12,101 as "eligible," for simplicity, although it is understood that some members of this sample may in fact have been nonmonetarily ineligible for UI benefits, and hence ineligible for a \$500 bonus.

84 percent of the eligible claimants who were offered the chance to participate in the JSIE chose to participate, only 65 percent of those offered the chance to participate in the HIE agreed to do so. Because the differences between the HIE and JSIE in the willingness of claimants to participate in each are potentially important to the effectiveness of each program, they are examined in detail in Chapter 7.

Table 5-1 further indicates that actual use of programs--as shown by the return of Notices of Hire and the actual cashing of bonuses--differed greatly between the HIE and JSIE. Whereas 765 Notices of Hire were submitted by JSIE participants, and 570 vouchers were actually cashed by these claimants, only 199 Notices of Hire were submitted and 112 vouchers cashed by employers under the HIE. The limited use of the HIE by claimants and employers suggests that, overall, the experiment had limited scope for reducing UI benefits paid or the weeks of insured unemployment, although results that we present in Chapter 6 suggest that the HIE did have an impact on certain subgroups of claimants.

Table 5-2 shows demographic and other characteristics of each of the three samples. Note that the figures are based on the sample of all monetarily and nonmonetarily eligible claimants who met the age and initial claim restrictions of the experiments--the same sample as is shown in Table 5-1. In effect, Table 5-2 offers a profile of the insured unemployed in northern and central Illinois during the third and fourth quarters of 1984. The following points are evident from Table 5-2. About 55 percent of the eligible claimants who were taken into the program were men. Slightly under two-thirds were white, somewhat over one-quarter were black, and about 8 percent were hispanic. The white and black subsamples are large enough to detect program effects in the expected range for either of these subgroups; however, the hispanic subsample is relatively small, and since there are only 90 Native Americans and 177 claimants of other ethnic origin (chiefly Asian) in the sample, it is highly doubtful that anything short of an enormous effect could be detected for these subgroups individually.

About 43 percent of the eligible claimants taken into the experiments were in their 20s, and a declining proportion of the sample is in successively older age groups. Slightly over half of the claimants indicated that they were married, and about the same proportion indicated that they had at least one child in their household. About 41 percent of the claimants said they received \$250 or more in income from some source other than their own earnings--from the earnings of a spouse or from an income transfer program, for example. Somewhat less than one-third said they were homeowners. Finally, roughly 12 to 14 percent indicated that they had experienced health problems in the last year.

Although the figures in Table 5-2 may be of interest in their own right, their main import lies in the support they lend to the randomness of the three sample groups. Indeed, none of the

differences in proportions between any pair of groups is statistically nonzero at conventional confidence levels. The randomness of the three subsamples along the lines of observable characteristics suggests that the three subsamples are in fact random. It follows that comparisons between the HIE group and the control group (or between the JSIE 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 the HIE group with the mean weeks for members of the control group will show the impact of the HIE on the duration of insured unemployment. The simplicity with which experimental results can be extracted--once the data have been appropriately assembled--illustrates the power of the experimental method.

II. Overall Effects of the Experiments on Benefit Receipt and Duration of Insured Unemployment

Table 5-3 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 5-1 and 5-2. We stress that this sample includes eligible claimants who refused to participate in one of the experiments, so that the HIE and JSIE groups can be legitimately compared with the control group. (Examining only HIE and JSIE group members who agreed to participate would involve a comparison of self-selected groups--HIE and JSIE agreeers--with all controls, some of whom would have refused participation had they been offered the opportunity. The result could be seriously 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 HIE and JSIE, the comparable figures are \$2,158 and \$2,074. 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 FSC received, but this time for the entire benefit year.

We would argue that the full benefit year is the appropriate time period to examine in determining the impact of the experiments on benefit receipt, rather than simply the spell of insured unemployment following the initial claim. It is possible that the HIE and JSIE 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 whole 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 only if interpreted as tests on the number of weeks of insured unemployment. Because weeks of insured unemployment are a "censored" measure of actual unemployment, special problems arise in using insured unemployment data to draw inferences about the duration of unemployment.²

The entries at the bottom of Table 5-3 show the proportion of claimants who exhausted their state regular benefits, and the proportion of claimants who terminated benefits within 11 weeks of their initial claim.

Table 5-4 displays differences between the mean values of the control group and the HIE group, and between the control group and the JSIE group. The differences are calculated from Table 5-3, and the probability value of each difference is shown.³ The most striking results shown by the table pertain to the JSIE: Average benefit receipt was lower in the JSIE group than in the control group by \$158 to \$194 over the whole benefit year (depending on whether FSC benefits are included in benefits received). The low probability values give us confidence that these differences are statistically nonzero. Further, the average

2. Insured unemployment is a censored measure of actual unemployment for two reasons. First, our administrative data allow us to observe only up to 26 weeks of unemployment for claimants who received state regular benefits, and up to 38 weeks for those who received FSC. Second, we do not know the actual labor force status of a claimant--as it would be determined by the Bureau of Labor Statistics in the Current Population Survey--unless he or she is either receiving UI benefits or employed in the covered sector. Thus, a break in a claim series as shown in the administrative data has an ambiguous interpretation. For both reasons, insured unemployment masks the true duration of unemployment.

3. The probability value gives the probability that the difference shown is in fact zero. For example, a probability value of 0.01 implies that there is only one chance in one hundred that the difference shown is simply a fluke. Generally, differences with probability values of 0.05 or less will be viewed as "statistically significant" or "statistically nonzero." (We have used a two-tailed criterion in constructing the probability values.)

number of weeks of insured unemployment was lower in the JSIE group than in the control group by somewhat over a week, again over the full benefit year. Finally, compared with JSIE enrollees, 3.2 percent more controls exhausted their state regular benefits, and 5.5 percent fewer controls terminated their benefits within 11 weeks of filing their initial claim.

The JSIE 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 JSIE enrollees, whether or not they agreed to participate, whether or not they actually cashed a voucher for \$500. (We explore the relationship between actual program use and duration of unemployment below.)

The results for the HIE are quite different. Although there appears to have been an initial reduction in benefits received by the HIE group in the spell of unemployment immediately following the initial claim, the reduction in benefits paid to HIE enrollees over the whole benefit year is statistically insignificant. (Note the probability values of 0.11 and 0.19 associated with the benefit differences over the full benefit year.) The evidence of an impact of the HIE on the number of weeks of insured unemployment is similar--although there appears to have been a reduction during the first spell of unemployment, the difference between controls and HIE enrollees over the full benefit year cannot be viewed as statistically significant. (Note the probability value of 0.19.)

In fact, in view of the comparatively low rate of use of the HIE, it may seem surprising that there was an impact even in the first spell of unemployment. The results suggest that, to the extent the HIE reduced the length of the initial spell of unemployment, this effect did not persist over the full benefit year. In other words, the data fail to support a finding that the HIE had any overall impact on benefit receipt or the duration of insured unemployment over the full benefit year. We stress, however, that evidence presented in Chapter 6 shows that the HIE did reduce the benefits paid and the weeks of insured unemployment of white women. Hence, these overall results mask an effect of the HIE on at least one major group of UI claimants. That the HIE affected one group of claimants but not others, and the relatively low level of participation in the HIE, lead us to conclude that much needs to be learned about the determinants of participation in the HIE before a full appraisal of a program based on the HIE can be made. We defer detailed discussion of this issue to Chapter 7.

To summarize, the results of the JSIE are striking. The incentive created by the \$500 bonus, which was actually paid to 570 claimants, appears to have reduced state regular benefit payments during the entire benefit year by an average of \$158. It is important to emphasize that this \$158 average reduction was

achieved over 4,186 claimants, representing all eligible claimants in the JSIE sample--not just those who received a bonus.

It follows that the state paid roughly \$660,000 less to members of the JSIE group than it would have paid in the absence of the program (a \$158 reduction in benefit payments per claimant times 4,186 claimants equals \$661,388). Since JSIE bonus payments amounted to \$285,000 (the \$500 bonus was paid to 570 claimants), the state succeeded in reducing its outlays by over \$375,000. This reduction of \$375,000 represents a 3.6 percent reduction in total state regular benefit payments made to claimants who were enrolled in the JSIE. In view of this reduction, a program modelled on the JSIE would seem to be extremely attractive from the state's point of view: For each \$1.00 of bonus payments made, state regular benefit payments were reduced by about \$2.32.

III. Overall Impact of the JSIE on Claimants Who Submitted Notices of Hire or Received Bonus Payments

The effects of the JSIE on the duration of insured unemployment and on benefit receipt are so striking that it is useful to digress briefly to ask whether they are plausible. At this point we simply ask the following: What do the results of the JSIE imply about the behavior of claimants who we know participated in the experiment; that is, about the behavior of those who submitted Notices of Hire or actually received a \$500 bonus?

At the most literal level, the question can be answered as follows. A 1.15 week reduction in the duration of insured unemployment was achieved over a sample of 4,186 JSIE enrollees. This implies that, in aggregate, the JSIE enrollees experienced 4,813.9 fewer weeks of insured unemployment than did the members of the control group (1.15 weeks times 4,186 claimants equals 4,813.9 weeks). If the 4,813.9 fewer weeks of unemployment were concentrated on claimants who submitted a Notice of Hire, then each of the Notice of Hire submitters would have shortened his or her spell of unemployment by an average of 6.3 weeks (4,813.9 weeks divided by 765 submitters). Alternatively, if the 4,813.9 fewer weeks of unemployment were concentrated on claimants who received a \$500 bonus, then each of the bonus recipients would have shortened his or her unemployment spell by an average of 8.4 weeks (4,813.9 weeks divided by 570 bonus recipients).

Hence, some simple arithmetic suggests that the results we observe could have resulted if those who actually submitted

4. The JSIE enrollees would have received \$10.4 million in state regular benefits in the absence of the JSIE (4,186 claimants times \$2,487 per claimant). The \$375,000 reduction is 3.6 percent of \$10.4 million.

Notices of Hire or received a \$500 bonus shortened their spells of unemployment on average by 6.3 to 8.4 weeks. Although these are sizable reductions, they are by no means out of the realm of possibility, given that (on average) 20.1 weeks of benefits were received by members of the control group.

As a further check on the plausibility of the results, we have compiled data on experimental outcomes by participation category--that is, for those who agreed to participate, refused to participate, submitted a Notice of Hire, and received a bonus. Because these data mix true experimental effects with other effects (such as the effects of an individual's unobserved motivation to search for a job), we have relegated these data to the Appendix of this chapter. The data do, however, tend to support the plausibility of the results.

IV. Overall Experimental Effects on Earnings after Reemployment

Although the results presented in the previous sections suggest that a program based on the JSIE could be highly effective in reducing UI program costs and in alleviating the burden on the UI Trust Fund, an important secondary effect of the program must be explored before a program based on the JSIE could be recommended. It is possible--indeed, the theory of job-search suggests--that the shorter search time induced by the job-search incentive might result in a less favorable match between worker and job, which would manifest itself in lower earnings in the subsequent job. If a JSIE participant who submitted a Notice of Hire (or received a bonus) simply accepted the first job that presented itself, the claimant's earnings after reemployment and the efficiency of the labor market would both be reduced.

Table 5-5 addresses the concern that JSIE participants may have sacrificed earnings in their post-program job in order to obtain the \$500 bonus. The table displays data on the pre- and post-program 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 new employment, as well as claimants who dropped out of the labor force, are excluded from consideration here. Since our concern focuses on the earnings of those who found new employment, and whether these earnings are lower for JSIE enrollees, this is clearly the appropriate group to examine.

The first row of Table 5-5 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 pre-program earnings measures.⁵ (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 5-5 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 that there is no difference between the post-program earnings of controls and of JSIE enrollees--the average for the controls is \$3,121, whereas the average for the JSIE 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 JSIE participants did not come at the expense of lower earnings. Rather, the data are consistent with the idea that the faster reemployment of JSIE enrollees resulted from more intense job search efforts by JSIE enrollees, and not from overly rapid acceptance of job offers.

The failure to find any evidence of earnings deterioration of JSIE enrollees tends to reinforce the provisional conclusion reached above that a job-search incentive program does increase the intensity of job search and would be efficient.

V. Internal Validity of the Experiments

In the remainder of this chapter we focus on the "internal validity" of the HIE and JSIE. The question here is whether comparing the control group with either experimental group should result in a biased view of experimental effects. Thus, internal validity refers to the validity of the results on their own grounds, or "internally."⁶

5. 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 actual differences (\$23 and \$17). 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. (All of these statistics are calculated from the subsample of claimants who became reemployed.)

6. "External validity" refers to the validity of the experimental results in another context or environment; that is, to the "transferability" of the results, as Aigner has called it. (See Dennis J. Aigner, "The Residential Electricity Time-of-Use Pricing Experiments: What Have We Learned?" in Social Experimentation, edited by Jerry A. Hausman and David A. Wise (Chicago: NBER and University of Chicago Press, 1985), pp. 11-53.) External validity is a concern mainly in determining whether

The presence of any of four problems may jeopardize the internal validity of an experiment. We will refer to these problems as learning effects, Hawthorne effects, selective attrition, and displacement effects.

A. Learning Effects

Learning effects are changes in the behavior of experimental participants that occur over time as the experiment proceeds. Learning effects occur for at least two reasons. First, participants may increase their understanding of the consequences of their behavior within the experiment as the experiment progresses. Second, it may take time for individuals to make adjustments and rearrangements in response to new or experimental incentives.

If learning effects are an inherent part of a program, then it is likely that a short-term experiment designed to test the effects of that program will obtain biased results. Specifically, if an experiment is too short to allow participants to understand and respond to the incentives created by an experiment, then of course the experimental results will be unlikely to show any experimental impact. The bias associated with learning effects can be avoided by designing the experiment so that it is long enough for participants to understand and respond to it.

We believe that the Job Search Incentive Experiment was relatively free of problems associated with learning effects. It is evident that the JSIE was simple to understand, and participants needed little time to respond and adapt to the program.

The Hiring Incentive Experiment, on the other hand, may have encountered problems resulting from learning effects. First, the HIE was inherently more complex. It required the claimant to explain to potential employers that they (the employers) would become eligible for a \$500 bonus by hiring the claimant and retaining the claimant for at least four months. Hence, the HIE required a higher level of comprehension, participation, and activity by the claimant. Also, it required the participation of the employer, who had little or no prior knowledge or understanding of the HIE. Second, employers may need to make time-consuming adjustments in order to make efficient use of the HIE. It is possible that more employers would have taken advantage of the HIE if they had been given more time to use it, or if they had been able to plan for it in advance. Reorganizing the workplace can be a costly and lengthy activity, and employers were caught unawares by the HIE.

experimental results give an accurate picture of what would occur if the experiment were turned into a program. We address the problem of external validity in Chapter 8.

The likelihood that the HIE experienced problems of learning effects leads us to believe that the experimental results of the HIE may be downward biased. That is, if the HIE had been of greater duration (and if it had been given greater publicity), participation in the HIE by claimants and use of the \$500 bonus by employers would probably have increased. That increase in participation and use would have been accompanied by an increase in the observed effects of the program on benefit payments and unemployment duration.

That the HIE did have a measurable effect on white women (see Chapter 6, Table 6-8) suggests both the potential of such a program and the desirability of further experimentation. Clearly, though, further experiments with hiring incentives would need to be conducted over longer periods of time, so as to mitigate the problems of learning effects that we believe were encountered with the HIE.

B. The Hawthorne Effect

The Hawthorne effect takes its name from an early experiment in which changes in lighting and room color were undertaken to determine the effect of such changes on workers' productivity. The experimenters did find that productivity improved, but they discovered that the improvements resulted not from the tested changes in lighting and color, but rather from the increased attention that was paid to workers whose work spaces were changed. Thus, a Hawthorne effect exists if subjects respond to an unintended treatment rather than to the designed treatment.

In the Illinois Hiring Incentive and Job Search Incentive Experiments, a Hawthorne effect could have resulted from either of two circumstances. First, it is possible that experimental participants increased the intensity of their job search not in response to the bonus offer, but simply because they knew they were part of an experiment and wanted to please the experimenters. Second, Job Service personnel could have made special efforts to place experimental claimants in jobs.

For the first circumstance to lead to a Hawthorne effect, experimental claimants would need to be aware that reduced duration of insured unemployment was a main objective of the experiments. Although the extent to which office personnel made claimants aware of the experiments' objectives is unknown, the relationship between the experimental treatments and the duration of unemployment is so obvious that it could have resulted in a Hawthorne effect. Nevertheless, we believe that Hawthorne effects arising from claimants' attempts to please the experimenters pose only a minor problem for the HIE and JSIE. Because UI claimants already face a work-search requirement and are monitored by the Department of Employment Security, it seems unlikely that the presence of an experiment would alter the environment enough to result in changed behavior.

Regarding the second possible source of a Hawthorne effect, we can test for the possibility that the faster reemployment of experimental enrollees might have resulted from greater efforts of the Job Service to place these claimants in jobs. Table 5-6 shows the number of referrals received by each of the three groups, and suggests that there was no difference between the control group and either experimental group in the number of referrals received. The percentage of controls who received referrals was 12.3, whereas 11.1 percent of the HIE enrollees and 12.2 percent of the JSIE enrollees received Job Service referrals. The differences (1.2 percent for the HIE group, 0.1 percent for the JSIE group) are not large enough to be statistically different from zero.

The main defenses against a Hawthorne effect are to prevent participants from knowing what behavior the experimenters are seeking to measure, and to prevent unintended influences on participants' behavior (such as increased placement efforts) from cropping up. Clearly, further experiments along the lines of the HIE or JSIE should continue to make every effort to guard against Hawthorne effects.

C. Selective Attrition

The internal validity of an experiment is jeopardized if attrition from the experimental sample occurs nonrandomly. Starting with those who initially refuse to participate, and ending with those who for any number of reasons withdraw from the experiment, it is possible that attrition is nonrandom and that those who remain in the program (and in the sample) are somehow special and systematically different from members of the control group.

Nonrandom attrition destroys the essence of the experimental advantage. For example, in the Job Search Incentive Experiment, those who refused to participate may have been less serious about finding a job than those who participated. A comparison of controls with only those who agreed to participate would lead to an overestimate of the effects that would be observed if the program were implemented.

The most credible solution to selective attrition is to compare the behavior of all those who were offered the experimental treatment with the behavior of the control group. This is exactly what we have done in section II above. Such a comparison is clearly unbiased, since random assignment assures that the characteristics of the treatment group are the same as those of the control group.

7. We are fortunate to have this option available to us. In many experiments, especially those that rely on surveys rather than on administrative data, attrition implies the loss of data on those who drop out of the experiment. Our reliance on administrative data is a great advantage.

D. Displacement

Any experiment that is intended to improve the employment prospects of its participants may face the problem of displacement. Displacement occurs when improvements that are experienced by experimental participants come at the expense of others. For example, in the Hiring Incentive Experiment, those who participated in the experiment may have gotten jobs that others would have gotten, except that those "others" did not carry a \$500 voucher. Similarly, participants in the Job Search Incentive Experiment may have taken jobs that, had those jobs remained open, would have been taken by others.

When it occurs, displacement can jeopardize either the internal validity of an experiment, or the external validity of an experiment. If the performance of the control group suffers because of the improved performance of the experimental group, then the internal validity of the experiment comes into question. That is, if the individuals whose performance in the labor market has been harmed are members of the control group, then comparing the performance of experimental participants with that of controls will result in an overstatement (or biased estimate) of the true effect of the experiment on participants. Only if the experience of controls is what it would have been in the absence of the experiment does a comparison of experimentals with controls yield proper experimental effects.

On the other hand, if the individuals who are harmed by the experiment are completely outside the experiment (that is, they are neither controls nor experimentals), then a comparison of experimentals with controls will yield correct estimates of the experiment's effects, but may nevertheless overstate the benefits of a full-scale program based on the experiment. The overstatement results because a comparison of controls with experimentals correctly estimates the benefits that accrue to experimentals, but fails to count the harm done to nonparticipants. This is a problem of external validity, and it will be dealt with in Chapter 8.

There is only a remote possibility that the HIE and JSIE were subject to a displacement problem that would affect the internal validity of the results. In the Hiring Incentive Experiment, the number of job applicants carrying \$500 vouchers was small enough (relative to the number of vacancies in the potentially affected labor markets) that the experiment probably had little effect on control group members. (See Chapter 8 for a complete discussion.) Thus, comparing controls with experimentals should yield correct estimates of the effect of the experiment on participants.

8. Such an effect might be referred to as a "dowry effect," in the sense that the \$500 bonus can be thought of as a gift or dowry borne by the claimant to the employer.

Similarly, it is unlikely that a comparison of the performance of those who participated in the JSIE with controls would be marred by displacement, because again the number of job vacancies in the region was large compared with the number of JSIE participants. The effectiveness of this experiment derived from the increased intensity of job search, which is likely to increase the efficiency of job matching, but is unlikely to displace other job seekers in a large, growing labor market.

We are convinced that displacement poses no problem to the internal validity of the experimental results. But again, this does not necessarily imply that displacement would remain benign if actual programs based on the HIE or JSIE were adopted. We defer this issue to Chapter 8.

E. Conclusions on Internal Validity

We conclude that, although the Hiring Incentive Experiment was probably free of problems resulting from Hawthorne effects, selective attrition, or displacement, it may have been subject to problems arising out of learning effects. That is, if the HIE had been of greater duration (and if it had been given greater publicity), it seems likely that participation in the HIE by claimants and use of the \$500 bonus by employers would have increased. That increase in participation and use would have been accompanied by an increase in the observed effects of the program on benefit payments and unemployment duration. It follows that our estimates of the HIE's effects are likely downward biased, understating the effects of the experimental treatment.

The Job Search Incentive Experiment, on the other hand, appears to have been free of any problems that would jeopardize the internal validity of its results. Although Hawthorne effects are always a possibility, suitable precautions were taken against them. Further, there appears to have been minimal potential for problems to arise from learning effects, selective attrition, or displacement. We conclude that the estimates of the JSIE's effects are virtually free of bias and hence internally valid.

VI. Summary

Section I of this chapter offers a profile of the claimants who were enrolled in the control group, the Hiring Incentive Experiment, and the Job Search Incentive Experiment. The main conclusion of that section is that the random assignment of claimants to each of the three groups appears to have been

9. The logical way to overcome such bias in any future experiment would be to conduct the experiment over a longer period of time, and to publicize it with employers.

successful. No statistically significant differences between HIE enrollees and controls, nor between JSIE enrollees and controls, could be measured. It follows that a straightforward comparison of the performance of HIE or JSIE enrollees with the performance of the control group will show the impact of the experiments on variables of interest--duration of insured unemployment and benefit receipt, for example.

Section II presents the central experimental results; that is, the overall effects of the HIE and JSIE on benefit receipt and unemployment duration. Looking at the entire group of HIE enrollees, there appears to have had no statistically significant impact of the HIE on benefit receipts or weeks of insured unemployment over the full benefit year. But as Chapter 6 will show, this overall result masks an effect of the HIE on an important subgroup of claimants--white women.

In contrast, the effect of the JSIE on benefit receipt and the duration of insured unemployment appears to have been very strong. For claimants enrolled in the JSIE, the incentive created by the \$500 bonus, which was actually paid to 570 claimants, appears to have reduced state-regular benefit payments during the entire benefit year by an average of \$158, and to have curtailed the average amount of insured unemployment by 1.15 weeks. We should emphasize that the \$158 reduction in benefit payments, and the 1.15 week curtailment of unemployment, were achieved over 4,186 claimants representing all eligible claimants in the JSIE sample--not just those who received a bonus.

In section III we explore the plausibility of the results of the JSIE, which are so striking, and conclude that the behavior of claimants implied by the experimental results is well within the realm of possibility.

In section IV we ask whether the shorter job-search time induced by the JSIE resulted in a less favorable match between worker and job. In other words, did JSIE participants accept the first available job, rather than wait for a better paying job that would represent a better match? By comparing the earnings of controls and JSIE participants after reemployment, we conclude that the answer is no: There is no statistically significant difference between the post-reemployment earnings of controls and JSIE enrollees.

Section V offers a discussion of the internal validity of the experimental results. But we are concerned that learning effects may have marred the internal validity of the HIE. That is, the duration of the HIE may have been too short to allow claimants and employers to become convinced of the legitimacy of the HIE, to understand it, and to respond to it. In short, the HIE is a more complex experiment requiring greater learning time. It follows that our estimates of the HIE's effects probably understate what would have occurred if the HIE had been of greater duration or if it had been publicized with employers. Our conclusion for the

JSIE is quite different: We believe that the JSIE was an internally valid experiment and that our estimates of its effects are unbiased.

TABLE 5-1

Illinois Hiring Incentive and Job Search Incentive Experiments:
Program Participation and Use

	Control		HIE		JSIE	
	N	Prop.	N	Prop.	N	Prop.
Eligible <u>a,b/</u>	3,952	1.00	3,963	1.00	4,186	1.00
Agreed to Participate <u>c/</u>	-	-	2,586	0.65	3,527	0.84
Submitted Notice of Hire <u>d/</u>	-	-	199	0.05	765	0.18
Voucher Paid	-	-	112	0.03	570	0.14

SOURCES: Eligibility from Illinois Department of Employment Security, Benefits Information System; other data from Office Logs kept during the experiments.

NOTES:

- 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. (Nonmonetary eligibility was determined here by our constructed nonmonetary eligibility code. See text for discussion.)
- 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 (i.e., were filing additional, reopened, or transitional claims), or could not be located in the Benefits Information System. An additional 3,348 (1,171 controls, 1,073 HIE, and 1,104 JSIE) were nonmonetarily ineligible (that is, 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 5-2

Characteristics of Claimants Assigned to Programs

	Control		HIE		JSIE	
	N	Prop.	N	Prop.	N	Prop.
Total	3,952	1.000	3,963	1.000	4,186	1.000
Male	2,162	0.547	2,131	0.538	2,357	0.563
White	2,497	0.632	2,565	0.647	2,723	0.651
Black	1,072	0.271	1,014	0.256	1,050	0.251
Hispanic	299	0.076	304	0.077	310	0.074
Native American	23	0.006	25	0.006	42	0.010
Other Race	61	0.015	55	0.014	61	0.015
Age under 20	0	0.000	0	0.000	0	0.000
Age 20-29	1,680	0.425	1,679	0.424	1,827	0.436
Age 30-39	1,315	0.333	1,292	0.326	1,357	0.324
Age 40-49	708	0.179	740	0.187	776	0.185
Age 50-54	248	0.063	252	0.064	226	0.054
Age over 54	0	0.000	0	0.000	0	0.000
Weekly Benefit Amount:						
\$ 51	347	0.088	333	0.084	355	0.085
\$ 52 - \$ 90	794	0.201	861	0.217	887	0.212
\$ 91 - \$120	666	0.169	711	0.179	738	0.176
\$121 - \$160	749	0.190	716	0.181	822	0.196
\$161	1,396	0.353	1,342	0.339	1,384	0.331
Dependents' Allowance	1,834	0.323	1,883	0.332	1,955	0.345

TABLE 5-2 (Continued)

Characteristics of Claimants Assigned to Programs

	Control		HIE		JSIE	
	N	Prop.	N	Prop.	N	Prop.
Married	1,886	0.509	1,917	0.529	2,006	0.517
Children in Household	1,997	0.528	2,003	0.540	2,046	0.521
Child under 6 in Household	967	0.255	971	0.262	1,016	0.258
Over \$249/Month of Other Income	1,523	0.392	1,602	0.419	1,751	0.436
Own House	1,197	0.316	1,237	0.335	1,235	0.314
Hospitalized in Last Year	423	0.112	463	0.126	479	0.123
Illness in Last Year	520	0.138	519	0.141	546	0.139

SOURCES: Totals, and data on sex, race, and age, from Illinois Department of Employment Security, Benefits Information System (BIS); data on marital status, children, other income, home ownership, and health status from Base Line Survey administered to claimants at time of filing.

NOTES: All initial claimants who met the program criteria and were eligible for UI benefits are included. A few nonmonetarily eligible claimants are also included for reasons discussed in the text.

Proportions for Base Line Survey data are calculated excluding those who failed to respond to a question.

TABLE 5-3

Means of Program Variables by Experimental Group

	Control		HIE		JSIE	
	<u>Mean</u>	<u>Std.Error Of Mean</u>	<u>Mean</u>	<u>Std.Error of Mean</u>	<u>Mean</u>	<u>Std.Error of Mean</u>
Benefits Paid (\$):						
(1) State Regular, First Spell	2,267	27.5	2,159	27.4	2,074	26.7
(2) Total, First Spell	2,558	33.8	2,446	33.8	2,329	32.9
(3) State Regular, Benefit Year	2,487	27.0	2,426	27.0	2,328	26.3
(4) Total, Benefit Year	2,786	33.1	2,725	33.8	2,592	32.2
Weeks of Insured Unemployment:						
(1) First Spell	18.3	0.205	17.7	0.205	17.0	0.199
(2) Benefit Year	20.1	0.194	19.7	0.194	18.9	0.188
Proportion of Claimants who:						
	<u>Prop.</u>	<u>Std.Error of Prop.</u>	<u>Prop.</u>	<u>Std.Error of Prop.</u>	<u>Prop.</u>	<u>Std.Error of Prop.</u>
(1) Exhausted Benefits	0.478	0.008	0.464	0.008	0.446	0.008
(2) Ended Benefits ≤ 11 Weeks	0.353	0.008	0.384	0.008	0.408	0.008
N	3,952		3,963		4,186	

SOURCE: Tabulations from Illinois Department of Employment Security, Benefits Information System.

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 and federal supplemental compensation (FSC). "Benefit Year" refers to benefits paid or weeks paid during the full benefit year for each claimant.

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.

TABLE 5-4

Differences Between Control Group and Experimental Group Means

	HIE minus Control		JSIE minus Control	
	<u>Difference of Means</u>	<u>Probability Value</u>	<u>Difference of Means</u>	<u>Probability Value</u>
Benefits Paid:				
(1) State Regular, First Spell	-108**	0.01	-193**	0.00
(2) Total, First Spell	-112*	0.02	-229**	0.00
(3) State Regular, Benefit Year	-61	0.11	-158**	0.00
(4) Total, Benefit Year	-61	0.19	-194**	0.00
Weeks of Insured Unemployment:				
(1) First Spell	-0.67*	0.02	-1.37**	0.00
(2) Benefit Year	-0.36	0.19	-1.15**	0.00
Proportion of Claimants who:				
	<u>Difference of Props.</u>	<u>Probability Value</u>	<u>Difference of Props.</u>	<u>Probability Value</u>
(1) Exhausted Benefits	-0.014	0.19	-0.032**	0.00
(2) Ended Benefits ≤ 11 Weeks	+0.031**	0.00	+0.055**	0.00

SOURCE: Calculations based on Table 5-3. See notes to Table 5-3.

NOTES: Two asterisks (**) denote rejection of the hypothesis that the difference of means is zero using a two-tailed 99-percent confidence test; one asterisk (*) denotes rejection of the hypothesis that the difference of means is zero using a two-tailed 95-percent confidence test. The probability value gives the probability that the difference between the experimental and control groups equals zero.

TABLE 5-5

Mean Pre- and Post-Program Earnings of Eligible Claimants
with Earnings in Quarter after Benefit Termination,
By Experimental Group

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

NOTES: Standard error of mean 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, 2,550 HIE enrollees, and 2,786 JSIE 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 pre-claim quarter. Note also that because the most recent earnings available to us are for 1985:III, we do not have a quarter of earnings after benefit termination for about one-third of our sample.

TABLE 5-6

Use of Employment Service by Fully Eligible Claimants

Number of Referrals Received	Number of Claimants		
	Control	HIE	JSIE
1	324	293	354
2	99	85	100
3	36	33	26
4	12	12	14
5	10	4	4
6	4	5	5
7	1	1	1
8	0	4	1
9	0	1	0
10	0	1	2
11	0	0	0
12	0	0	1
13	1	0	1
	487	439	509
Total			
Sample Size	3,952	3,963	4,186
Proportion of Sample Receiving Referrals	0.123	0.111	0.122

SOURCE; Illinois Department of Employment Security, Employment Service Automated Retrieval System.

NOTES; The sample includes only claimants who were monetarily and nonmonetarily eligible for UI benefits and who met the initial claim and age restrictions of the experiments.

APPENDIX A5

APPENDIX TO CHAPTER 5: OUTCOMES BY PARTICIPATION CATEGORY

In this Appendix, we present data we have compiled on experimental outcomes by participation category--that is, for those who agreed to participate, refused to participate, submitted a Notice of Hire, and received a bonus. These data are intended to serve mainly as a further check on the plausibility of the results presented in the body of this chapter.

Table A5-1 shows figures on three experimental outcomes by category of participation. The outcomes shown are mean benefits paid over the full benefit year, mean weeks of benefits over the full benefit year, and the proportion of claimants who terminated benefits within 11 weeks of filing their initial claim. Four participation categories are shown in Table A5-1: agreement to participate, refusal to participate, submission of a Notice of Hire, and receipt of a bonus. The mean of each variable for the control group is also shown.

Table A5-1 shows that, whereas 20.1 weeks of state regular benefits were received by the average control group member over the full benefit year (see the middle two columns under "Weeks of Benefits"), 7.2 weeks of benefits were received by the average JSIE enrollee who submitted a Notice of Hire, and 5.9 weeks of benefits were received by the average JSIE bonus recipient. The difference between the control mean and the means for those who submitted Notices of Hire or received a bonus--about 13 or 14 weeks--should not be regarded as an effect of the JSIE on these participants. Because it is likely that JSIE enrollees who submitted a Notice of Hire or received a bonus would have experienced fewer than average weeks of insured unemployment even without the incentive provided by the bonus, the 13- to 14-week difference is the sum of a treatment effect and a self-selection effect (that is, there are differences between these JSIE enrollees and the average JSIE enrollee). The figures do describe, however, the experience of different categories of participant.

Table A5-2 offers additional evidence on earnings after reemployment. This table shows mean earnings of HIE and JSIE enrollees in the quarter after benefit termination by category of participation. (Note that this measure of post-reemployment earnings is the same as that used in the bottom row of Table 5-6.) Four participation categories are again shown in Table A5-2: agreement to participate, refusal to participate, submission of a Notice of Hire, and receipt of a bonus.

The figures in Table A5-2 buttress the findings of Table 5-6 in showing no negative relationship between the JSIE (or the HIE) and earnings after reemployment. Indeed, those who submitted a Notice of Hire or received a bonus had significantly higher earnings in the quarter after benefit termination than did either controls or the full group of JSIE (or HIE) enrollees. Because

there may be self-selection in the submission of a Notice of Hire (or in bonus receipt), the finding that JSIE participants who submitted a Notice of Hire (or received a bonus) had higher reemployment earnings should not be interpreted as a causal effect of the experiment. Nevertheless, the data are consistent with the hypothesis that JSIE enrollees did not shorten their spells of unemployment by accepting low-paying jobs that were readily available.

TABLE A5-1

Experimental Outcomes by Participation Category
(Mean with Standard Error in Parentheses)

Level of Participation	Benefits Paid (\$)		Weeks of Benefits		Proportion < 11 weeks	
	HIE	JSIE	HIE	JSIE	HIE	JSIE
Agreed to Participate (2,586;3,527)	2,347.9 (33.4)	2,307.9 (28.6)	19.669 (0.240)	18.711 (0.205)	0.384 (0.009)	0.416 (0.008)
Refused to Participate (1,377;659)	2,571.4 (45.8)	2,436.0 (66.2)	19.747 (0.328)	19.979 (0.475)	0.374 (0.013)	0.366 (0.019)
Submitted Notice of Hire (199;765)	1,022.2 (116.8)	984.2 (59.6)	7.638 (0.825)	7.220 (0.421)	0.944 (0.031)	0.901 (0.016)
Received \$500 Bonus (112;570)	652.9 (156.3)	817.0 (69.3)	5.071 (1.107)	5.881 (0.491)	0.991 (0.044)	0.974 (0.019)
Control Mean	2,486.5 (27.0)		20.060 (0.194)		0.353 (0.008)	

NOTES: Means and proportions are computed over the sample described in the notes to table 5-1. "Benefits Paid" are state regular benefits paid over the full benefit year. "Weeks of Benefits" are the number of weeks of benefits paid over the full benefit year. "Proportion < 11 weeks" refers to the proportion of claimants who terminated benefits within 11 weeks of filing their initial claim. The number of observations in the HIE and JSIE groups are shown for each participation category in parentheses in the first column.

TABLE A5-2

Mean Earnings in Quarter after Benefit Termination
by Participation Category

<u>Level of Participation</u>	<u>Dollar Earnings in Quarter after Benefit Termination</u>	
	<u>HIE</u>	<u>JSIE</u>
Agreed to Participate (1,677;2,343)	2,857.1 (58.6)	3,145.0 (49.6)
Refused to Participate (873;444)	3,467.0 (81.2)	3,044.8 (113.9)
Submitted Notice of Hire (177;667)	3,596.0 (179.2)	4,014.4 (91.7)
Received \$500 Bonus (101;499)	3,976.2 (236.9)	4,292.6 (106.6)
Control Mean	3,120.7 (47.3)	

NOTES: Standard error of mean in parentheses.

See notes to Table A5-1.

Chapter 6

EXPERIMENTAL EFFECTS: DISAGGREGATED RESULTS

The evidence presented in Chapter 5 suggests that, overall, the Job Search Incentive Experiment (JSIE) curtailed the number of weeks of insured unemployment of those who were enrolled in it by more than one week (on average, over the full benefit year). By contrast, the Hiring Incentive Experiment (HIE) appears to have had no statistically significant effect, overall, on the number of weeks of insured unemployment or on benefits received over the full benefit year.

The overall results in Chapter 5 measure the net effect of the experiments on all eligible claimants. But it is clear from Table 5-2 that those who enrolled in the experiments compose a diverse group, and it is possible that certain subgroups of enrollees were more strongly affected by the experimental treatments than were others. In particular, it turns out that the overall results of the HIE--which suggest that the HIE had no affect on weeks of insured unemployment or benefits received over the full benefit year--mask effects of the experiment on important subgroups of the population. That is, although some groups were affected by the HIE, the overall results are diluted by inclusion of groups of claimants who were unaffected by the HIE.

The goal of this chapter is to explore in detail the effects of the HIE and JSIE on various subgroups of the population. In section I, we outline some problems that arise in making experimental comparisons for various subgroups. In section II, we discuss experimental effects by characteristics of the claimant--age, race, sex, education, and occupation. In section III, we examine experimental effects by the labor market experience of claimants during the base period (such as base period earnings and the variability of those earnings). Section IV differs from the earlier sections by focusing on claimants who became reemployed. Here we ask whether the effects of the HIE and JSIE depended on the industry in which a claimant found new employment, or on the characteristics of the hiring employer. Section V brings together the results of the earlier sections and offers some conclusions.

I. Potential Problems of Inferring Experimental Effects for Subgroups

Because the random assignment of claimants to the HIE, JSIE, and control groups appears to have been successful, it is in principle correct to extend the method used to determine experimental impacts in Chapter 5--that is, the comparison of group means--to an examination of the effects of the experiments on various subgroups. For example, if we wanted to know whether men and women both experienced reductions in insured unemployment

under the JSIE, then we would compare the average number of weeks of insured unemployment--call this variable "WEEKS"--for men in the JSIE group with WEEKS for men in the control group. This comparison will yield the effect of the JSIE on WEEKS for men. Similarly, we would compare WEEKS for women in the JSIE group with WEEKS for women in the control group to obtain the effect of the JSIE on WEEKS for women.

We want to guard against two possible problems, however. Comparing experimental means with control means is a satisfactory method of inferring experimental effects if we have a large sample and if experimental and control groups being compared are in fact comparable random samples. But in examining subgroups we are by definition restricting our attention to smaller samples, which means it is more difficult to detect a given experimental effect. Further, as sample size diminishes, it becomes increasingly likely that these smaller samples will differ systematically, despite randomization in the overall assignment of claimants to control and experimental groups. The first problem--small size of subsamples--can only be solved definitively by conducting a larger experiment. The second problem--differences between experimental and control groups that emerge when subsample sizes are small--may be overcome (if it exists) by computing regression-adjusted experimental effects. We treat these two matters in turn.

A. Limitations Imposed by Small Subsample Size

Certain subgroups of claimants, hispanics for example, may be so small that a comparison of experimental enrollees with controls would reveal a statistically significant experimental effect only if the experimental effect on this small group were enormous. The issue of sample size and the detection of an experimental effect has been touched on already (see Chapter 2, section III.A.6), but it is worth reviewing in the present context.

How small can a subsample become before we will be unable to detect an experimental effect of reasonable magnitude? In general, the answer depends on three factors: what we are willing to view as an effect of reasonable magnitude, how certain we want to be that an effect we observe is a true effect, and the variability of the underlying difference between the experimental and control groups.

To take an example, we might be interested in determining for a small group (such as hispanics) whether there is a difference between the control group and the JSIE group in the mean number of

1. For general discussions of sample size requirements, see, for example, Morris Hamburg, Statistical Analysis for Decision Making, second edition (New York: Harcourt, Brace, Jovanovich, 1977), pp. 249-255 and 286-297; and Lawrence Lapin, Statistics for Modern Business Decisions, second edition (New York: Harcourt, Brace, Jovanovich, 1978), pp. 256-270, 468-471, and 490-493.

weeks of insured unemployment experienced during the full benefit year. The number of claimants we must observe in the control and JSIE groups combined (n) depends on three factors: First, it depends on the size of the difference between the two groups that we are trying to detect. (Call this difference e for experimental effect, and express it as a difference of means--say, 1 for a one week difference between the control and JSIE groups.) Second, it depends on the confidence level we choose. (This is expressed as a normal deviate or z -value, z .) And third, it depends on the standard error of the difference in means between the control and JSIE groups. (This standard error, which we will call s_d (for standard error of the difference), equals the square root of $[2s^2/n]$, where s is the estimated standard deviation of the number of weeks of insured unemployment experienced by claimants. This formula incorporates two simplifying assumptions: that the standard deviation of weeks of insured unemployment is the same for controls as for JSIE enrollees, and that the control and JSIE groups are of the same size.) It turns out that the relationship between the required sample size (n) and these three factors can be expressed as:

$$n = [2(s_d^2)(z^2)]/e^2, \quad (6.1)$$

where, again, n is the required sample size, z expresses the chosen level of confidence, s_d equals the standard error of the difference of means between the control and JSIE groups, and e is the difference in mean weeks of unemployment between the control and JSIE groups that we are trying to detect.²

Applying equation (6.1) to some specific cases yields results that are useful to interpreting the results discussed in sections II through IV. The following table shows the required sample sizes needed to detect an experimental effect of either 1 week or of one-half week with a confidence level of either 95 percent or 99 percent, as derived from equation (6.1):³

2. The relationship is derived by substituting the standard error of the difference between two means (s_d , which is the square root of $[2s^2/n]$) into the definitional relationship between the chosen confidence level (z), the size of the experimental effect we are trying to detect (e), and the standard error (s_d), which is $z = e/s_d$. The result of making the substitution and solving for n is equation (1) in the text.

3. The standard deviation of weeks of insured unemployment (s) used in the calculations is 12.25.

Confidence Level Desired	Size of Effect (difference in weeks)	
	1 week	0.5 week
95 percent ($z = 1.96$)	1,153	4,610
99 percent ($z = 2.57$)	1,982	7,926

The table shows that observing a statistically significant effect of one-half week requires a sample that is four times as large as that required to observe an effect of 1 week. Also, the required sample size⁴ increases as we demand a higher level of confidence.

We conclude that we may observe experimental effects of the same magnitude as were seen in the JSIE at the 99-percent significance level with a subsample as small as about 2,000 (that is, about 1,000 controls and about 1,000 experimental enrollees, either HIE or JSIE). If we are willing to accept the 95-percent confidence level, subsamples of about 1,200 (600 controls and 600 experimental enrollees) will suffice. These results suggest the usefulness of disaggregation because, with a total of roughly 4,000 claimants in each of the three groups, as many as six subgroups of equal size could be analyzed, and statistically significant effects (at the 95-percent confidence level) of reasonable magnitude would be detectable. Smaller effects, however, will be difficult to observe: Even a two-way grouping of claimants (into women and men, for example) would result in subsamples that are barely large enough to detect a significant effect of one-half week at the 95-percent confidence level. Therefore, in analyzing the disaggregations that follow, we should only expect to highlight the groups of claimants who showed the strongest response to the experimental incentives. Many subgroups that may have shown some (relatively weak) response to experimental incentives⁵ will appear to have shown no statistically significant response.

4. The exercise could be repeated for other variables of interest--proportion of claimants who were rehired within 11 weeks, or state-regular benefits received by claimants during the benefit year, for example. Carrying out such exercises yields results similar to those shown in the table.

5. It is also worth noting that, in evaluating results from a large number of subsamples, one must expect the occasional appearance of spuriously significant results. That is, if an experimental effect is associated with a probability value of 0.05 (so that it is "significant at the 95 percent level of confidence"), there is a 5 percent chance that the difference is in fact zero. It follows that one should view all statistical results critically and in context.

B. Computation of Regression-Adjusted Experimental Effects

To verify the accuracy of the comparisons between experimental group means and control group means, we have computed regression-adjusted experimental effects. Computing regression-adjusted experimental effects entails estimating a linear equation by ordinary least-squares (OLS, or "regression") in which an experimental outcome such as weeks of insured unemployment (WEEKS) is regressed on experimental status (HIE or JSIE), on variables that "fully interact" the experimental treatment variables (HIE and JSIE) with the variables that capture the characteristic that is of interest (for example, sex, as discussed above), and on variables that control for other characteristics that might affect WEEKS. For example, if we were interested in how experimental status affected the number of weeks of insured unemployment experienced by men and women in the HIE, JSIE, and control groups, we would estimate the following equation:

$$\begin{aligned} \text{WEEKS}_i = & a_0 + a_1 \text{HIE}_i + a_2 \text{JSIE}_i + a_3 \text{SEX}_i + \\ & a_4 (\text{HIE}_i)(\text{SEX}_i) + a_5 (\text{JSIE}_i)(\text{SEX}_i) + \\ & b_1 X_{1i} + b_2 X_{2i} + \dots + b_K X_{Ki} + e_i. \end{aligned} \quad (6.2)$$

In this equation, WEEKS_i is the number of weeks of insured unemployment of claimant i , HIE_i equals 1 if the claimant was enrolled in the HIE and 0 otherwise, JSIE_i equals 1 if the claimant was enrolled in the JSIE and 0 otherwise, SEX_i equals 1 if the claimant was male and 0 if female, X_{1i} through X_{Ki} are variables that control for the age, race, household status, and pre-unemployment earnings of the claimant, the a 's and b 's are linear coefficients estimated by OLS, and e_i is a random error term.

The HIE and JSIE effects may be retrieved from equation (6.2) as follows. There are six groups that are of interest--women and men in the control group, women and men in the HIE group, and women and men in the JSIE group. The mean WEEKS for each of these groups (adjusting for other characteristics) may be obtained from the regression coefficients by noting that:

$$\text{Mean WEEKS for control women} = a_0;$$

$$\text{Mean WEEKS for control men} = a_0 + a_3;$$

$$\text{Mean WEEKS for HIE women} = a_0 + a_1;$$

$$\text{Mean WEEKS for HIE men} = a_0 + a_1 + a_3 + a_4;$$

$$\text{Mean WEEKS for JSIE women} = a_0 + a_2;$$

$$\text{Mean WEEKS for JSIE men} = a_0 + a_2 + a_3 + a_5.$$

All these means are adjusted in the sense that they control for the characteristics of claimants represented by the X_k variables. It follows that the effect of the HIE on women equals a_1 (the difference between mean WEEKS for HIE women and mean WEEKS for control women), and that the effect of the JSIE on women equals a_3 (the difference between mean WEEKS for JSIE women and control women). For men, the HIE effect is similarly computed as $(a_1 + a_4)$, and the JSIE effect as $(a_3 + a_5)$. Note that the approach⁶ illustrated here for regression-adjusted experimental effects by sex can be extended to examine experimental effects by any other characteristic of interest.

The regression-adjusted treatment effects will differ significantly from the treatment effects derived by simple comparison of experimental group means with control means only if there are systematic differences in the distribution of some characteristic of claimants assigned to the HIE, JSIE, and control groups. In other words, if assignment to the HIE or JSIE were related to some individual characteristic such as sex or race, then regression adjustment would be required to obtain proper estimates of the experimental impact. Failure to "adjust" or "control" for sex or race would result in a biased estimate of the experimental effect by attributing an effect to the experimental treatment that might in fact be due to race or sex.

The question we face is whether claimants with one or another characteristic are more or less likely to turn up in the HIE or JSIE groups. We have already concluded (see Chapter 5, Table 5-2) that, for the overall samples, the answer is no. But for subsamples, it is possible that certain claimants with certain characteristics are disproportionately present in one of the experimental groups, and that regression adjustment will be required.

It turns out that whether the regression-adjusted treatment effects differ from the simple treatment effects depends on the variable by which we disaggregate. When we break down the experimental effects by characteristics of the claimant or by the claimant's labor market experience in the base period, there are negligible differences between the regression-adjusted treatment effects and the simple treatment effects. (These are the disaggregated treatment effects that are reported in sections II and III below.) Thus, the random assignment appears to have been

6. A general discussion of dummy-variable models of this kind is included in J. Johnston, Econometric Methods, third edition (New York: McGraw-Hill, 1984), pp. 225-233.

successful to the extent that, when looking at experimental effects by age, race, sex, and so on, regression adjustment is unnecessary.

On the other hand, when we break down the experimental effects by variables that were potentially correlated with experimental treatments--such as industry of reemployment or characteristics of the hiring employer--differences between the regression-adjusted and simple treatment effects do arise. (These are the disaggregated treatment effects that are reported in section IV below.) This makes sense: Employers in industries that employ particular kinds of workers may have been most prone to take advantage of the HIE; or workers in certain industries, who in turn have particular characteristics, may have been most prone to take advantage of the JSIE. It follows that in appraising the effects of the HIE or JSIE broken down by industry of reemployment (or by characteristics of the hiring employer) we should control for the characteristics that vary across the HIE, JSIE, and control subgroups. Thus, in section VI below, we rely on regression adjustment to infer the effects of the HIE and JSIE by industry of reemployment and by characteristics of hiring employers.

II. Experimental Effects by Age, Race, Sex, Education, and Occupation of Claimant

With these possible limitations in mind, we examine the effects of the experiments, broken down by various subgroups of claimants. We address first the variations in experimental effects by demographic, educational, and occupational characteristics of claimants.

A. Experimental Effects by Age

Table 6-1 displays the effects of the HIE and JSIE on three outcomes: (a) state-regular benefits paid to claimants during their full benefit year; (b) the number of weeks of benefits paid to claimants during the full benefit year; and (c) the proportion of claimants who were reemployed within 11 weeks of filing the initial claim. These effects are broken down by seven age groups.

Each figure in the table that is not in parentheses represents an experimental effect--that is, the difference between the mean value for the control group and the mean value for either the HIE or JSIE group. For example, the figure -80.7 under "Benefits Paid" to HIE claimants aged 20 to 24 implies that the average state-regular benefit payments to HIE enrollees aged 20 to 24 was \$80.70 less than the average state-regular benefit payments to controls aged 20 to 24. Figures in parentheses under each experimental effect are probability values indicating the probability that the experimental effect is zero. The probability

value for the HIE effect on benefits paid to claimants aged 20 to 24 (0.34) indicates that there is a 34 percent chance that the \$80.70 difference is statistically zero. Since we usually impose a confidence level of 95 percent or greater, we will usually view probability values greater than 0.05 as indicating no statistically significant experimental effect. (Occasionally, when dealing with small samples, it may be sensible to relax this standard to the 90-percent confidence level, and to view probability values between 0.05 and 0.10 as indicating a significant effect.)

The figures in Table 6-1 suggest that the HIE had a strong and statistically significant effect on at least one subgroup of claimants--those aged 30 to 34. This group stands out clearly from the other age groups, with a large reduction in benefit receipt and weeks of insured unemployment, as well as a marked increase in hiring within 11 weeks of filing the initial claim. There is no obvious reason why this age group should have experienced an HIE effect, whereas those in neighboring age groups were unaffected. The HIE's effect on the 30 to 34 age group needs to be considered in concert with HIE effects on other identifiable subgroups, in order for a sensible explanation to emerge. Nevertheless, the result illustrates the importance of disaggregating the effects of the HIE, because it suggests that at least one identifiable group of claimants was influenced by the program.

The figures in Table 6-1 also suggest that the JSIE had a greater impact on claimants under age 35 than on older claimants. JSIE enrollees under age 35 received fewer benefits and experienced fewer weeks of insured unemployment than did controls, and a significantly higher proportion of the JSIE enrollees under 35 were reemployed within 11 weeks. Although JSIE claimants between the ages of 35 and 50 may have experienced similar effects, those effects were statistically less significant, although this may be in part the result of the smaller number of older claimants in the sample. It is clear that the JSIE did not affect claimants over the age of 50.

Table 6-2 displays the regression-adjusted effects of the HIE and JSIE by age category. These regression-adjusted effects were computed as outlined in section I.B above (see also the notes to Table 6-2). In calculating these effects, we have controlled for nonrandom variation among the control, HIE, and JSIE groups in the following variables: race, sex, presence of dependents, and base period earnings. If there were no such variation among the three groups, then the results of Table 6-1 and Table 6-2 would be the same.

In fact, the HIE and JSIE effects shown in Table 6-2 are remarkably similar to those shown in Table 6-1. Although there are minor differences in some of the point estimates of the experimental effects, the direction, general magnitude, and statistical significance of the effects are unchanged.

That the regression-adjusted experimental effects shown in Table 6-2 yield results very close to the unadjusted results of Table 6-1 reinforces our confidence that the assignment procedure used during enrollment was truly random. Indeed, we have computed regression-adjusted experimental effects by other characteristics of claimants and obtained the same result--the regression-adjusted effects differ negligibly from the unadjusted results. Accordingly, in the rest of section II₇ and throughout section III, we report only the unadjusted results.

Results presented in Table 5-5 suggested that the earnings of claimants after they became reemployed were unaffected by the HIE or JSIE. They suggest, for example, that even though JSIE enrollees found a new job more quickly than did controls, they did not accept a lower-paying job simply to obtain the \$500 JSIE bonus. It is important to appraise whether the same is true when we break down the experimental effects by age group. Table 6-3 shows the difference between the reemployment earnings of controls and HIE enrollees, and between controls and JSIE enrollees, for each of the seven age groups we are considering. The figures are based on the subsample of claimants who stopped receiving benefits (at some time after making the initial claim that brought them into the experiment), and had earnings₈ in the first full quarter after they stopped receiving benefits.

Ideally we would like to know the hourly wage earned by claimants once they have found new employment--this would indicate most clearly whether a claimant had accepted an inferior job merely to qualify for a \$500 bonus. Available data provide us with no measure of hourly wage, but earnings in the first full quarter after benefit termination can serve as a reasonable proxy.

Table 6-3 displays differences (by age group) between experimental and control group earnings in the first full quarter after claimants stopped receiving UI benefits. (The same measure is used to compare all controls with all experimental enrollees in the third row of Table 5-5.) For the HIE, Table 6-3 shows that no age group suffered reduced earnings after reemployment, including the 30 to 34 year-olds who showed significant reductions in benefits and duration of insured unemployment.

For the JSIE, the most striking result is that no age group experienced significantly reduced earnings after reemployment.

7. For reasons stated in section I.B, we do use regression-adjusted results in section III below when we examine experimental effects by industry of reemployment and by employer characteristics.

8. Claimants who exhausted benefits and failed to find new employment, and claimants who dropped out of the labor force, are not in the sample.

The 50 to 54 year-olds experienced a positive effect on earnings after reemployment, although we regard this as a spurious effect since the JSIE affected neither benefits nor weeks of unemployment for this group. The rather large--albeit statistically insignificant--earnings reductions associated with JSIE claimants aged 40 to 44 and 45 to 49 might be a concern except that, these same claimants were among the least affected by the JSIE. In fact, the JSIE enrollees who reduced their benefit receipts and weeks of insured unemployment most (those under age 35) showed the least sign of any earnings loss.

B. Experimental Effects by Race

Table 6-4 shows the effects of the HIE and JSIE broken down by five race and ethnicity categories: white, black, hispanic, Native American, and other. Since nearly two-thirds of the total sample is classified as white, the overall results of Chapter 5 were dominated by the program effects on whites, shown in the first row of Table 6-4.

Note first that, as a rule, the effects of the HIE on each racial and ethnic group are insignificant at the 95-percent confidence level. (The only exception is the HIE's effect on the proportion of white claimants who returned to work within 11 weeks.) There is some hint that whites may have been more affected by the HIE than others. We show below that white women were significantly affected by the HIE, whereas all men and black women were not. It seems likely that the response of white women underlies whatever difference between whites and others appears here. Small sample size prevents us from drawing inferences about the effects of the HIE on racial or ethnic groups other than whites and blacks.

The effect of the JSIE does appear to have varied by race. The JSIE had a large and statistically significant effect on the benefits and weeks of unemployment of whites. In contrast, the effect of the JSIE on blacks' benefits and weeks of unemployment was smaller and statistically less significant. The samples of hispanics and Native Americans are too small to permit firm conclusions, but the JSIE may have affected these groups in roughly the same way that it affected whites. The only true

9. This result (that claimants aged 40 to 49 responded weakly to the experiment and hinted of a reemployment earnings loss) could be interpreted as showing that the returns to job search increase with age. That is, for younger workers, job search is less productive because skills possessed by younger workers tend to be more general. For older and more experienced workers, job search is more productive because skills tend to be more specific. If this interpretation is correct, then it makes sense that older workers responded weakly to the JSIE, for had they cut short their job search, they would have been harmed. Also, it follows that the JSIE bonus is used most by those groups who will benefit from it without experiencing any offsetting cost.

aberration in the table is in the "Other" row, which suggests that both the HIE and JSIE may have had a perverse effect on the experience of claimants categorized as "other"--chiefly Asians. But the "other" sample is too small to allow firm conclusions.

Table 6-5 shows that neither the HIE nor the JSIE served to reduce the post-experimental earnings of claimants in any racial category. The negative figures shown for the post-experimental earnings of blacks who were enrolled in either the HIE or JSIE are statistically insignificant. Moreover, blacks showed no significant response to either the HIE or JSIE, so it would be incorrect to attribute any earnings reduction to either experiment.

C. Experimental Effects by Sex

Table 6-6 displays the breakdown of HIE and JSIE effects by sex. These results are striking. There is virtually no difference between women and men in their response to the JSIE--both sexes showed similar responses as measured by benefits received, weeks of insured unemployment, and proportion of claimants rehired within 11 weeks of filing the initial claim.

But the difference between men and women in their response to the HIE is stark. Whereas there appears to have been no significant effect of the HIE on men, the HIE resulted in fewer benefits paid to women, fewer weeks of insured unemployment for women (this effect is nearly significant at the 95-percent confidence level), and a significantly higher proportion of women being rehired within 11 weeks of filing the initial claim.

Furthermore, Table 6-7 shows clearly that the response of women to the HIE did not entail acceptance of lower earnings. Table 6-7 also shows that the response of claimants--both women and men--to the JSIE was not accompanied by lower earnings after reemployment.

D. Experimental Effects by Both Race and Sex

Table 6-8 displays the breakdown of experimental effects by race and sex combined. These results are dramatic. When white women are singled out, the effects of the HIE are unambiguously statistically significant. This we view as a strong result, attained as it is over a relatively small sample. Moreover, Table 6-9 shows that no loss of earnings accompanied the response of women to the HIE.

Table 6-8 also shows that the JSIE affected white women and men about equally. But there may have been a difference between black women and men in the JSIE's effects. The change in proportion of black women who terminated benefits within 11 weeks is much higher for black women than for black men, and the change for black women is statistically significant. Further, black women show nearly twice as great a reduction in weeks of benefits

as black men, although neither reduction is statistically significant. The sample sizes for subgroups other than whites and blacks are too small to allow detection of statistically significant results.

Table 6-8 shows also that the reemployment earnings of black men who were enrolled in the HIE or JSIE may have been lower than the earnings of black controls. Although the differences are statistically insignificant, the point estimates are fairly large. Because neither the HIE nor the JSIE appears to have affected black men's benefits or weeks of unemployment (see Table 6-8), we attribute the lower reemployment earnings of black HIE and JSIE enrollees to random variation.

E. Experimental Effects by Education

We can observe the number of years of schooling a claimant completed only if the claimant registered with the Job Service and was recorded in the ESARS data base (see Chapter 4). Hence, the subsamples of claimants broken down by level of education are smaller than would otherwise be the case. Specifically, we observe the number of years of completed schooling of only 10,355 claimants, whereas the sample we have been working with to this point has comprised 12,101 claimants. Nevertheless, some useful inferences can be made from Table 6-10, which displays experimental effects by five categories of education.

For the HIE, no particular educational group showed an experimental response at conventional significance levels, although the responses for those with some high school (but less than a diploma) are larger and closer to statistical significance than are the responses for any other category.

The effects of the JSIE differed substantially by educational category. It appears that the JSIE had the largest and most easily perceived effect on high-school graduates, who happen to be the largest educational subgroup. Claimants with some high school or some college may have responded to the JSIE, but the results suggest that their response was weaker. (Sample size is not a problem with these subgroups--the subsamples are large enough that we should be able to discern an effect if it were present.) College graduates exhibit a still weaker response to the JSIE, although here the small sample size begins to hamper inference. There are too few claimants with less than 8 years of education to draw any conclusions about these claimants. For the JSIE, then, the strongest effects were on high-school graduates, and those with either less or more education showed weaker responses.

Accompanying the strong effect of the JSIE on high-school graduates is a possible loss of earnings after reemployment, shown in Table 6-11. Although the difference in earnings between high-school graduates who were enrolled in the JSIE and those who were controls was \$140.70, this difference is significant at only the 85 percent level. Therefore we are uncertain whether the JSIE

really affected the reemployment earnings of JSIE enrollees who were high-school graduates.

F. Experimental Effects by Occupation

Table 6-12 displays the effects of the HIE and JSIE broken down by seven occupational categories. Since each of these categories represents an aggregation of diverse occupations with various levels of skill, experience, and training, results based on these groupings must be interpreted with caution.¹⁰

Claimants who were in Clerical and Sales occupations seem to have been affected by the HIE. Benefits paid to HIE enrollees in Clerical and Sales occupations were significantly lower than benefits paid to controls, and 6.3 percent more of these HIE claimants were rehired within 11 weeks than were similar controls. It is fairly clear that Professional, Technical, and Managerial workers, as well as workers in Machine Trades, Processing, Agriculture, and the Miscellaneous categories did not reduce their benefits receipts or duration of unemployment in response to the HIE. For the other occupational categories, the effects of the HIE are ambiguous.

The JSIE affected more occupational groups than did the HIE. Claimants whose occupations were in Clerical and Sales, Machine Trades and Processing, and Packaging and Materials Handling, showed strong responses to the JSIE. Service workers and those in Structural Work and Benchwork may also have responded to the JSIE, but those in Professional, Technical, and Managerial occupations showed no statistically significant response to the JSIE. This suggests an inverse relation between occupational skill requirements and the impact of the JSIE, since Clerical, Sales, Packaging, and Materials Handling occupations require relatively low training and skill levels on average, whereas Professional, Technical, and Managerial occupations have higher training and skill requirements.

Table 6-13 shows that the only statistically significant experimental effect on earnings after reemployment was for

10. Data on the occupation of claimants comes from the ESARS data base, and hence is available only for those claimants who are Job Service registrants. The seven categories were chosen in an effort to create meaningful occupational groups, while maintaining subgroups large enough to allow detection of reasonable experimental effects. The "Professional, Technical, and Managerial" category comprises Dictionary of Occupational Titles (DOT) occupational categories 0 and 1; "Clerical and Sales" is DOT category 2; "Service" is DOT category 3; "Machine Trades, Processing" comprises DOT categories 5 and 6; "Structural Work, Benchwork" comprises DOT categories 7 and 8; "Packaging and Materials Handling" is DOT occupational division 92; and "Miscellaneous and Agriculture" comprises DOT categories 4 and 9 (except occupational division 92).

claimants in Structural Work and Benchwork who were enrolled in the JSIE. Since this group showed neither significant nor consistent response to the JSIE, we interpret this negative effect on earnings as spurious.

III. Experimental Effects by Labor Market Experience in the Base Period

In addition to examining whether the effects of the HIE and JSIE varied with demographic characteristics such as age, race, and sex, it is useful to examine variation in the programs' impacts by the following additional variables: Average quarterly earnings in the base period, variability of quarterly earnings during the base period, and weekly benefit amount. We consider each in turn.

A. Experimental Effects by Average Quarterly Earnings in the Base Period

Table 6-14 displays experimental effects broken down by six categories of claimants' earnings before they filed for UI benefits. The measure of earnings we use is the average quarterly earnings in the base period (which in Illinois is the first four of the last five completed quarters before filing the initial claim).

The figures in Table 6-14 reveal no clearly significant effect of the HIE on any particular earnings group. Nevertheless, one group tends to stand out. Claimants whose quarterly earnings averaged \$1,000 to \$1,999 in the base period exhibited a larger response to the HIE, and an effect that is much closer to being statistically significant, than any other group. This finding accords with the finding reported previously that the HIE had a greater effect on claimants in Clerical and Sales occupations, which offer relatively low pay.

Table 6-14 shows that the JSIE was least effective among claimants who had especially low or high earnings while they were employed. But for claimants whose earnings averaged \$2,000 to \$9,000 per quarter, the JSIE consistently reduced benefits received and weeks of insured unemployment. Thus, the JSIE appears to have been most effective in reducing weeks of insured unemployment for claimants in the middle categories.

Table 6-15 shows the experimental effects on earnings after reemployment, with the effects broken down by the same earnings categories as in Table 6-14. The figures offer no strong evidence that earnings after reemployment were harmed by either the HIE or the JSIE. The significant earnings losses apparently experienced by both HIE and JSIE enrollees whose earnings were over \$9,000 should be wholly discounted, for these same claimants actually

responded perversely to the treatments (see the bottom row of Table 6-14).

B. Experimental Effects by Variability of Earnings in the Base Period

Were claimants with more stable employment during the base period more likely to be helped by either the HIE or JSIE? Or were those with less stable employment more likely to be affected by the experiments? To answer these questions, we examine the effects of the experiments by the variability of earnings in the the base period. Earnings variability should serve as a measure of the stability of employment before the spell of insured unemployment that we are examining. Our measure of earnings variability is the coefficient of variation of quarterly earnings in the base period.¹¹ Table 6-16 divides the sample into five groups based on the variability of their base period earnings, and shows how the effects of the HIE and JSIE differed with the degree to which earnings in the base period varied.

The most striking result shown in Table 6-16 is that the HIE had a large and statistically significant effect on claimants in the next-to-lowest earnings variability category (0.1 to 0.2). Why claimants in the next-to-lowest variability category should be so affected by the HIE, while those in the lowest category are not, poses a puzzle. It suggests, though, that those affected by the HIE worked throughout the year and had earnings that varied only mildly. This minor variability in earnings could be the result of the mild seasonal fluctuations in hours of work that characterize part-time employment.

Regarding the effects of the JSIE by earnings variability, it is clear that claimants who experienced highly variable earnings during the base period were relatively unaffected by the program. In contrast, claimants with more stable earnings in the base period (that is, with coefficient of variation less than 0.6) responded strongly to the JSIE. This finding suggests that claimants in highly seasonal industries, and claimants who are

11. The coefficient of variation (CV) for any variable, is the estimated sample standard deviation (s) divided by the sample mean (\bar{X}). We calculated the CV of base period earnings for each claimant by computing the difference between earnings in each quarter and the claimant's base period average, squaring this difference and summing the differences obtained for each of the four quarters. (We also divided this sum of squared differences by 3, which is the number of observations we have of each claimant (4) minus 1. This adjusts for the loss of one degree of freedom in computing the CV.) The square root of the sum of squared deviations equals the sample standard deviation (s), and dividing the estimated standard deviation by average base period earnings (\bar{X}) yields the CV. See Hamburg, cited previously, pp. 39-40.

frequent job changers with (perhaps) a relatively weak attachment to the labor force, were the least likely to respond to the JSIE.

Table 6-17 shows the effects of the HIE and JSIE on reemployment earnings. The results for the HIE suggest that the claimants who shortened their spell of unemployment in response to the HIE--those with earnings variability in the base period of 0.1 to 0.2--may have accepted lower-paying jobs as well. Further, the figures indicate that one of the three groups who responded to the JSIE--again those with earnings variability in the base period of 0.1 to 0.2--accepted lower-paying jobs as part of their response to the JSIE. Note that the other two groups affected by the JSIE--those with earnings variability of 0.0 to 0.1 and from 0.2 to 0.6--did not accept lower-paying jobs in the process of shortening their spell of unemployment. Hence, there is no discernible relationship between the JSIE's effect on the number of weeks of unemployment experienced by a group of claimants and its effect on the reemployment earnings of the same group. Although we have uncovered one case in which the JSIE both reduced the number of weeks of unemployment and reduced reemployment earnings, this is by no means a pattern.

C. Experimental Effects by Weekly Benefit Amount

Table 6-18 decomposes the effects of the HIE and JSIE by Weekly Benefit Amount (WBA) received by claimants. Since the WBA is a function of base period earnings, it should come as no surprise that Table 6-18's results are consistent with the results found in examining the experimental effects by base period earnings (Table 6-14).

For example, if the HIE had an effect on any of these groups, it was on claimants who received a WBA of \$52 to \$90, which is relatively low but above the minimum of \$51 per week. This accords with the finding that the HIE may have affected claimants whose quarterly earnings averaged between \$1,000 and \$2,000, but probably did not affect claimants with higher or lower quarterly earnings.

The JSIE effects shown in Table 6-18 also accord with the effects shown in Table 6-14. The JSIE seems to have had little effect on claimants whose WBA was below \$91 per week. But the JSIE did reduce the benefits received and weeks of insured unemployment of claimants whose WBA was \$91 or greater.¹²

12. Recall that Table 6-14 shows that claimants with very low or very high base period earnings were unaffected by the JSIE. We do not observe the effect of the JSIE dropping off at high WBA levels in Table 6-18 because base period earnings need to be unusually high for a claimant to receive the maximum WBA. (Since the basic WBA in Illinois during the experiment was equal to 48 percent of the average weekly wage during the two quarters of the base period in which earnings were highest, a claimant whose earnings totaled \$8,721 in any two quarters of the base period would receive the

Table 6-19 shows that neither the HIE nor the JSIE had an adverse effect on the reemployment earnings of claimants, when these effects are broken down by Weekly Benefit Amount.

IV. Experimental Effects by Characteristics of the Hiring Employer

It is also possible that the effects of the HIE and JSIE varied with certain characteristics of the industry or employer where a claimant found a job. We explore this possibility presently.

Both the methods and the sample used in this section differ from those used in sections II and III. Regarding first methods, the experimental comparisons in this section--that is, the results showing the effects of the HIE and JSIE on benefits paid, weeks of benefits, and the proportion of claimants who terminated benefits within 11 weeks--are regression adjusted.¹³ Regression adjustment is needed to calculate experimental effects by industry of reemployment and characteristics of the hiring employer because these latter variables are correlated with the experimental treatment. (See the discussion in section I.B above.)

Regarding the sample, note that the experimental comparisons in this section use a sample of only 7,868. This is much smaller than the sample of 12,101 used in most of the calculations above for three reasons. First, and most important, only claimants who had earnings in UI-covered unemployment in the first full quarter following UI benefit termination are included in the sample used in these comparisons. It follows that all of the comparisons we draw in this section are between HIE or JSIE enrollees and controls who obtained covered employment after filing their initial claim. Second, not all claimants about whom we have complete information from the Benefits Information System have matching records in the Wage Records data base, so we have no data on the post-experimental experience of some claimants. Third, for some employers the Standard Industrial Classification code is missing from the Contributions Tax System data base, which is the source of our data on industry classification. Both of the latter two points pose problems of missing data; however, as long as the incidence of the two problems is random, they pose no obstacle to drawing valid experimental comparisons.

maximum WBA of \$161.)

13. Note that the experimental effects on earnings are not regression adjusted, because a comparison of the earnings of the control, HIE, and JSIE groups before enrollment is enough to tell us whether it is appropriate to compare the three groups.

A. Experimental Effects by Industry of Reemployment

Table 6-20 displays the results of decomposing the effects of the HIE and JSIE by¹⁴ the industry in which a claimant found a job after reemployment.

The HIE had a statistically significant effect on the benefits paid to claimants who found employment in only one industry--Wholesale and Retail Trade.¹⁵ The HIE also had a large (but statistically insignificant) effect on the number of weeks of benefits received by those who found employment in Wholesale and Retail Trade, suggesting that employers in the Trade sector, who include fast-food establishments, took advantage of the HIE bonus and hired claimants who presented themselves with an HIE voucher. The figures also hint at the possibility of an effect in the Construction industry--the effects carry a confidence level of only 30 to 50 percent, but the point estimates are large. No other industry shows any signs of having taken advantage of the HIE.

The JSIE also clearly reduced the benefits paid and weeks of insured unemployment of claimants who found reemployment in Wholesale or Retail Trade. In addition, the JSIE appears to have reduced the benefits paid and weeks of insured unemployment of claimants who found employment in Finance, Insurance, and Real Estate. (Effects in these industries are significant at the 88 percent level or higher.) The JSIE may also have affected claimants who were reemployed in the Service sector, although the effects on these claimants are smaller and statistically less significant than the effects on claimants who found employment in Trade or in Finance, Insurance, and Real Estate.

Table 6-21 shows the effects of the HIE and JSIE on earnings after reemployment, by industry. Several results in this table are noteworthy. First, and most striking, is the result that HIE enrollees who found reemployment in Wholesale and Retail Trade did not suffer lower earnings, even though they shortened the length of their spell of unemployment compared with controls who found reemployment in Trade (as shown in Table 6-20). Second, HIE enrollees reemployed in Construction appear to have suffered large earnings losses; however, the same claimants did not reduce the duration of their spell of insured unemployment in response to the HIE, so we cannot link the earnings loss to the HIE per se. Third, Table 6-21's results hint that the HIE may have depressed the earnings of HIE enrollees who were reemployed in two other

14. Because many claimants found jobs with more than one employer (possibly in different industries), we have selected the "most important" employer for each claimant. We define the "most important" employer as the employer who provided the highest proportion of earnings in the complete quarters following the quarter in which each claimant filed for UI benefits.

15. There is a 10 percent probability that the \$151 decrease in benefits detected in the data is spurious.

industries--Transportation and Communication; and Finance, Insurance, and Real Estate. Although small subsample sizes may be preventing us from observing statistically significant effects in these cases, neither effect has a probability value of 0.10 or less. Taking the evidence as a whole, we conclude that there is no link between the more rapid rehiring that the HIE may have induced among claimants who were reemployed in some industries (that is, the Trade sector, and Finance, Insurance, and Real Estate), and the earnings of those claimants after reemployment.

Table 6-21's results yield similar conclusions about the JSIE; that is, we find no definite link between the effectiveness of the JSIE in reducing unemployment duration (Table 6-20) and loss of earnings after reemployment. Of the two groups of JSIE claimants who were reemployed more rapidly--those rehired in Trade and those rehired in Finance, Insurance, and Real Estate--the data hint of an earnings loss after reemployment only for rehired in Finance, Insurance, and Real Estate. However, the finding of an earnings loss for JSIE enrollees reemployed in Finance, Insurance, and Real Estate is not statistically strong enough to firmly establish a link between the effectiveness of the JSIE in reducing unemployment duration (as seen in Table 6-20) and loss of earnings after reemployment.

B. Experimental Effects by Other Characteristics of the Hiring Employer

Table 6-22 shows the breakdown of experimental effects by three separate characteristics of hiring employers: the size of the hiring employer (as measured by the number of employees in the establishment), the average wage level of the hiring employer, and the UI payroll tax rate faced by the hiring employer.

The top panel decomposes the effects of the HIE and JSIE by the size of the employer, with number of employees in the establishment serving as the measure of size. The decomposition seems to hint that larger employers may have been more prone to use the HIE than smaller employers, although only one of the effects shown is statistically significant (the effect on the proportion of claimants rehired within 11 weeks). Even though the evidence in favor of it is somewhat thin, the finding that larger employers were most likely to use the HIE is reasonable, in that smaller employers may be unable or unwilling to bear the administrative costs associated with using the HIE.

The figures shown in the top panel of Table 6-22 suggest a similar relationship between the effectiveness of the JSIE and the size of the hiring employer, in that claimants hired by the smallest firms were least affected by the JSIE. However, it appears that workers who were hired by medium-sized firms were most affected by the JSIE--both the size and the statistical significance of the effects on claimants who were rehired in medium-sized firms is striking. Because the employer plays no role in the use of the JSIE, the apparently greater use of the

JSIE by claimants who went to work in medium-sized establishments is probably an artifact of the occupations and industries in which users of JSIE vouchers found jobs.

The middle panel of Table 6-22 shows how the effects of the HIE and JSIE varied with the average quarterly wages paid by the hiring employer. The evidence, although not strong, suggests that employers who pay average quarterly wages in the middle range of \$3,501 to \$6,500 were most likely to use the HIE. We suspect that this finding follows from the fact that larger employers, who are also generally higher-wage employers, tended to take advantage of the HIE.

The middle panel of Table 6-22 shows that the JSIE was most effective with claimants who found employment in low- and middle-wage establishments. That the JSIE was least effective with claimants who found employment in high-wage firms is not surprising given that high-wage workers tended not to be affected by the JSIE.

The bottom panel of Table 6-22 shows how the effects of the experiments varied with the UI payroll tax rate paid by hiring employers. None of the HIE effects displayed in the bottom panel is statistically significant, and there is no evidence that the effects of the HIE varied with the UI payroll tax paid by an employer.

Figures in the bottom panel of Table 6-22 suggest that there is little relationship between the effectiveness of the JSIE and the UI payroll tax rate paid by employers who hired claimants. (Although the effects of the JSIE are statistically insignificant for claimants who went to work for employers who paid a low UI tax rate, the size of the effects of the JSIE on these claimants is about the same as the size for other claimants. We attribute the lack of statistical significance for this category to small subsample size.)

Table 6-23 shows the effects of the HIE and JSIE on reemployment earnings of claimants, by the characteristics of the hiring employer. For neither the HIE nor the JSIE are any of the earnings effects significant at the 90 percent level. Further, the employer-characteristic breakdowns suggest no correspondence between the effects of the experiments on weeks of unemployment (as shown in Table 6-22) and the effects of the experiments on reemployment earnings. We conclude that characteristics of the hiring employer were irrelevant to the effects of the experiments on reemployment earnings.

V. Summary

Our purpose in this chapter has been to examine whether the effects of the HIE or the JSIE were greater for some subgroups of claimants than for others. The results presented in Chapter 5 suggested that the JSIE had a strong effect on participants, whereas the HIE did not. But these overall conclusions may require modification in light of the disaggregation presented in this chapter.

A. HIE

The most startling result to emerge from disaggregation can be seen in Table 6-8, which shows clearly that the HIE had a strong effect on white women. The results show that the HIE caused a \$164 decline in the benefits paid to white women, and a one-week reduction in weeks of insured unemployment, both over the full benefit year. Further, white women enrolled in the HIE were 5 percent more likely than were controls to terminate benefit receipt within 11 weeks of filing their initial claim. These results are all highly statistically significant, in that they have less than a 5 percent chance of being spurious. Thus, the overall results reported in Chapter 5, which suggest that the HIE had no effect, mask a strong effect on an important group of labor force participants--white women.

The reason for the racial difference in the effects of the HIE can be seen in Table 7-2 (in Chapter 7), which shows the proportion of each race and sex category who submitted Notices of Hire and received bonuses. Table 7-2 shows that the HIE was 4 to 10 times more likely to be used by whites and their employers than by blacks and their employers.

But Table 7-2 offers no clue as to why white women were affected by the HIE whereas white men were not. Both had participation rates of about 6 or 7 percent. Here, the experiments give us no direct evidence, but a possible explanation does present itself. If the jobs women get tend to involve less on-the-job training than the jobs men get, then the \$500 hiring bonus would be more likely to induce an employer to hire a woman. That is, the \$500 bonus is a larger proportion of the training costs incurred by the employer who is hiring a woman. Evidence about on-the-job training is equivocal, although turnover patterns and evidence on the effect of female intensity on women's earnings suggest that women do receive less.¹⁶

16. See Francine D. Blau and Lawrence M. Kahn, "Causes and Consequences of Layoffs," Economic Inquiry 19 (April 1981), pp. 270-296; and John R. Wolfe, "How Are Women's Earnings Affected by the Female-Intensity of Their Occupations?" Unpublished manuscript, Michigan State University, October 1986.

The disaggregated results presented in this chapter also establish four other points about the HIE. First, the HIE affected workers in their early 30s more strongly than it affected other workers (Table 6-1). Second, the effects of the HIE did not vary with claimants' level of education (Table 6-10). This second finding is somewhat surprising, in that the HIE required communication with prospective employers in order to be effective. One would expect a higher level of education to be helpful both in understanding the HIE and in communicating it to prospective employers, but this appears not to be the case.

Third, there is evidence that the effects of the HIE varied with occupation (Table 6-12). Specifically, claimants who were in Clerical and Sales occupations seem to have been more strongly affected by the HIE than were claimants in other occupations. Fourth, the HIE reduced the benefits and weeks of insured unemployment of claimants who gained reemployment in Wholesale and Retail Trade industries (Table 6-20).

The findings from disaggregating the effects of the HIE thus present a consistent picture of the worker who is helped by the HIE, and the type of employer who is most likely to use the HIE. Clearly, white women in their early 30s who are employed in Clerical and Sales occupations were aided by the HIE. Also, employers in Wholesale and Retail Trade, who not coincidentally employ many such workers, were the most likely to make use of the HIE.

B. JSIE

The JSIE differed sharply from the HIE, in that its overall impact was so much stronger. As a result, we would expect the JSIE to affect a broader set of subgroups than did the HIE, and that is in fact the case. Both men and women were affected strongly by the JSIE (Table 6-6). But there does appear to be a difference by race in the response to the JSIE, with blacks responding less strongly than whites (Table 6-4). When the JSIE's effects are broken down by both race and sex, an ordering of effects emerges: White women responded most strongly to the JSIE, white men had the second strongest response, black women responded somewhat less strongly, and black men may not have responded at all (Table 6-8).

The reason for the difference in the effects of the JSIE by race appears to be the lower rate of participation by black men and women in the JSIE. As can be seen in Table 7-2 (Chapter 7), whites were 2 to 3 times more likely to use the JSIE than were blacks. We can only speculate about why blacks tended to make less use of the JSIE, but recall that blacks also tended to make less use of the HIE.

Five further points about the JSIE emerge from the disaggregations presented in this chapter. First, JSIE enrollees under age 35 responded more strongly than did older JSIE

enrollees (Table 6-1). Second, JSIE enrollees who were high-school graduates (but who had no further schooling) showed the strongest response to the JSIE (Table 6-10).

Third, there were important differences across occupation in the effect of the JSIE (Table 6-12). Claimants in at least three occupational groups (Clerical and Sales; Machine Trades and Processing; and Packaging and Materials Handling) responded strongly to the JSIE. Claimants in Professional, Technical, and Managerial occupations clearly did not respond to the JSIE. Thus, claimants in occupations requiring more skills seem to have been less affected by the JSIE.

Fourth, the JSIE was least effective among claimants who had especially low or high earnings before they became unemployed (Table 6-14). Claimants who had average quarterly earnings in the middle range of \$2,000 to \$9,000 responded to the JSIE. Also, the JSIE was least effective among claimants whose earnings were highly variable before they became unemployed (Table 6-16).

Fifth, the JSIE had the greatest effect on claimants who found reemployment in either of two industries--Wholesale and Retail Trade; and Finance, Insurance, and Real Estate. JSIE enrollees who became reemployed in three other industries--Construction; Manufacturing; and Transportation and Communications--were clearly not affected by the JSIE. It follows that the industries in which JSIE enrollees were most likely to find rapid reemployment were also the industries that were growing most rapidly. This is a useful finding because it suggests again that the JSIE was unlikely to have had any displacement effect.¹⁷

C. Work Search and Earnings after Reemployment

Especially with regard to the JSIE, we are concerned with the following question: Did those enrolled in the experiment shorten their spell of unemployment by searching more intensely for work, or did they shorten their spell of unemployment by quickly accepting a low-paying job in order to qualify for the \$500 bonus? If JSIE enrollees increased the intensity of their job search, then the JSIE bonus improved the efficiency of the labor market by promoting more rapid matches between job seekers and employers. If, on the other hand, JSIE enrollees quickly accepted low-paying jobs to obtain the \$500 bonus, then it would follow that longer spells of job search are productive because they lead to higher-paying jobs. It would also follow that UI benefits provide an important (and efficiency-enhancing) subsidy to job search. The attractiveness of an actual program based on the JSIE would be correspondingly reduced.

17. For more on displacement, see the discussions in Chapter 5, section V.D, and in Chapter 8, section II.C.

In this chapter, we have presented evidence on whether the shorter spell of insured unemployment that was experienced by many groups of JSIE enrollees led in turn to lower earnings in the subsequent job. By and large, the findings show that those enrolled in the JSIE did not receive lower earnings once they became reemployed. This finding is consistent with the view that the JSIE reduced the amount of insured unemployment by increasing the intensity with which JSIE enrollees searched for work.¹⁸ We conclude that the JSIE increased the intensity with which JSIE enrollees searched for work, and therefore improved the efficiency of the labor market by promoting quicker matches between job seekers and employers.

18. The finding is also consistent with the view that wage offers do not improve with the duration or intensity of job search. The data that are available from the Illinois experiments do not permit a rigorous test of whether, on the one hand, the JSIE increased job-search intensity or, alternatively, wage offers do not increase with time. Evidence from other studies, however, suggest that wage offers do improve as search continues, so we favor the hypothesis that the JSIE increased the intensity of job search. (See, for example, Nicholas M. Kiefer and George R. Neumann, "Estimation of Wage Offer Distributions and Reservation Wages," in Studies in the Economics of Search, edited by S. A. Lippman and J. J. McCall (Amsterdam: North-Holland, 1979), pp. 171-189.)

TABLE 6-1

Experimental Effects by Age

Age in Years	Benefits Paid (\$)		Weeks of Benefits		Proportion < 11 Weeks	
	Difference: Control vs.		Difference: Control vs.		Difference: Control vs.	
	HIE	JSIE	HIE	JSIE	HIE	JSIE
20 - 24 (732;769;779)	-80.7 (0.34)	-191.4 (0.02)	-0.087 (0.89)	-1.198 (0.05)	+0.026 (0.29)	+0.072 (0.00)
25 - 29 (948;910;1,048)	-45.7 (0.55)	-171.4 (0.02)	+0.093 (0.87)	-1.020 (0.06)	+0.033 (0.13)	+0.068 (0.00)
30 - 34 (777;757;761)	-177.1 (0.03)	-212.0 (0.01)	-1.162 (0.06)	-1.405 (0.02)	+0.061 (0.01)	+0.049 (0.05)
35 - 39 (538;535;596)	+4.4 (0.97)	-149.1 (0.13)	-0.118 (0.87)	-0.775 (0.28)	+0.009 (0.75)	+0.036 (0.21)
40 - 44 (420;414;433)	-6.7 (0.95)	-81.1 (0.47)	-0.986 (0.24)	-1.726 (0.04)	+0.021 (0.52)	+0.062 (0.06)
45 - 49 (289;326;343)	-1.3 (0.99)	-124.4 (0.34)	-0.470 (0.63)	-1.254 (0.20)	+0.040 (0.30)	+0.068 (0.08)
50 - 54 (248;252;226)	+62.5 (0.67)	+22.4 (0.88)	+0.317 (0.77)	-0.165 (0.88)	-0.010 (0.82)	-0.018 (0.68)

NOTES: A negative difference implies that the experimental mean is less than the control mean. Numbers in parentheses under the differences are probability values showing the probability that the difference is statistically zero (derived using an F test). Number of observations in the control, HIE, and JSIE groups for each age category are shown in parentheses in the first column. "Benefits Paid" refers to the average dollar benefits paid to claimants over the full benefit year. "Weeks of Benefits" refers to the average number of weeks of benefits received over the full benefit year. "Proportion < 11 Weeks" refers to the proportion of claimants who were rehired within 11 weeks of filing the initial claim.

TABLE 6-2

Regression-Adjusted Experimental Effects by Age

Age in Years	Benefits Paid (\$)		Weeks of Benefits		Proportion < 11 Weeks	
	Difference: Control vs.		Difference: Control vs.		Difference: Control vs.	
	HIE	JSIE	HIE	JSIE	HIE	JSIE
20 - 24 (732;769;779)	-61.2 (0.41)	-161.8 (0.03)	-0.068 (0.91)	-1.136 (0.06)	+0.026 (0.30)	+0.069 (0.00)
25 - 29 (948;910;1,048)	-25.4 (0.70)	-129.9 (0.04)	-0.025 (0.96)	-1.027 (0.05)	+0.037 (0.10)	+0.067 (0.00)
30 - 34 (777;757;761)	-153.3 (0.04)	-213.2 (0.00)	-0.906 (0.14)	-1.017 (0.09)	+0.054 (0.03)	+0.038 (0.12)
35 - 39 (538;535;596)	-22.3 (0.80)	-102.3 (0.23)	+0.014 (0.98)	-0.602 (0.39)	+0.005 (0.87)	+0.032 (0.26)
40 - 44 (420;414;433)	-89.6 (0.37)	-164.9 (0.09)	-1.022 (0.21)	-1.432 (0.08)	+0.023 (0.50)	+0.052 (0.11)
45 - 49 (289;326;343)	+43.4 (0.71)	-130.9 (0.25)	-0.202 (0.83)	-1.347 (0.16)	+0.036 (0.36)	+0.071 (0.06)
50 - 54 (248;252;226)	+96.6 (0.45)	+29.2 (0.83)	+0.166 (0.88)	-0.473 (0.67)	-0.004 (0.92)	-0.009 (0.85)

NOTES: See notes to Table 6-1. The regression-adjustment procedure is described in section I.B of the text.

TABLE 6-3

Experimental Effects on Reemployment Earnings by Age

<u>Age in years</u>	<u>Dollar Earnings in the Quarter after Benefit Termination</u> <u>Difference: Controls vs.</u>	
	<u>HIE</u>	<u>JSIE</u>
20 - 24 (488;513;549)	-16.6 (0.91)	+12.8 (0.93)
25 - 29 (652;613;696)	-162.4 (0.22)	-7.5 (0.95)
30 - 34 (462;478;507)	-13.1 (0.93)	-18.0 (0.91)
35 - 39 (342;338;398)	-18.4 (0.92)	+38.3 (0.83)
40 - 44 (257;257;291)	-76.6 (0.71)	-168.7 (0.40)
45 - 49 (180;193;219)	-40.1 (0.87)	-119.0 (0.62)
50 - 54 (150;158;127)	+112.4 (0.77)	+661.6 (0.02)

NOTES: A negative difference implies that the experimental mean is less than the control mean. Numbers in parentheses under the differences are probability values showing the probability that the difference is statistically zero (derived using an F test). Number of observations in the control, HIE, and the JSIE groups for each age category are shown in parentheses in the first column.

TABLE 6-4

Experimental Effects by Race

Race	Benefits Paid (\$)		Weeks of Benefits		Proportion < 11 Weeks	
	HIE	JSIE	HIE	JSIE	HIE	JSIE
White (2,497;2,565;2,723)	-82.4 (0.08)	-213.8 (0.00)	-0.344 (0.31)	-1.357 (0.00)	+0.031 (0.02)	+0.057 (0.00)
Black (1,072;1,014;1,050)	-8.7 (0.91)	-68.7 (0.35)	-0.373 (0.48)	-0.743 (0.15)	+0.026 (0.21)	+0.051 (0.01)
Hispanic (299;304;310)	-119.8 (0.39)	-80.4 (0.56)	-0.384 (0.69)	-0.224 (0.82)	+0.024 (0.54)	+0.053 (0.18)
Native American (23;25;42)	-248.6 (0.61)	-105.3 (0.81)	-1.014 (0.77)	-2.126 (0.49)	+0.102 (0.46)	+0.116 (0.35)
Other (61;55;61)	+592.8 (0.06)	+652.5 (0.03)	+3.846 (0.08)	+2.656 (0.22)	-0.021 (0.81)	-0.082 (0.35)

NOTES: See notes to Table 6-1.

TABLE 6-5

Experimental Effects on Reemployment Earnings by Race

<u>Race</u>	<u>Dollar Earnings in the Quarter after Benefit Termination</u>	
	<u>Difference: Controls vs.</u>	
	<u>HIE</u>	<u>JSIE</u>
White (1,668;1,715;1,890)	-57.4 (0.49)	+7.9 (0.92)
Black (617;592;638)	-113.2 (0.41)	-89.6 (0.51)
Hispanic (194;195;196)	+33.1 (0.89)	+127.0 (0.60)
Native American (13;11;24)	+292.0 (0.77)	+907.2 (0.27)
Other (39;37;39)	-47.8 (0.93)	+9.5 (0.99)

NOTES: See notes to Table 6-3.

TABLE 6-6
Experimental Effects by Sex

Sex	Benefits Paid (\$)		Weeks of Benefits		Proportion < 11 Weeks	
	Difference: Control vs.		Difference: Control vs.		Difference: Control vs.	
	HIE	JSIE	HIE	JSIE	HIE	JSIE
Female (1,790; 1,832; 1,829)	-112.1 (0.04)	-174.8 (0.00)	-0.762 (0.06)	-1.285 (0.00)	+0.051 (0.00)	+0.06 (0.00)
Male (2,162; 2,131; 2,357)	-12.5 (0.81)	-152.8 (0.00)	-0.034 (0.93)	-1.024 (0.00)	+0.014 (0.34)	+0.051 (0.00)

NOTES: See notes to Table 6-1.

TABLE 6-7

Experimental Effects on Reemployment Earnings by Sex

<u>Sex</u>	<u>Dollar Earnings in the Quarter after Benefit Termination</u>	
	<u>Difference: Controls vs.</u>	
	<u>HIE</u>	<u>JSIE</u>
Female	+4.2	+19.4
(1,130;1,197;1,220)	(0.97)	(0.84)
Male	-64.1	-15.8
(1,401;1,353;1,567)	(0.48)	(0.86)

NOTES: See notes to Table 6-3.

TABLE 6-8

Experimental Effects by Race/Sex

Race/Sex	Benefits Paid (\$)		Weeks of Benefits		Proportion < 11 Weeks	
	Difference: Control vs.		Difference: Control vs.		Difference: Control vs.	
	HIE	JSIE	HIE	JSIE	HIE	JSIE
White/Female (1,113;1,166;1,170)	-164.1 (0.02)	-262.0 (0.00)	-1.008 (0.05)	-1.623 (0.00)	+0.051 (0.01)	+0.062 (0.00)
White/Male (1,384;1,399;1,553)	-9.8 (0.88)	-185.1 (0.00)	+0.192 (0.67)	-1.125 (0.01)	+0.014 (0.41)	+0.052 (0.00)
Black/Female (527;504;512)	-49.3 (0.64)	-65.9 (0.53)	-0.539 (0.47)	-0.978 (0.19)	+0.052 (0.08)	+0.064 (0.03)
Black/Male (545;510;538)	-33.2 (0.75)	-72.7 (0.48)	-0.211 (0.78)	-0.518 (0.48)	0.000 (0.98)	+0.037 (0.21)
Hispanic/Female (113;128;110)	-77.2 (0.72)	-27.8 (0.90)	-0.402 (0.80)	+0.103 (0.95)	+0.046 (0.45)	+0.036 (0.57)
Hispanic/Male (186;176;200)	-122.9 (0.49)	-122.8 (0.48)	-0.349 (0.78)	-0.414 (0.73)	+0.009 (0.86)	+0.060 (0.22)
Native American/Female (3;5;6)	+600.8 (0.63)	+345.2 (0.77)	-3.134 (0.72)	-1.833 (0.83)	+0.006 (0.85)	+0.167 (0.62)
Native American/Male (20;20;36)	-328.5 (0.54)	-158.3 (0.74)	0.000 (0.0)	-2.094 (0.53)	+0.100 (0.51)	+0.106 (0.43)
Other/Female (34;29;31)	+929.6 (0.03)	+1,013.9 (0.02)	+7.429 (0.01)	+4.847 (0.10)	-0.077 (0.53)	-0.107 (0.37)
Other/Male (27;26;30)	+169.8 (0.71)	+204.6 (0.65)	-0.510 (0.88)	-0.174 (0.96)	+0.052 (0.69)	-0.037 (0.77)

NOTES: See notes to Table 6-1.

TABLE 6-9

Experimental Effects on Reemployment Earnings by Race/Sex

<u>Race/Sex</u>	<u>Dollar Earnings in the Quarter after Benefit Termination</u>	
	<u>Difference: Controls vs.</u>	
	<u>HIE</u>	<u>JSIE</u>
White/Female (730;792;818)	+29.9 (0.80)	+19.9 (0.87)
White/Male (939;923;1,072)	-79.6 (0.46)	-11.2 (0.91)
Black/Female (302;301;306)	-39.3 (0.84)	+8.4 (0.97)
Black/Male (315;291;332)	-168.1 (0.38)	-190.4 (0.30)
Hispanic/Female (73;84;73)	+25.6 (0.95)	+310.2 (0.42)
Hispanic/Male (121;111;123)	+133.5 (0.67)	+12.2 (0.97)
Native American/Female (1;2;3)	+2,513.4 (0.38)	+1,922.6 (0.48)
Native American/Male (12,9,21)	+76.1 (0.94)	+881.3 (0.30)
Other/Female (24;18;20)	-414.0 (0.57)	-1,017.8 (0.15)
Other/Male (15;19;19)	+191.0 (0.81)	+1,000.2 (0.22)

NOTES: See notes to Table 6-3.

TABLE 6-10

Experimental Effects by Education

Years of Schooling Completed	Benefits Paid (\$)		Weeks of Benefits		Proportion < 11 Weeks	
	Difference: Control vs.		Difference: Control vs.		Difference: Control vs.	
	HIE	JSIE	HIE	JSIE	HIE	JSIE
Less than 8 (63;76;61)	+218.9 (0.45)	+185.8 (0.54)	-0.540 (0.79)	-1.129 (0.60)	+0.030 (0.72)	+0.042 (0.63)
8 - 11 (562;554;606)	-172.0 (0.09)	-132.1 (0.18)	-0.902 (0.21)	-0.816 (0.25)	+0.044 (0.13)	+0.049 (0.08)
12 (1,627;1,767;1,829)	-71.8 (0.22)	-254.1 (0.00)	-0.214 (0.01)	-1.831 (0.00)	+0.045 (0.01)	+0.092 (0.00)
13 - 15 (576;619;659)	-42.9 (0.66)	-139.6 (0.15)	-0.309 (0.66)	-0.653 (0.34)	+0.009 (0.75)	+0.018 (0.50)
More than 15 (418;431;507)	+58.0 (0.62)	-70.9 (0.53)	-0.250 (0.76)	-0.948 (0.24)	+0.018 (0.59)	+0.046 (0.15)

NOTES: See notes to Table 6-1.

TABLE 6-11

Experimental Effects on Reemployment Earnings by Education

Years of Schooling Completed	Dollar Earnings in the Quarter after Benefit Termination	
	Difference: Controls vs.	
	HIE	JSIE
Less than 8 (32;51;29)	+336.2 (0.52)	+427.5 (0.47)
8 - 11 (331;331;360)	-38.1 (0.83)	-132.7 (0.45)
12 (1,070;1,123;1,239)	-125.8 (0.21)	-140.7 (0.15)
13 - 15 (364;419;454)	-67.3 (0.69)	-80.0 (0.63)
More than 15 (258;273;503)	+477.9 (0.02)	+901.5 (0.00)

NOTES: See notes to Table 6-3.

TABLE 6-12

Experimental Effects by Occupation

Occupation	Benefits Paid (\$)		Weeks of Benefits		Proportion < 11 Weeks	
	Difference: Control vs.		Difference: Control vs.		Difference: Control vs.	
	HIE	JSIE	HIE	JSIE	HIE	JSIE
Professional, Technical, Managerial (722;766;828)	+113.7 (0.19)	-546.7 (0.51)	+0.387 (0.54)	-0.679 (0.27)	-0.011 (0.67)	+0.020 (0.41)
Clerical, Sales (941;1,025;1,062)	-146.0 (0.05)	-403.8 (0.01)	-0.693 (0.21)	-1.66 (0.00)	+0.063 (0.00)	+0.083 (0.00)
Service (302;346;365)	-105.4 (0.43)	-466.7 (0.30)	-1.420 (0.14)	-2.034 (0.03)	+0.021 (0.58)	+0.063 (0.10)
Machine Trades Processing (420;416;453)	+8.0 (0.95)	-330.5 (0.02)	-0.027 (0.97)	-1.588 (0.05)	+0.039 (0.24)	+0.092 (0.01)
Structural Work, Benchwork (351;376;428)	-163.6 (0.19)	-518.3 (0.48)	-0.681 (0.45)	+0.189 (0.83)	+0.066 (0.07)	+0.063 (0.07)
Packaging and Materials Handling (386;386;399)	-115.2 (0.34)	-344.3 (0.00)	+0.059 (0.95)	-1.460 (0.09)	+0.031 (0.37)	+0.082 (0.02)
Miscellaneous and Agricultural (32;36;47)	+329.2 (0.42)	-301.7 (0.43)	-0.813 (0.78)	-6.621 (0.02)	+0.076 (0.52)	+0.134 (0.23)

NOTES: See notes for Table 6-1.

TABLE 6-13

Experimental Effects on Reemployment Earnings by Occupation

<u>Occupation</u>	<u>Dollar Earnings in the Quarter after Benefit Termination</u>	
	<u>HIE</u>	<u>JSIE</u>
Professional, Technical, Managerial (441;485;548)	+171.7 (0.26)	+237.0 (0.11)
Clerical, Sales (581;640;685)	-168.3 (0.21)	+36.9 (0.78)
Services (187;189;233)	-54.3 (0.82)	-56.9 (0.80)
Machine Trades Processing (294;278;327)	+74.1 (0.70)	+226.0 (0.23)
Structural Work, Benchwork (228;262;279)	-222.8 (0.29)	-400.5 (0.05)
Packaging and Materials Handling (252;260;272)	+55.9 (0.79)	-166.0 (0.41)
Miscellaneous and Agricultural (23;20;32)	-540.2 (0.45)	+321.9 (0.61)

NOTES: See notes to Table 6-3.

TABLE 6-14

Experimental Effects by Average Quarterly Base Period Earnings

Average Quarterly Base Period Earnings	Benefits Paid (\$)		Weeks of Benefits		Proportion < 11 Weeks	
	Difference: Control vs.		Difference: Control vs.		Difference: Control vs.	
	HIE	JSIE	HIE	JSIE	HIE	JSIE
< \$1,000 (504;495;525)	-33.2 (0.73)	-0.4 (0.99)	-0.467 (0.54)	-0.051 (0.94)	+0.055 (0.07)	+0.039 (0.20)
\$1,000 - \$1,999 (890;919;954)	-106.5 (0.14)	-95.7 (0.18)	-0.962 (0.09)	-1.036 (0.07)	+0.042 (0.07)	+0.043 (0.06)
\$2,000 - \$2,999 (778;831;865)	-41.9 (0.58)	-163.8 (0.03)	-0.113 (0.85)	-1.116 (0.05)	+0.005 (0.82)	+0.046 (0.06)
\$3,000 - \$5,999 (1,396;1,299;1,369)	-40.9 (0.48)	-228.4 (0.00)	-0.068 (0.88)	-1.393 (0.00)	+0.037 (0.05)	+0.720 (0.00)
\$6,000 - \$8,999 (296;323;367)	-84.7 (0.49)	-360.3 (0.00)	-0.845 (0.38)	-2.779 (0.00)	+0.052 (0.18)	+0.118 (0.00)
> \$8,999 (88;96;106)	+401.9 (0.07)	+398.4 (0.07)	+1.825 (0.31)	+2.253 (0.18)	-0.162 (0.02)	-0.141 (0.04)

NOTES: See notes to Table 6-3.

TABLE 6-15

Experimental Effects on Reemployment Earnings
by Average Quarterly Base Period Earnings

<u>Average Quarterly Base Period Earnings</u>	<u>Dollar Earnings in the Quarter after Benefit Termination</u>	
	<u>Difference: Controls vs.</u>	
	<u>HIE</u>	<u>JSIE</u>
< \$1,000 (322;297;320)	+135.5 (0.41)	+252.1 (0.12)
\$1,000 - \$2,000 (548;589;642)	-140.7 (0.25)	+16.6 (0.89)
\$2,000 - \$3,000 (472;545;570)	+127.7 (0.33)	-132.7 (0.30)
\$3,000 - \$6,000 (938;863;940)	-104.3 (0.28)	-102.9 (0.28)
\$6,000 - \$9,000 (210;204;261)	-63.2 (0.76)	+455.6 (0.02)
> \$9,000 (41;52;54)	-1,158.5 (0.01)	-1,691.7 (0.02)

NOTES: See notes to Table 6-3.

TABLE 6-16

Experimental Effects by Variability of Earnings
in the Base Period

Coefficient of Variation of Earnings	Benefits Paid (\$)		Weeks of Benefits		Proportion < 11 Weeks	
	Difference: Control vs.		Difference: Control vs.		Difference: Control vs.	
	HIE	JSIE	HIE	JSIE	HIE	JSIE
0.0 - 0.1 (954;980;1,051)	-6.3 (0.93)	-206.3 (0.01)	-0.438 (0.43)	-2.158 (0.00)	+0.020 (0.36)	+0.066 (0.00)
0.1 - 0.2 (665;712;737)	-242.8 (0.01)	-323.9 (0.00)	-1.205 (0.07)	-1.656 (0.01)	+0.062 (0.02)	+0.074 (0.00)
0.2 - 0.6 (821;815;811)	-12.0 (0.89)	-149.0 (0.07)	-0.212 (0.72)	-1.306 (0.03)	+0.035 (0.15)	+0.070 (0.00)
0.6 - 1.1 (875;890;962)	-88.3 (0.27)	-81.1 (0.30)	+0.023 (0.97)	-0.217 (0.70)	+0.035 (0.12)	+0.043 (0.06)
> 1.1 (637;566;625)	-41.8 (0.67)	-74.0 (0.43)	-0.050 (0.94)	-0.168 (0.81)	-0.002 (0.94)	+0.016 (0.56)

NOTES: See notes to Table 6-1.

TABLE 6-17

Experimental Effects on Reemployment Earnings by
Variability of Earnings in the Base Period

<u>Coefficient of Variation of Earnings</u>	<u>Dollar Earnings in the Quarter after Benefit Termination</u>	
	<u>Difference: Controls vs.</u>	
	<u>HIE</u>	<u>JSIE</u>
0.0 - 0.1 (597;619;691)	-108.4 (0.43)	-51.4 (0.70)
0.1 - 0.2 (433;456;485)	-337.2 (0.04)	-333.2 (0.03)
0.2 - 0.6 (543;549;576)	+66.4 (0.65)	+134.4 (0.35)
0.6 - 1.1 (580;573;654)	+9.5 (0.95)	+131.1 (0.34)
> 1.1 (378;353;381)	+20.4 (0.91)	+54.6 (0.75)

NOTES: See notes to Table 6-3.

TABLE 6-18

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Experimental Effects by Weekly Benefit Amount

Weekly Benefit Amount	Benefits Paid (\$)		Weeks of Benefits		Proportion < 11 Weeks	
	Difference: Control vs.		Difference: Control vs.		Difference: Control vs.	
	HIE	JSIE	HIE	JSIE	HIE	JSIE
\$51 (347;333;355)	-32.3 (0.78)	+6.4 (0.95)	-0.421 (0.65)	+0.450 (0.62)	+0.038 (0.31)	+0.022 (0.55)
\$52 - \$90 (794;861;887)	-66.2 (0.36)	-86.5 (0.23)	-1.159 (0.05)	-1.725 (0.00)	+0.046 (0.05)	+0.038 (0.11)
\$91 - \$120 (666;711;738)	-69.8 (0.38)	-158.2 (0.05)	-0.524 (0.42)	-1.084 (0.10)	+0.027 (0.30)	+0.070 (0.01)
\$121 - \$160 (749;716;822)	-35.9 (0.64)	-136.5 (0.07)	-0.098 (0.88)	-1.114 (0.07)	+0.050 (0.05)	+0.070 (0.00)
\$161 (1,396;1,342;1,384)	-80.3 (0.78)	-186.1 (0.00)	+0.074 (0.87)	-0.315 (0.00)	+0.011 (0.54)	+0.060 (0.00)

NOTES: See notes to Table 6-1.

TABLE 6-19

Experimental Effects on Reemployment Earnings
by Weekly Benefit Amount

<u>Weekly Benefit Amount</u>	<u>Dollar Earnings in the Quarter after Benefit Termination</u>	
	<u>HIE</u>	<u>JSIE</u>
\$51 (224;193;215)	+42.2 (0.84)	+118.2 (0.56)
\$52 - \$90 (460;538;575)	+84.0 (0.54)	+186.1 (0.16)
\$91 - \$120 (439;470;499)	-111.0 (0.43)	-56.3 (0.69)
\$121 - \$160 (472;478;543)	+150.7 (0.28)	-25.6 (0.85)
\$161 (936;871;955)	-58.0 (0.56)	+95.1 (0.33)

NOTES: See notes to Table 6-3.

TABLE 6-20

Regression-Adjusted Experimental Effects by Industry in which
Claimants Found a Job after Spell of Unemployment

Industry	Benefits Paid		Weeks of Benefits		Proportion < 11 weeks	
	Difference: Control vs.		Difference: Control vs.		Difference: Control vs.	
	HIE	JSIE	HIE	JSIE	HIE	JSIE
Construction (109;100;95)	-221.1 (0.27)	-271.9 (0.18)	-1.259 (0.43)	-1.397 (0.38)	+0.044 (0.52)	+0.015 (0.83)
Manufacturing (681;675;657)	+23.4 (0.77)	-63.7 (0.42)	-0.186 (0.77)	-0.480 (0.44)	+0.033 (0.22)	+0.058 (0.03)
Wholesale and Retail Trade (589;630;716)	-135.1 (0.10)	-314.9 (0.00)	-0.909 (0.17)	-2.493 (0.00)	+0.066 (0.02)	+0.092 (0.00)
Transportation, Communications (135;127;151)	+71.9 (0.69)	+39.2 (0.82)	+0.006 (1.00)	-0.205 (0.88)	-0.029 (0.64)	+0.064 (0.27)
Finance, Insurance, Real Estate (161;137;168)	-51.5 (0.76)	-328.5 (0.04)	+0.002 (1.00)	-1.931 (0.12)	+0.012 (0.84)	+0.097 (0.08)
Service (683;722;814)	-46.5 (0.55)	-118.5 (0.12)	+0.054 (0.93)	-0.794 (0.18)	+0.025 (0.34)	+0.043 (0.09)
Other (173;159;184)	+189.8 (0.23)	+26.9 (0.86)	+1.173 (0.35)	+0.703 (0.56)	-0.045 (0.41)	+0.007 (0.89)

NOTES: See notes to Table 6-2.

TABLE 6-21

Experimental Effects on Reemployment Earnings by Industry in
which Claimants Found a Job after Spell of Unemployment

<u>Industry</u>	<u>Dollar Earnings in the</u> <u>Quarter after Benefit</u> <u>Termination</u>	
	<u>Difference: Controls vs.</u>	
	<u>HIE</u>	<u>JSIE</u>
Construction (109;100;95)	-780.0 (0.02)	-542.4 (0.10)
Manufacturing (681;675;657)	+42.1 (0.74)	+171.6 (0.19)
Wholesale and Retail Trade (589;630;716)	+5.6 (0.97)	+103.9 (0.43)
Transportation, Communication (135;127;151)	-351.8 (0.23)	-155.1 (0.58)
Finance, Insurance, Real Estate (161;137;168)	-343.4 (0.21)	-382.5 (0.14)
Service (683;722;814)	+81.7 (0.52)	+62.7 (0.61)
Other (160;146;171)	-160.6 (0.55)	+295.4 (0.26)

NOTES: See notes to Table 6-3.

TABLE 6-22

Regression-Adjusted Experimental Effects by Characteristics of Hiring Employer

Number of Employees	Benefits Paid (\$)		Weeks of Benefits		Proportion < 11 Weeks	
	HIE	JSIE	HIE	JSIE	HIE	JSIE
< 11 employees (460;448;532)	+104.7 (0.27)	-87.9 (0.34)	+0.722 (0.33)	-0.694 (0.33)	+0.012 (0.71)	+0.039 (0.21)
11 - 150 employees (862;905;971)	-75.5 (0.27)	-223.6 (0.00)	-0.421 (0.43)	-1.526 (0.00)	+0.019 (0.40)	+0.059 (0.01)
> 150 employees (1,209;1,197;1,284)	-49.0 (0.40)	-138.3 (0.02)	-0.321 (0.48)	-0.989 (0.03)	+0.040 (0.04)	+0.069 (0.00)
<u>Average Quarterly Wage Paid</u>						
< \$3,501 (964;957;1,098)	+53.0 (0.42)	-178.7 (0.01)	+0.488 (0.34)	-1.334 (0.01)	-0.004 (0.86)	+0.044 (0.04)
\$3,501 - \$6,500 (945;999;1,073)	-95.8 (0.14)	-151.4 (0.02)	-0.692 (0.18)	-0.980 (0.05)	+0.048 (0.03)	+0.060 (0.01)
> \$6,500 (622;594;616)	-58.1 (0.48)	-135.0 (0.10)	-0.354 (0.58)	-1.036 (0.11)	+0.044 (0.11)	+0.087 (0.00)
<u>UI Payroll Tax Rate</u>						
< 1.5 percent (307;335;348)	-13.2 (0.91)	-145.0 (0.20)	-0.345 (0.70)	-0.690 (0.44)	+0.030 (0.44)	+0.053 (0.17)
1.5 - 6.5 percent (1,246;1,237;1,412)	-57.1 (0.33)	-173.5 (0.00)	-0.337 (0.46)	-1.199 (0.01)	+0.032 (0.10)	+0.049 (0.01)
> 6.5 percent (978;978;1,027)	-12.9 (0.84)	-123.7 (0.06)	+0.001 (1.00)	-0.979 (0.05)	+0.025 (0.26)	+0.071 (0.00)

NOTES: See notes to Table 6-2.

TABLE 6-23

Experimental Effects on Reemployment Earnings
by Characteristics of Hiring Employer

<u>Number of Employees</u>	<u>Dollar Earnings in Quarter After Benefit Termination</u>	
	<u>Difference: Controls vs.</u>	
	<u>HIE</u>	<u>JSIE</u>
< 11 employees (460;448;532)	-180.7 (0.25)	-68.9 (0.65)
11 - 150 employees (862;905;971)	+36.7 (0.75)	+21.5 (0.85)
> 150 employees (1,209;1,197;1,284)	-81.1 (0.40)	+49.7 (0.60)
<u>Average Quarterly Wage Paid</u>		
< \$3,501 (964;957;1,098)	+47.4 (0.64)	+20.0 (0.84)
\$3,501 - \$6,500 (945;999;1,073)	-89.4 (0.37)	+41.9 (0.67)
> \$6,500 (622;594;616)	-136.7 (0.28)	+136.3 (0.27)
<u>UI Payroll Tax Rate</u>		
< 1.5 percent (307;335;348)	-217.3 (0.25)	-92.1 (0.62)
1.5 - 6.5 percent (1,246;1,237;1,412)	-65.6 (0.50)	+146.5 (0.12)
> 6.5 percent (978;978;1,027)	+16.2 (0.88)	-148.0 (0.17)

NOTES: See notes to Table 6-3.

Chapter 7

PARTICIPATION IN THE EXPERIMENTS

I. The Meaning and Importance of Participation

The analyses of the bonus experiments described in Chapters 5 and 6 use the full sample of those offered participation and determined to be eligible for UI benefits. These analyses possess all the power of randomization. However, there is also interest in the subset of eligibles who participated in the program because it is reasonable to expect that the behavioral changes due to the experiment were concentrated on those who actually participated. Furthermore, to infer from the experiment what would occur under a fully implemented program, it is necessary to ascertain the rate of participation in the experiment, and to estimate how that rate is likely to change in a program.

Unfortunately, the meaning of participation in the bonus experiments is ambiguous. Following Ashenfelter, we could define participation as receiving a benefit, i.e., a bonus.¹ This definition, however, is unsatisfactory in many respects. It would include as participants, claimants who would have obtained jobs within 11 weeks of filing without the bonus and do not alter their job seeking behavior, and it would exclude claimants who attempt to "win" the bonus by intensifying job search and still fail to obtain a job within 11 weeks. Ideally, we would want a behavioral definition of participation that would include as participants all those individuals who signed agreements to participate and who took the bonus offer into account in determining their job search behavior.

For the JSIE, it would mean taking the offer of the bonus into account in the decision as to job search intensity and acceptance of job offers. For the HIE, participation would be indicated by the claimant communicating the existence of the voucher option to prospective employers.

However, determining who participated using these definitions is not possible because the behavioral responses to the bonus offer are not observed. Applying Ashenfelter's method for identifying participants, every eligible claimant who obtained employment within 11 weeks is a potential participant. Actual participation in the experiments differs from potential participation for two reasons; first, it is increased by the number of

1. In the income maintenance experiments, for instance, participation was defined by Ashenfelter as being the act of obtaining income maintenance payments (Ashenfelter, O., "Determining Participation in Income-Tested Social Programs," Journal of the American Statistical Association, Sept. 1983, vol. 78, no. 383, pp. 517-25).

claimants who would not have obtained employment within 11 weeks without the bonus offer due to economic incentives, and second, it is decreased by the number of claimants who for nonpecuniary reasons do not participate in the experiment.

Nonpecuniary influences were very important, and caused a large proportion of eligible claimants not to participate in the experiments. For each experiment, the eligible claimant was offered the opportunity to sign an agreement, which stated that he/she had been informed of the offer, understood its conditions, and "agreed to participate." Signing this agreement, however, proved to be an inadequate indicator of participation.

About 65 percent of eligible claimants agreed to participate in the HIE and about 84 percent of eligibles agreed to participate in the JSIE. These proportions were essentially the same for those who succeeded in terminating benefits within 11 weeks and those who did not. For the HIE, 1,007 claimants who agreed to participate met this eligibility criteria, but only 191 of them (19.0 percent) filed Notices of Hire. For the JSIE, participation was much higher, but still far from all inclusive. For the JSIE, 1,468 claimants who agreed to participate met the eligibility requirements, and 711 of them (48.8 percent) filed Notices of Hire. (See Table 7-1.)

Nonpecuniary reasons for failure to participate had dramatic results in terms of the racial characteristics of those affected by the experiments. As noted in Chapter 6, the HIE was effective for one group--white women. As shown in Table 7-2, there was little difference among the racial or sex groups regarding the proportion who signed agreements to participate in either experiment, but a large difference in the proportion submitting Notices of Hire or receiving bonuses.

For the HIE, blacks did not make use of the bonus offer in obtaining employment, despite signing agreements to participate. Notices of Hire were submitted for only 1 or 2 percent of black claimants who were eligible to participate--about one-seventh of the proportion for white claimants. For the JSIE, the racial differences were not so dramatic; about 20 percent of eligible white and 10 percent of eligible black claimants submitted Notices of Hire. The higher participation of blacks in the JSIE resulted in this experiment having a positive effect on job acquisition of black women.

Having determined that signing the agreement to participate in the experiment was an inadequate measure of active participation, it was deemed important to identify active participants and to determine the nature of the nonpecuniary reasons that claimants offered for failure to participate. A telephone follow-up survey was conducted on a sample of eligible claimants in an effort to identify "active participants," but the effort was not wholly successful, as demonstrated below.

II. The Follow-Up Survey of Participation

To determine more accurately the extent of real participation in the HIE and to determine for both experimental treatments the reasons for nonparticipation, the Upjohn Institute, under authorization from the State of Illinois, contracted with Central Telephone Interviewing System (CTIS) of Chicago to conduct interviews on a random sample of those offered enrollment in the experiment. The purpose of the follow-up survey was to answer questions about the decision to participate in the experiments.

CTIS was asked to conduct interviews on a random sample of 1,000 HIE eligibles and 1,000 JSIE eligibles. To complete 2,000 interviews, it was determined that a sample of 3,600 was required. (In fact, a sample of 3,000 was originally selected, but was later supplemented by an additional sample of 600 prospective participants.) The survey sample was selected from among those who were offered enrollment in either the JSIE or the HIE and were determined to be eligible to participate in the experiments, according to information available on the analytical data base. (The data base used in the evaluation presented in Chapters 5 and 6.)

Different survey instruments were designed for each of the two experiments. Copies of the survey instrument are included as Appendix A7. The JSIE survey instrument was limited to questions about the respondent's recollection of the offer, his/her agreement to participate, and reasons for refusal to participate. The HIE instrument was somewhat more complicated because of the need to determine whether individuals who agreed to participate in the HIE actually communicated knowledge of the experiment to prospective employers, and whether they believed that the employer was responsive to the bonus offer.

The follow-up survey was conducted in December 1985 and early January 1986. The disposition of those calls is shown in Table 7-3. Except in the category "Moved, no forwarding address or phone," there were no significant differences in reasons for survey noncompletion between the two experiments. Both surveys had completion rates of 55 percent. For this population, one year after the completion of the enrollment period, the response rate was as good or better than expected.

CTIS attempted to interview 3,600 randomly selected participants, 1,800 in each of the two experiments. Although CTIS made at least seven attempts to contact each member of the panel, interviewing stopped when the goal of 1,000 complete interviews for each of the two experiments was reached. As a result, Table 7-3 shows that 123 of the HIE sample and 100 of the JSIE sample remained unaccounted for at the conclusion of the survey. Table 7-4 shows that the proportion of eligible claimants who participated in the experiment was the same among those interviewed in

the follow-up survey and the total population of those eligible to participate. This indicates that those interviewed in the follow-up survey were a random sample of the population of eligible claimants offered enrollment in the experiment.

The follow-up survey sample was comprised of individuals offered participation in the experiment who were monetarily eligible for UI benefits. To obtain survey results for a sample of individuals representative of the population of claimants both monetarily and nonmonetarily eligible to participate in the experiment, individuals who were nonmonetarily ineligible to receive benefits, or were outside of the 20 to 54 age group, or who could not be matched with a record in the BIS data base, were removed from the follow-up survey data base. Removing these other ineligibles, reduces the sample used in this analysis from 1,000 to 788 HIEs and from 1,000 to 783 JSIEs, as shown in Table 7-5.

A. Participation in the Hiring Incentive Experiment

It is useful to divide the 788 HIE eligibles who responded to the follow-up survey into two categories: those who terminated UI benefits within the 11-week filing period because of job acquisition, and those who did not terminate benefits within that time period or terminated benefits for reasons other than job acquisition, and therefore did not confer bonus eligibility upon an employer. Table 7-6 shows that 250, or 32 percent, of the 788 respondents met the 11-week bonus eligibility criteria, and 538 did not.

If we define participation as the act of signing the agreement to participate, Table 7-6 shows that 67 percent of those who eventually terminated benefits within 11 weeks agreed to participate and 65 percent of those who did not terminate within 11 weeks agreed to participate. These rates are not significantly different and indicate that there was no tendency for more job-eager claimants to agree to participate.

Some of the claimants offered participation in the HIE signed the agreement to participate, but apparently made no use of the voucher. Since participation in the HIE required that participants inform prospective employers about the bonus, we feel confident classifying as nonparticipants those eligibles who thought they had refused, forgot that they had agreed to participate, or did not tell any employer about the experiment. We will call this group, "passive nonparticipants."

Including passive nonparticipants in the refusal category reduces the participation rate to 40 percent for the group terminating benefits in 11 weeks, and 45 percent for the group not terminating in the period. (See Table 7-6.) The large number of passive nonparticipants demonstrates that we are not justified in using the signed agreement as a measure of true participation in the experiment. Since the difference in "active" participation rates between those terminating bonuses within 11 weeks and those

not terminating benefits in that period is not statistically significant at standard confidence levels, there is no indication that those with a predisposition to terminate benefits within 11 weeks tended to participate more in the experiment than those without such a predisposition. These results are also consistent with the hypothesis that there is no such predisposition.

In an attempt to better understand participation in the HIE, questions were asked on the follow-up survey to elicit information as to the reasons claimants actively refused, or passively failed, to participate in the experiment. Four hypotheses were explored. The first is that people did not participate because they wished to avoid the stigma of being unemployed, or being on unemployment insurance, or needing to offer the employer money to obtain employment. The second hypothesis is that those who did not participate were weakly attached to the labor market and did not intend to search for work seriously. A third hypothesis is that those who did not participate either did not understand the experiment or were suspicious of the motives of the agency. The fourth hypothesis is that claimants were simply opposed to the notion of giving employers money to hire them.

As shown in the table below (Table 7-7), total nonparticipation accounted for by these hypotheses, plus nonparticipation by those expecting recall, account for less than half of nonparticipation, and no single explanation stands out as a major contributor.

The apparent absence of stigma as a reason for nonparticipation is particularly interesting in the light of work by Gary Burtless showing that stigma was a strong factor in the lack of use of TJTC for the disadvantaged population.² Given that the experiment was an entirely new experience for the claimants, we would have expected more claimants to have had difficulty understanding the experiment, or at least suspicious of the experimenters' motives, but few respondents gave either of these as reasons for nonparticipation. We are left with more than half of nonparticipation unexplained.

Eliminating active and passive refusers should provide an accurate picture of participation in the HIE. However, there remains a significant gap between active participation and number of eligible claimants for whom Notices of Hire were submitted. Among the 99 active participants terminating benefits within 11 weeks, the survey showed that Notices of Hire were submitted for only 36. It is likely that most of the residual unexplained nonuse of the voucher resulted from active or passive rejection by employers, or by a failure of some participants to communicate to the hiring employers the nature of the employers' entitlement to a bonus.

2. "Are Targeted Wage Subsidies Harmful? Evidence from a Wage Voucher Experiment," Industrial and Labor Relations Review, vol. 39, no. 1 (Oct. 1985).

Outright rejection by employers was mentioned by 30 of the 788 responders, only 3 of whom terminated benefits within 11 weeks of filing. (See Table 7-6.) Lack of interest on the part of employers, or failure of communication on the part of claimants, contributed to a Notice of Hire not being submitted in 60 cases in which a respondent was an active participant and obtained employment within 11 weeks.

Overall, the largest factor contributing to the low utilization of the HIE was the lack of interest on the part of UI claimants in using the bonus offer to enhance their employment prospects. Only 40 percent (99) of the 250 responders to the follow-up survey deemed eligible for UI benefits, eligible to participate in the experiment, and terminated benefits within 11 weeks of filing, told employers about the bonus offer. The other 60 percent (151), actively or passively refused to participate in the program and made no use of the bonus offer.

Of the 99 eligible claimants who apparently participated by telling employers about the bonus offer, a Notice of Hire was submitted for only 36, or 36 percent. We have no information about employer participation that would permit an analysis of the reasons for this low rate of employer participation, but assume it is some combination of lack of interest and lack of knowledge.

It stands to reason that employer rejection was more significant in the cases in which the claimants did not obtain employment within 11 weeks. Although we have no evidence regarding the passive rejection by employers for this group, active rejection by employers was experienced by 27, or 11 percent, of those claimants who failed to obtain employment within 11 weeks and who claimed to have attempted to use the bonus offer.

Overall, the results of the follow-up survey indicate that participation in the HIE is an ambiguous concept. By any definition, utilization of the HIE bonus offer by UI claimants and employers was low. Notices of Hire were submitted by employers for only 14 percent of claimants who terminated benefits within 11 weeks. (See Table 7-6.) Apparently, a significant change in approach would be necessary to increase acceptance and participation by both claimants and employers if a program modelled on the HIE is to have a substantial effect on the reemployment rate of UI claimants.

B. Participation in the Job Search Incentive Experiment

Participation in the JSIE is even more difficult to define than in the HIE. In the HIE, participation is signaled by communicating the bonus offer to prospective employers. In the JSIE, participation is the act of taking into account the bonus offer when deciding upon job search strategy and making decisions to accept or reject job offers.

Table 7-8 shows that 286 of the 783 eligible respondents terminated UI benefits within 11 weeks and obtained employment, making them eligible to apply for the voucher (and eventually receive the bonus if they remained employed for four months). Of this group, 161 submitted Notices of Hire, yet 257 had signed agreements to participate. Subtracting the passive nonparticipants, i.e., those who did not remember that they had agreed to participate or thought they had refused, leaves 242 "active" participants among claimants terminating benefits in less than 11 weeks.

Table 7-8 displays the lists of reasons for failure to submit a Notice of Hire for the 81 active participants who qualified for submittal (and the bonus if they remained employed for four months), yet did not submit a notice. The reasons are not very illuminating. Thirteen said they were not eligible for UI, yet the records show that they received benefits; 12 said that they did not get a job, yet the records showed that they terminated benefits; 15 said that they did not think they qualified for the bonus, yet in all cases they terminated benefits and did not reopen claims within the four month grace period. Thus, there is a great deal of misinformation contained in these explanations, and the real active participation rate remains illusive.

We estimate the active participation rate of those terminating benefits within 11 weeks is somewhere between 56 percent and 85 percent of those eligible. The lower rate is the proportion of eligible claimants who obtained employment, terminated benefits within 11 weeks of filing and submitted a Notice of Hire. The higher rate is the proportion of eligibles who did not actively refuse to participate, or had forgotten that they had signed the agreement to participate. We have no knowledge regarding the attempt to accelerate job search in response to the bonus offer of those who failed to terminate benefits within 11 weeks.

For those who refused to participate in the experiment, according to their responses to the follow-up survey (see Table 7-9), stigma played a very minor role, and weak job search motivation played a somewhat larger, though still minor, role. Failure to understand or trust the experiment accounted for at least one-third of the refusals.

Most of the nonutilization of the experiment by those who were eligible to at least submit a Notice of Hire represents confusion, lack of comprehension, suspicion of government's motives, or reluctance to become involved with bureaucracy. It is likely that in a real program, a substantial proportion of these individuals would participate, and cash their vouchers.

If it is true that as many as 45 percent of those who terminated benefits within 11 weeks failed to take advantage of a bonus that was available to them, then it is also likely that many of those who failed to qualify were not motivated by the bonus offer. If this is the case, then it can be assumed that in a full program two counteracting changes will occur in the behavior of those

offered the bonus: (1) more of those who qualify will cash the voucher, and (2) more of those offered the bonus will change their behavior and obtain employment more rapidly. The net balance of these two behavioral changes cannot be ascertained, and therefore the net cost effect of the changes is also unknown. The implications of these changes in behavior for a fully implemented bonus program are discussed in Chapter 8.

TABLE 7-1

Participation in the HIE and JSIE
by Length of Unemployment Spell

	<u>HIE</u>		<u>JSIE</u>	
	<u>Terminated</u> <u>in < 11 wks.</u>	<u>Not Terminated</u> <u>in < 11 wks.</u>	<u>Terminated</u> <u>in < 11 wks.</u>	<u>Not Terminated</u> <u>in < 11 wks.</u>
Agreed to Participate 1	1,007/1,522 =66.2%	1,579/2,441 =64.7%	1,468/1,709 =85.9%	2,059/2,477 =83.1%
NOH Submitted	191/1,522 =12.6%	2/2,441 = 0.1%	711/1,709 =41.6%	28/2,477 = 1.1%
Bonus Paid	111/1,522 =7.29%	1/2,441 = 0.04%	555/1,709 =32.48%	15/2,477 = 0.61%

1. Missing counted as refusal.

TABLE 7-2

Participation in the HIE and JSIE
by Race/Sex

<u>Race/Sex</u>	<u>Agreed to Participate</u>	<u>HIE</u>	
		<u>Proportion Who Submitted or Received</u> <u>NOH</u>	<u>Bonus</u>
White/Female	722/1,166 =0.6192	85/1,166=0.0729	54/1,166=0.0463
White/Male	939/1,399 =0.6712	91/1,399=0.0650	50/1,399=0.0358
Black/Female	328/504 =0.6508	7/504=0.0139	2/504=0.0040
Black/Male	359/510 =0.7039	9/510=0.0176	2/510=0.0039

		<u>JSIE</u>	
		<u>Proportion Who Submitted or Received</u> <u>NOH</u>	<u>Bonus</u>
White/Female	996/1,170 =0.8513	252/1,170=0.2154	206/1,170=0.1761
White/Male	1,366/1,553 =0.8800	357/1,553=0.2299	264/1,553=0.1700
Black/Female	408/512 =0.8000	55/512=0.1074	32/512=0.0625
Black/Male	438/538 =0.8141	52/538=0.0967	34/538=0.0632

NOTE: Each fraction is the number of claimants in a given experimental/race/sex category who submitted a Notice of Hire (or received a bonus) divided by the total number of claimants in that same experimental/race/sex category. For example, 85/1,166 indicates that 85 of the 1,166 white women who were enrolled in the HIE submitted a Notice of Hire. Standard errors of proportions are shown in parentheses.

TABLE 7-3

Illinois Unemployment Insurance Experiments
Follow-Up Survey

<u>Disposition</u>	<u>Hiring Incentive Experiment</u>	<u>Job Search Incentive Experiment</u>
No answer/respondent not available	123	100
No such person at this telephone number	242	227
Moved, no forwarding address or telephone	56	96
Respondent or household refused	58	63
Claimed no one in household on unemployment in 1984	10	8
Language barrier	17	16
Surveyed in pre-test	1	1
Respondent deceased	3	1
Not a working telephone number	290	288
Completed interviews	<u>1000</u>	<u>1000</u>
Total	1800	1800

TABLE 7-4

Agreement to Participate by Data Source

	<u>Follow-Up Survey</u>	<u>Office Logs</u>
Proportion of Eligibles who Agreed to Participate in the Experiment:		
HIE	.64	.65
JSIE	.86	.84

TABLE 7-5

Follow-Up Survey
Responders Eligible to Participate

	<u>HIE</u>	<u>JSIE</u>	<u>Total</u>
Follow-Up Survey Completions	1000	1000	2000
Removed because of duplication or failure to match	- 44	- 63	-107
Removed for non-monetary ineligibility	-166	-151	-317
Removed for under or over age	<u>- 2</u>	<u>- 3</u>	<u>- 5</u>
Total Eligibles	788	783	1571

SOURCE: Follow-Up Survey.

TABLE 7-6

Submittal of Notices of Hires in the HIE
by Length of Unemployment Spell

	Rehired in \leq 11 wks. no. % 3			Not Rehired in \leq 11 wks. no. %		
Total Eligible 1	250	100%		538	100%	
Refused 1	- 83			- 187		
Signed Agreement to Participate	167	67%		351	65%	
Passive Nonparticipation:						
Did not use 2	- 29			- 49		
Accepted, but did not tell employer	- 39			- 60		
Actively Participated	99	40%		242	45%	
Employer Rejected Offer	- 3			27		
Other Non-Use	- 60			211		
NOH submitted by employer	36	14%		4 4	1%	

SOURCE: Follow-Up Survey.

1. According to Office Logs.

2. Office Logs showed acceptance, but responses to follow-up survey shows that respondents believed that they had refused, or they did not know if they accepted or refused.

3. Rehired in \leq 11 weeks means terminating benefits in less than 11 weeks, having some earnings in the quarter after termination, and had a rehire date in the BIS file. This is a more stringent qualification than simply terminating benefits in 11 weeks, which is the condition imposed in Chapters 5 and 6.

4. These four did not technically meet the requirements for submission of Notice of Hire.

TABLE 7-7

Reasons for Nonparticipation in HIE

	Rehired in \leq 11 wks.	Not Rehired in \leq 11 wks.
Stigma	11	14
Weak work search	12	43
Comprehension problem or suspicion	12	41
Did not want employer to get bonus	7	28
Expected Recall	14	17
Other Survey Responses	44	74
Other nonparticipants (responded D.K.)	<u>51</u>	<u>79</u>
Total Nonparticipation	151	296

SOURCE: Follow-Up Survey. See notes to Table 7-6.

TABLE 7-8

Participation in the JSIE

	Rehired ≤ 11 wks.		Not Rehired ≤ 11 wks.	
	-----		-----	
Total Eligible	286	100%	497	100%
Refused (Office Logs)	- 29		- 56	
Signed Agreement; i.e., Nominal Participant	257	90%	441	89%
Refused/D.K. (Survey only)	- 15		- 33	
Active Participant	242	85%	408	82%
Participants not submitting NOH:				
Did not think qualified for bonus	15			
Forgot about it	7			
Did not understand	2			
Did not want to ask employer	1			
Employer refused to sign	1			
Not eligible for UI	13			
Did not get job	12			
Other or no response	<u>30</u>			
Total not submitting NOH	81			
Total submitting NOH	161	56%	161	

SOURCE: Follow-Up Survey. See notes to Table 7-6.

1. Presumably ineligible to submit Notice of Hire.

TABLE 7-9

Reasons for Nonparticipation in the JSIE

	Rehired in \leq 11 wks.	Not Rehired in \leq 11 wks.
Stigma	5	1
Expect Recall	1	2
Comprehension/Suspicion	7	12
Weak Work Search	3	11
Other	<u>5</u>	<u>15</u>
Total Refusals (Survey) 1	21	41

SOURCE: Follow-Up Survey. See notes to Table 7-6.

1. 23 of those who were rehired in $<$ 11 weeks and 48 of those who were not had actually refused to participate, according to the Office Log, but answered "did not know" to the question on participation in the survey.

APPENDIX A7

Follow-Up Survey Instrument

Social Security # _____

Illinois Unemployment Insurance Experiments

Hiring-Incentive Follow-up Survey

You may remember that in _____ (month) _____,¹ 1984 when you applied for unemployment benefits you were given an opportunity to take part in an experimental program that would pay your employer \$500 if you obtained a job within 11 weeks of the time you filed for benefits.

Q1. Did you agree to take part in this program?

1. Yes. -----> Go to Q5.
2. No. -----> Go to Q4.
3. DK/DR/RA.² -----> Go to Q2.

Q2. PROMPT: When you applied for unemployment benefits last _____ (month) _____, you were sent to a job service specialist who described to you a special program and asked if you would sign an agreement to take part in this program.

Do you remember being given the chance to take part in this program?

1. Yes. -----> Go to Q3.
2. No. -----> Go to Q10.
3. DK/DR/RA. -----> Go to Q10.

1. Month in which individual filed an unemployment claim will be supplied by the Institute along with telephone number and other data.

2. Don't know/Don't remember/Refuse to answer.

Q3. Did you agree to take part in this program?

1. Yes. -----> Go to Q5.
2. No. -----> Go to Q4.
3. DK/DR/RA. -----> Go to Q10.

Q4. Why did you not agree to take part in the program?

1. Did not want prospective employers to know I was unemployed.
2. Did not want prospective employers to know I was receiving unemployment benefits.
3. Expected to go back to same job and didn't see any reason to give money to employer.
4. Demeaning to offer employer money to hire me.
5. Opposed to giving money to employer to hire anyone.
6. Opposed to giving bonuses to anyone.
7. Didn't understand the program or the instructions.
8. Suspicious of the program or thought it might jeopardize benefits.
9. Was pregnant or had child care responsibilities.
10. Didn't expect to get a job.
11. Didn't want to bother with paperwork.
12. Wanted to get job on own.
13. Other. (Please specify:_____.)
14. DK/DR/RA.

Go to Q10.

Q5. Did you tell any employer who interviewed you for a job about the bonus offer?

1. Yes. -----> Go to Q7.
2. No. -----> Go to Q6.
3. DK/DR/RA. -----> Go to Q7.

Q6. Why didn't you tell any employer about the bonus offer?

1. Didn't interview for any job within the 11 weeks.
2. Forgot about the bonus offer.
3. Didn't understand how to use the bonus offer.
4. Did not want prospective employers to know I was unemployed.
5. Did not want prospective employers to know I was receiving unemployment benefits.
6. Expected to go back to same job and didn't see any reason to give money to employer.
7. Demeaning to offer employer money to hire me.
8. Opposed to giving money to employer to hire anyone.
9. Didn't understand the program or the instructions.
10. Suspicious of the program or thought it might jeopardize benefits.
11. Was pregnant or had child care responsibilities.
12. Didn't expect to get a job.
13. Other. (Please specify:_____.)
14. DK/DR/RA.

Go to Q7.

Q7. Did the employer that hired you ask you to sign a "notice of hire"? (PROMPT: The "notice of hire" is the preliminary form that the employer had to submit to qualify for the \$500 bonus.)

1. Yes.
2. No.
3. Not hired.
4. DK/DR/RA.

Go to Q8.

Q8. Do you feel that the offer of the bonus helped you get a job?

1. Yes. -----> Go to Q10.
2. No. -----> Go to Q9.
3. DK/DR/RA. -----> Go to Q10.

Q9. Why do you think the offer of a bonus did not help you get a job?

1. The employer rejected the bonus offer.
2. I would have gotten the job anyway.
3. Someone else got the job.
4. The employer decided not to fill that vacancy.
5. Other. (Please specify:_____.)
6. DK/DR/RA.

Go to Q10.

Q10. Did you get a job within the 11 weeks after you filed for unemployment benefits back in _____ (month) _____, 1984?

1. Yes.
2. No.
3. DK/DR/RA.

Go to Q11.

Q11. When you applied for unemployment benefits, were you married and living with your spouse, separated, living with someone as if you were married, single, widowed, or divorced?

1. Married, living with spouse.
2. Separated.
3. Living with someone as if married.
4. Single.
5. Widowed.
6. Divorced.
7. DK/DR/RA.

Go to Q12.

Q12. Since that time (that is, when you applied for unemployment benefits), has your marital status or living arrangement changed?

1. Yes. -----> Go to Q13.
2. No. -----> Go to Q14.
3. DK/DR/RA. -----> Go to Q13.

Q13. Are you currently married and living with your spouse, separated, living with someone as if you were married, single, widowed, or divorced?

1. Married, living with spouse.
2. Separated.
3. Living with someone as if married.
4. Single.
5. Widowed.
6. Divorced.
7. DK/DR/RA.

Go to Q14.

Q14. Is English the language most often spoken in your home?

1. Yes
2. No.
3. DK/DR/RA.

INTERVIEWER: Did the respondent have significant difficulty with the interview because of language?

1. Yes.
2. No.

Social Security # _____

Illinois Unemployment Insurance Experiments

Job-Search Incentive Follow-Up Survey

You may remember that in _____ (month) _____,¹ 1984 when you applied for unemployment benefits you were given an opportunity to take part in an experimental program that would pay you \$500 if you obtained a job within 11 weeks of the time you filed for benefits.

Q1. Did you agree to take part in this program?

1. Yes. -----> Go to Q5.
2. No. -----> Go to Q4.
3. DK/DR/RA.² -----> Go to Q2.

Q2. PROMPT: When you applied for unemployment benefits last _____ (month) _____, you were sent to a job service specialist who described to you a special program and asked if you would sign an agreement to take part in this program.

Do you remember being given the chance to participate in this program?

1. Yes. -----> Go to Q3.
2. No. -----> Go to Q7.
3. No answer. -----> Go to Q7.

1. Month in which individual filed an unemployment insurance claim will be supplied by the Institute along with telephone number and other data.

2. Don't know/Don't remember/Refuse to answer.

Q3. Did you agree to participate in this program.

1. Yes. -----> Go to Q5.
2. No. -----> Go to Q4.
3. DK/DR/RA. -----> Go to Q5.

Q4. Why did you not agree to take part in the program?

1. Did not want prospective employers to know I was unemployed.
2. Did not want prospective employers to know I was receiving unemployment benefits.
3. Opposed to giving money to someone for getting a job.
4. Didn't understand the program or the instructions.
5. Suspicious of the program or thought it might jeopardize benefits.
6. Was pregnant or had child care responsibilities.
7. Didn't expect to look for or to get a job.
8. Did not want to bother with paperwork.
9. Other. (Please specify:_____.)
10. DK/DR/RA.

Go to Q7.

Q5. Did you submit a "notice of hire" form to the Department of Employment Security? (PROMPT IF NECESSARY: The "notice of hire" is the preliminary form that you had to submit after finding a job, in order to qualify for a bonus.)

1. Yes. -----> Go to Q7.
2. No. -----> Go to Q6.
3. DK/DR/RA. -----> Go to Q7.

Q6. Why didn't you submit a "notice of hire" form?

1. Did not think I qualified for a bonus.
2. Forgot about it.
3. Did not understand the procedures.
4. Did not want to ask my employer to sign the form.
5. Employer refused to sign the form.
6. Other. (Please specify:_____.)
7. DK/DR/RA.

Go to Q7.

Q7. Did you get a job within 11 weeks of when you applied for unemployment benefits back in ____ (month) ____, 1984?

1. Yes.
2. No.
3. DK/DR/RA.

Go to Q8.

Q8. At the time you applied for unemployment benefits, were you married and living with your spouse, separated, living with someone as if you were married, single, widowed, or divorced?

1. Married, living with spouse.
2. Separated.
3. Living with someone as if married.
4. Single.
5. Widowed.
6. Divorced.
7. DK/DR/RA.

Go to Q9.

Q9. Since that time (that is, when you applied for unemployment benefits), has your marital status or living arrangement changed?

1. Yes. -----> Go to Q10.
2. No. -----> Go to Q11.
3. DK/DR/RA. -----> Go to Q10.

Q10. Are you currently married and living with your spouse, separated, living with someone as if you were married, single, widowed, or divorced?

1. Married, living with spouse.
2. Separated.
3. Living with someone as if married.
4. Single.
5. Widowed.
6. Divorced.
7. DK/DR/RA.

Go to Q11.

Q11. Is English the language most often spoken in your home?

1. Yes
2. No.
3. DK/DR/RA.

INTERVIEWER: Did the respondent have significant difficulty with the interview because of language?

1. Yes.
2. No.

Chapter 8

SUMMARY OF RESULTS AND POLICY IMPLICATIONS

The Illinois Unemployment Insurance Experiments tested two approaches to reducing the burden on the Unemployment Insurance Trust Fund by shortening UI claimants' spells of insured unemployment. Each approach was represented by a separate experiment. The Hiring Incentive Experiment (HIE) was designed to determine if bonuses paid to employers would increase the demand for UI beneficiaries, and hence speed up the hiring of UI beneficiaries. The Job Search Incentive Experiment (JSIE) was designed to determine if bonuses paid to UI beneficiaries would increase their job search effort and shorten the duration of their spell of insured unemployment. In this final chapter, we will present a summary of the experiments and their results, and some implications of these results for policy.

I. Summary of the Experiments and the Results

As described in Chapter 2, the experimental design consisted of the following elements: eligibility criteria for participation; components of the treatments; sample size; and site selection. The decision was made to restrict eligibility to a subset of UI claimants in order to make the experimental population more homogeneous, and to concentrate on those most likely to be part of a program if implemented. Thus, the following groups of UI claimants were excluded from the experiment: those who were not filing an initial claim; those who were less than 20, or more than 54 years of age; those who did not register with the Job Service, because they were on layoff with a definite recall date or members of unions who obtained employment through hiring halls; and those who were designated as recently separated veterans (UCX) or recent federal employees (UCFE).

The two experiments had very similar designs, except that the bonus recipient differed between the two. In the HIE, an employer was eligible to receive a \$500 cash payment by hiring a worker who:

- was an initial claimant for UI benefits, was eligible to receive benefits, and had agreed to participate in the experiment;
- was hired before receiving 11 weeks of benefits;
- remained employed continuously for 4 months; and
- worked on the job for 30 or more hours per week.

In the JSIE, a UI claimant meeting the same four criteria was eligible to receive a \$500 cash payment.

The most important feature of the sample design was that claimants were randomly assigned to the two experiments and a control group. Using claimants' Social Security numbers to randomly assign them to treatments, over 17,000 claimants were processed in 22 Job Service offices in northern and central Illinois, including Chicago. These offices served a widely diversified set of clients, covering just over 60 percent of the state's labor force. Comparing information collected on all of the assigned claimants shows that no social or demographic characteristic differed across the three groups, which indicates that the randomization process was successful in creating three random samples: one for each of the two experiments, and one for the control group.

If an eligible claimant was assigned to one of the experimental treatments, a Job Service specialist described the treatment to the claimant and asked him or her to sign an agreement to participate (Chapter 3). Each claimant who agreed to participate and had a valid UI claim was enrolled in the experiment and sent a packet of materials. For the HIE, the packet contained a "Notice of Hire" to be submitted by an employer that hired the participant within the qualification period. For the JSIE, the packet contained a Notice of Hire to be submitted by the claimant. Upon completing 4 months of continuous employment, the employer (in the HIE), or the claimant (in the JSIE) would submit a voucher and receive payment, provided that the Illinois Department of Employment Security (IDES) verified that the conditions of the experiment had been met.

To evaluate the experiments, seven data bases were used (Chapter 4). Three derived from instruments constructed specifically for the experiments, and the other four were administrative data bases of the IDES. The three instruments constructed for the experiments were: the Base Line Survey (which contains Social Security numbers, treatment assignments, and some family status information); the Office Logs (which include data on treatment assignments and agreement to participate); and the Telephone Follow-Up Survey (which contains information for a sample of eligibles, and was designed to discern reasons for non-participation). The four IDES administrative data bases were: the Benefits Information System (data on claims and benefit receipts); the Wage Records (quarterly earnings data); the Contributions Tax System (data on employers of each claimant); and The Employment Security Automated Reporting System (data on Job Service registration and activities).

The results of the HIE are complex (Chapter 5). The treatment reduced benefit payments and weeks of insured unemployment in the spell of unemployment immediately following the initial claim. This finding in itself is remarkable, because only a small percentage of eligible claimants participated in the experiment--employers of only 3 percent of the 3,963 eligible claimants assigned to the HIE collected bonuses--and the effects are

measured over the entire sample of eligible claimants. The HIE, however, had no statistically significant effects on benefits paid or weeks of unemployment over the full benefit year, which we regard as the more important finding.

In contrast, the effect of the JSIE on benefit receipts and the duration of insured unemployment appears to have been very strong. For the sample of eligible claimants assigned to the JSIE, benefit payments over the full benefit year were reduced by \$158. The number of weeks of insured unemployment experienced by the JSIE group was 1.15 weeks less than the number experienced by controls. It should be kept in mind that these effects were achieved over the entire sample of 4,186 eligible claimants, only 570 of whom collected bonuses.

An important question that we have addressed (in Chapter 5) is whether the shorter duration of unemployment achieved in the JSIE resulted in a less favorable match between workers and jobs. In other words, in order to obtain more rapid reemployment, did JSIE participants accept less satisfactory jobs? We answered this question by comparing the earnings of JSIE eligibles with controls after reemployment. We found no difference between the post-reemployment earnings of these two groups.

Examination of several different subgroups of claimants disclosed some important findings masked by the aggregate results (Chapter 6). The most startling result to emerge from disaggregation was the strong effect of the HIE on white women. The HIE caused a \$164 decline in the benefits paid to white women, and a one-week reduction in their weeks of insured unemployment, both over the full benefit year. (These are average effects for the entire group of HIE eligibles.) The reason for the racial difference in the effects of the HIE can be found in the great difference between the participation rates of whites and blacks. Employers of white women were more than six times as likely to submit a Notice of Hire as employers of black women. In fact, employers submitted Notices of Hire for only 7 black women in the HIE. The experiment, however, does not provide any direct evidence about why white women were affected by the HIE, whereas white men were not.

The disaggregated results establish four other points about the HIE. First, the HIE affected workers in their early 30s more strongly than it affected other workers. Second, the effects of the HIE did not vary with claimants' level of education, which is surprising given that the HIE required claimants to inform prospective employers about the bonus offer. Third, there is some evidence that workers in Clerical and Sales occupations were more strongly affected than other workers. Fourth, workers who found jobs in Wholesale and Retail Trade were more affected by the HIE than were workers who found jobs in other industries.

Since the overall effects of the JSIE were so much stronger than those of the HIE, we would expect a broader set of subgroups to be affected by the JSIE, and this was the case. Both men and women were affected by the JSIE. However, blacks were affected less than whites, and black men may not have been affected. It seems likely that the weak effect of the JSIE on black men was the result of less participation, as evidenced by the lower rate at which Notices of Hire were submitted or vouchers cashed (Table 7-2). We do not know if the lower participation rate of black men was the result of less intense job search, or greater difficulty in obtaining job offers.

Other points about the JSIE emerge from the disaggregated results. First, JSIE enrollees under 35 responded more strongly than other age groups. Second, high school graduates (without further schooling) responded more strongly than claimants of other educational levels. Third, there were some differences by occupation, with those least affected being workers in white collar occupations requiring high skill and greater education. Fourth, the JSIE was ineffective for claimants with very low earnings (below \$1,000 per quarter), and very high earnings (above \$9,000 per quarter). Finally, the JSIE had the greatest effect on claimants who found reemployment in either of two fast growing industries--Wholesale and Retail Trade, and Finance, Insurance, and Real Estate.

Overall, the evaluation shows that the JSIE was remarkably successful in achieving its results. The reduction in benefit payments achieved by the JSIE translates into a net saving of \$2.32 for the UI Trust Fund for every \$1 spent on bonuses.

Before accepting the results of the JSIE as conclusive, it is important to ascertain whether the experiment was internally valid; i.e., that its results were unbiased. Several events or conditions could have led to biased results (see Chapter 5, Section V). First, there could have been learning effects bias; that is, error in measurement by not allowing sufficient time for changes in the behavior of experimental participants that would have occurred if the experiment had been conducted over a longer period. Second, there could have been "Hawthorne Effects," that is, unintended treatments introduced by the way the experiment was administered. Third, selective attrition would have biased the comparisons between experimental and control groups, if only participants had been compared with controls. Lastly, the results could have been upward biased if the increased job search effort of JSIE participants led to a reduction in job acquisition by controls. We have concluded that none of these potentially biasing effects existed to any measurable degree in the JSIE, and that the JSIE results are internally valid. The HIE results, however, may be biased downward somewhat because of learning effects.

II. Transferring the Experimental Results to a Program

A program modelled on either the HIE or the JSIE could differ from its experimental prototype in at least three ways: First, administration of a program could differ from that of an experiment. Second, claimant and employer participation in a program could differ from participation in an experiment. Third, greater displacement might occur in a full-scale program than in an experiment because of the larger number of participants in a program. These differences could lead in turn to a program's results differing from the results observed in the experimental prototype. In this section, we discuss each of the differences and how they can be expected to affect the results of programs based on the HIE and JSIE.

A. Program Administration

Administration of the HIE and JSIE was more complex than would be necessary in an actual program modelled on either experiment. In the experiments, logs were kept of those offered participation in the experiments, indicating whether each claimant agreed to participate, whether he or she was eligible to participate, and whether he or she submitted a Notice of Hire, or received a bonus. Each claimant who was offered the opportunity to participate had to sign an agreement in order to participate, and was subsequently notified of his or her eligibility to participate in the experiment. Each eligible claimant was mailed a packet of materials. In the JSIE, participants who obtained employment within 11 weeks of filing would submit a Notice of Hire. The participant then received a voucher from IDES, which was submitted for payment after 4 months of continued full-time employment. The HIE was similar, except that the employer submitted the Notice and the voucher and received payment.

In a program based on either the HIE or JSIE, all initial claimants eligible to receive UI benefits and required to register with the Job Service would be eligible to participate in the bonus program. A notice informing new claimants of the bonus offer (conditional on being found eligible for UI benefits) could be given to each claimant at the time of registering with the Job Service together with a voucher that would be sent to the IDES (by the claimant or the employer, depending on the program) if the claimant became employed within 11 weeks of filing and retained the job for 4 months. Upon receipt of the voucher, IDES would verify that benefit payments conformed to the required pattern, and would issue a check. This simple procedure would require little additional administrative expense for the IDES, as a straightforward verification program could be installed in the claimant data base.

A difficult issue in designing a program based on either the HIE or the JSIE is whether claimants on layoff who are recalled to their previous job should be eligible for the bonus. If such claimants were eligible for the bonus, bonuses would be paid in many cases in which job search behavior and unemployment duration would be unchanged. Consideration should be given to excluding recalled claimants from bonus eligibility, although implementing such an exclusion raises complex administrative issues.

The extent of the claimant's involvement is an important issue in designing a program modelled on the HIE. If the purpose of the program is to increase job search effort, as well as placement, then the vouchering approach, in which the claimant is informed of the program and carries the voucher to the employer, should be maintained. The Notice of Hire could be omitted from the process, although the employer would still need to submit a voucher to receive payment after the eligible claimant had been employed for 4 months.

B. Changes in Participation in a Permanent Program

HIE. For the employer experiment, the important question is whether the extremely low rate of participation would be replicated in a full program. Notices of Hire were filed by employers for only 14 percent of UI claimants who were eligible to participate in the experiment and who terminated their benefits within 11 weeks of filing. The Follow-Up Survey showed that 60 percent of those claimants offered enrollment in the HIE either refused outright to participate or simply did not notify any employers about the bonus offer (see Table 7-6). Lack of prior knowledge about the experimental program, and thus its lack of credibility, undoubtedly kept claimant and employer participation in this experiment lower than it would be in a full program. It is likely that participation in a real program would be higher.

Nevertheless, social programs have never achieved 100 percent participation. Robert Moffitt has reported that in 1970, only 69 percent of eligible families participated in Aid to Families with Dependent Children (AFDC), only 43 percent of eligible families with an unemployed male participated in the AFDC-U program, and the participation rate among eligibles in the Food Stamp program was only 38 percent.¹

For the HIE, there are specific reasons for expecting less than 100 percent use under the best of circumstances. The Follow-Up Survey indicated that some UI claimants were concerned about stigma, i.e., did not want employers to know of their UI status, or to know that they were unemployed (Table 7-7). Others expressed the belief that they did not want employers to get

1. Robert Moffitt, "An Economic Model of Welfare Stigma," American Economic Review, 73 (September 1983), pp. 1023-1035.

bonuses for hiring them. These negative feelings about an employer bonus can be expected to diminish over time for some of the claimants, but certainly not for all of them.

Participation by employers in the HIE was also very low. Part of the reason must rest with the method of communication. In the HIE, employers received all information about the program through the voucher-carrying claimant. The ability of the claimant to accurately and persuasively communicate the content of the program to prospective employers is questionable. Furthermore, the absence of any other source of information about the program undoubtedly reduced the credibility of the program. Although the employer was provided with a letter from the IDHS and a phone number that could be used to verify the authenticity of the offer, many prospective employers undoubtedly did not make the effort to determine the validity of the offer.

In a program based on the HIE, information about the voucher would be provided through news media, announcements by the IDHS, and communication among employers who knew about or had used the program. The provision of accurate and continually reinforcing information about the program should serve to establish its credibility among employers. It seems reasonable that as employers became more familiar with the program they would become more receptive, and might seek out job applicants who would tend to make the employer eligible for a bonus.

There are additional reasons, besides the method of communication, why employers might resist a program based on the HIE. Many employers would not want to become involved with government red tape for such a small sum of money. Although we do not know whether increased exposure to the bonus program over time will lead to changes in the take-up rate of employers, we can glean some indication from the results of the Targeted Jobs Tax Credit (TJTC).

The TJTC provided tax credits to business equal to 50 percent of the first \$6,000 in wages during the first year of employment, and 25 percent in the second year, for the hiring of economically disadvantaged youth. The Congressional Budget Office has estimated that in 1983 only about 4 percent of the nation's employers, who provided fewer than 20 percent of the nation's jobs, were participating in TJTC.²

John Bishop has concluded that a strong outreach program and better education could have doubled the participation rate in TJTC. Although TJTC is not directly comparable to the HIE, the

2. See John Bishop, Subsidizing the Hiring and Training of the Disadvantaged, The W. E. Upjohn Institute for Employment Research, Kalamazoo, MI, forthcoming, draft p. 24.

indications from analysis of TJTC would lead us to expect that the participation rate for eligible UI claimants would increase, and perhaps substantially, in a permanent program modelled on the HIE.

JSIE. Participation in the JSIE was much higher than in the HIE. We estimate that 56 percent of those eligible to submit Notices of Hire (because they were eligible for the JSIE and found a job within 11 weeks) actually did so (see Table 7-6). Participation in a program modelled on the JSIE would be more straightforward than one modelled on the HIE. It would not require action by employers. It would not impose on claimants the need to expose their UI status to employers, nor would it impose the psychic costs on UI claimants of offering money to employers to hire them. It follows that we can expect the eventual participation rate in a program modelled on the JSIE to be much higher than in a program modelled on the HIE. Nevertheless, based on evidence of participation in other social programs, it remains unlikely that participation in a program modelled on the JSIE would ever reach 100 percent.

C. Displacement in the Program Environment

Displacement occurs if program participants obtain their jobs at the expense of other workers. If this occurs, the benefits to society as a whole (net social benefits) are less than the private benefits to the program participants. As George Johnson has pointed out, however, the determination of the net social benefit from a jobs program is not straightforward.³

For instance, in a retraining program, the net social benefits of moving a worker from Industry A to Industry B will be greater than the private benefits if the unemployment rate in A is higher than in B. The social benefits will be less than the private benefits if the reverse is true.

Although the JSIE and HIE cause movement from unemployment to employment, rather than from Industry A to B, they may also generate social benefits that exceed private benefits. For the JSIE, social benefits derive from the increased speed of filling job vacancies as a result of increased job search effort. The social benefits of the HIE derive both from increased job search effort and from (possible) net job creation caused by the bonus offer.

HIE. As discussed in Chapter 5, the internal validity of the HIE would have been compromised if HIE participants were able to obtain jobs at the expense of the controls, thereby reducing the

3. See G. Johnson, "The Labor Market Displacement Effect in the Analysis of the Net Impact of Manpower Training Programs," in Bloch, Farrell E., ed., Evaluating Manpower Training Programs, Research in Labor Economics, Supplement, 1979, JAI Press.

reemployment rate of controls. In fact, the number of control group members was sufficiently small relative to the total number of job seekers (i.e., no more than 1 percent) that it was unlikely that any single job taken by an HIE participant due to the experiment would affect a control group member. 4

The concern over displacement in a program based on the HIE stems from the observation that employers have an incentive to substitute voucher-carrying claimants for other similarly qualified job seekers. Conceivably, there could be a one-for-one displacement of nonparticipants by participants. However, two attributes of the HIE-type program would cause less than one-for-one displacement. First, the voucher-carrying claimant would speed up his or her job search to be able to use the voucher, and would thereby reduce the amount of frictional unemployment overall.

Second, a program modelled on the HIE has the potential to generate new jobs. The \$500 bonus to employers would lower the marginal cost of employing new workers, and may induce more hiring or more rapid filling of vacancies. Although the experiment does not provide any direct information as to how many jobs would be created by an HIE-type program, other wage-subsidy programs provide some evidence. For example, John Bishop has estimated that the Targeted Jobs Tax Credit (TJTC) created 30 new jobs for every 100 workers who were subsidized by the program.⁵ If a dollar spent on an HIE-type program were to have the same job-creating effect as a dollar spent on the TJTC, then the HIE-type program would create 3.3 jobs for every 100 workers for whom a bonus was paid.⁶ In such a case, the added earnings created by the new jobs would more than cover the cost of the \$500 bonuses. (If the average job created paid an annual salary of \$18,000, then a \$50,000 expenditure on 100 bonuses would create 3.3 jobs paying a total of \$59,400.) Thus, the net social benefits of the program would be positive.

4. During 1984 in Illinois, the average number of unemployed workers per week was about 512,000 (U.S. Department of Labor, Bureau of Labor Statistics, Employment and Earnings). Since the labor market covered by the 22 Job Service Offices involved in the experiment provided about 60 percent of the 4.7 million jobs in the state, we estimate that the number of unemployed in our market area would have been about 300,000 per week. Since the total number of controls numbered 4,000 (spread over a 16-week period), it is highly unlikely that controls were more than 1 percent of the unemployed at any one time.

5. Bishop, op. cit., p. 164.

6. The TJTC subsidy could pay \$4,500 per worker over a two-year period. An HIE-type program paying a one-time \$500 bonus provides only one-ninth as large a subsidy, and could be expected to generate only one-ninth as many jobs ($\$500/\$4,500 \times 30 = 3.3$).

JSIE. Displacement could also occur in a program based on the JSIE. The reduction in the duration of unemployment by program participants would result in participants filling job vacancies that could have been filled by nonparticipants. However, the displacement would be considerably less than one-for-one, because the increased job search effort of participants would speed the process of job matching, thereby lowering the frictional rate of unemployment.

III. Further Research

The results of the JSIE suggest that bonuses offered to workers are a promising means of increasing job search intensity, efficiently reducing unemployment insurance costs, and reducing the total amount of unemployment in the economy. The large measured impact of the JSIE on weeks of insured unemployment suggests that there is sufficient voluntary unemployment during a spell of insured unemployment to make a job search incentive program effective. However, it would be wrong to conclude from these findings that alternative reforms of the UI system--for example, imposing more stringent work-search requirements--would be equally effective in reducing unemployment. Also, we are reluctant to generalize the results of the JSIE to other possible work-search incentive policies. For example, lump-sum payment of a large portion of the maximum benefit amount at the time eligibility is determined--even if the payment were equal to the bonus of \$500--could be expected to produce different results.

Further tests of bonus payments to claimants would be highly desirable in order to verify that the results were not unique to the time and place in which the JSIE was conducted. Moreover, as indicated by general theoretical considerations, it would be important to determine which combination of bonus level and qualification period would result in the highest net benefit. Making such a determination would require an experiment in which both the bonus level and the qualification period were variables.

For the HIE, more testing is clearly necessary before any conclusions can be reached. The low participation rate precludes any firm conclusions about the likely benefits of a program modelled on the HIE. However, that the program was effective for white women suggests that bonus payments to employers might be an efficient approach. In further tests, methods of better informing employers of the program and of providing greater incentive for claimants to use the voucher need to be devised. It is possible, however, that the likelihood of a high degree of displacement of nonparticipants by participants makes a program modelled on the HIE less appealing than other options.

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