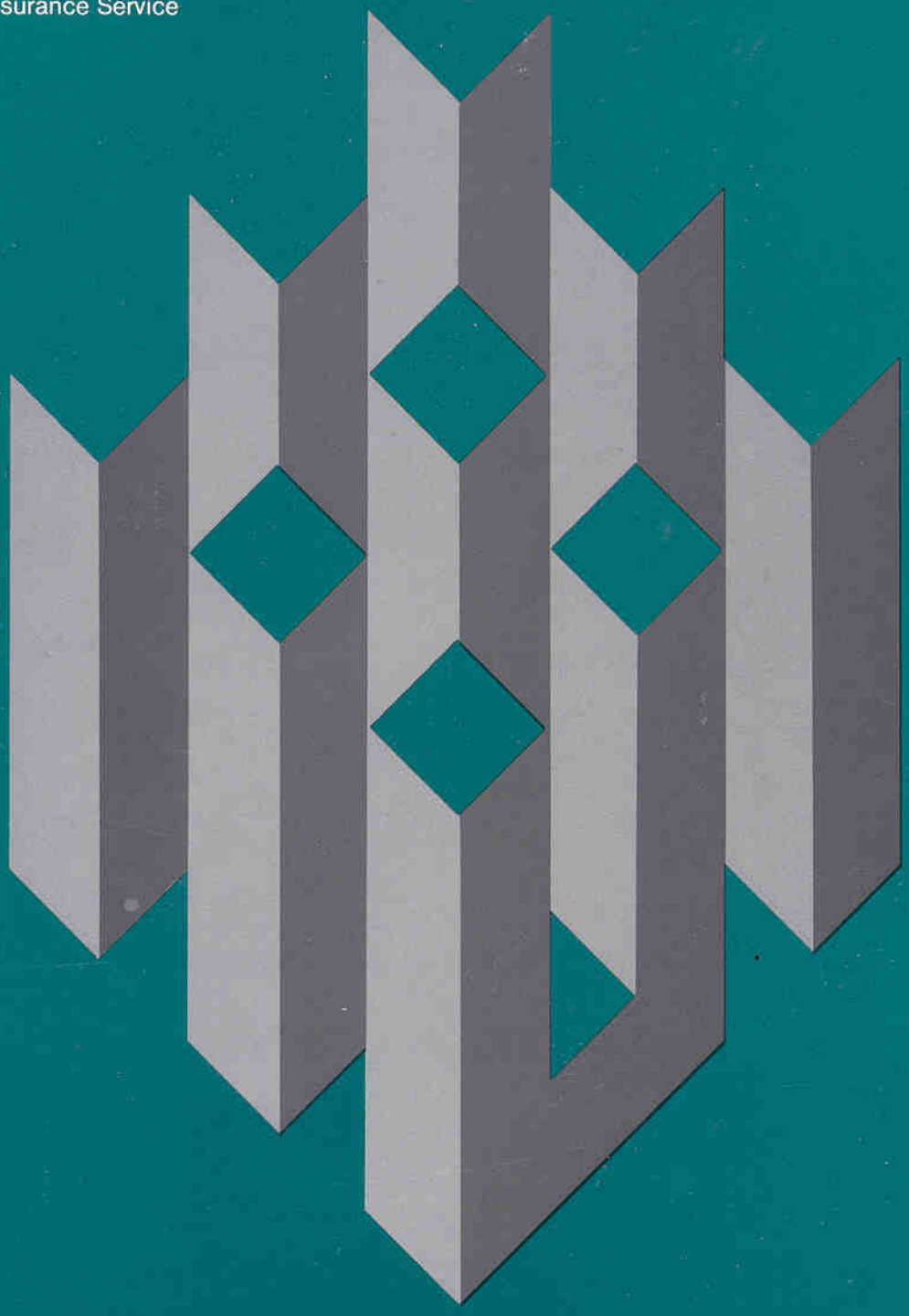
Pennsylvania Reemployment Bonus Demonstration Final Report

Unemployment Insurance
Occasional Paper 92-1

U.S. Department of Labor
Employment and Training Administration
Unemployment Insurance Service



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Occasional Paper 92-1

U.S. Department of Labor
Lynn Martin, Secretary

Employment and Training Administration
Roberts T. Jones
Assistant Secretary of Labor

Unemployment Insurance Service
Mary Ann Wyrsch, Director
1992

This report was prepared for the Unemployment Insurance Service, U.S. Department of Labor under Cooperative Agreement Number 99-7-0805-04-137-01 with the Pennsylvania Department of Labor and Mathematica Policy Research (subcontractor). The authors of this report are Walter Corson, Paul Decker, Shari Dunstan, and Stuart Kerachsky. Since contractors conducting research and evaluation projects under the government sponsorship are encouraged to express their own judgments freely, this report does not necessarily represent the official opinion or policy of the U.S. Department of Labor.

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PENNSYLVANIA REEMPLOYMENT BONUS DEMONSTRATION

FINAL REPORT

September 1991

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This report was prepared for the U.S. Department of Labor, Employment and Training Administration, Unemployment Insurance Service under contract number 99-7-0805-04-137-01 with Mathematica Policy Research, Inc. Since contractors that conduct research under government sponsorship are encouraged to express their own judgements freely, this report does not necessarily represent the official opinion or policy of the U. S. Department of Labor. The contractor is solely responsible for the contents of this report.

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At USDOL William Coyne served as the project officer initially and guided the project through its design and early implementation phases. His replacement, Wayne Zajac, guided the project through its operational and analysis phases. These two individuals together with Stephen Wandner, who provided overall guidance to the project at the federal level, provided valuable input to the demonstration and research designs and to the analysis.

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Principal Investigators

CONTENTS

CONTENTS (continued)

CONTENTS (continued)

CONTENTS (continued)

TABLES

TABLES (continued)

TABLES (continued)

TABLES (continued)

FIGURES

EXHIBITS

EXECUTIVE SUMMARY

The Unemployment Insurance (UI) program provides short-term income support to involuntarily unemployed individuals while they seek work. To promote rapid reemployment, the program currently uses work-search requirements and employment-service referrals; however, policy interest has recently been expressed in providing additional job-search assistance and other employment-oriented services to UI claimants, including additional monetary incentives for claimants to seek work on their own. These monetary incentives could be provided in the form of a reemployment bonus--a lump-sum benefit paid to those who become reemployed or self-employed quickly. A reemployment bonus would compensate for the reemployment disincentives inherent in the regular UI system, which pays benefits to claimants for the weeks in which they remain unemployed.

The Pennsylvania Reemployment Bonus Demonstration tested the effect of alternative reemployment bonuses on the reemployment and UI receipt of UI claimants. The demonstration showed that reemployment bonuses can reduce the amount of time spent on UI, thereby reducing benefit payments. The demonstration also provided some evidence that the bonus offers increased the employment and earnings of claimants. Overall, the demonstration showed that the benefits of reemployment bonuses can exceed their costs to society, claimants, and the government. However, for all the bonus offers tested, the amount of the bonus payments plus their administrative costs exceeded the savings in UI payments. Thus, reemployment bonuses do not appear to be cost-effective from the standpoint of the UI system itself.

DESIGN OF THE DEMONSTRATION

The Pennsylvania Reemployment Bonus Demonstration was designed to test the effect of alternative parameters of a reemployment bonus offer, including the amount of the bonuses that are offered and the time period in which the bonus offer remains valid--the qualification period. It tested six treatments, or bonus-offer variations, according to a design in which eligible claimants were assigned randomly to the six treatments and to a control group that was not offered a bonus. The purpose of this random assignment design was to allow differences in the behavior of claimants in the treatment groups to be attributed to the effect of the treatments with a known degree of statistical precision.

The six treatments were chosen to bracket the policy-relevant range of reemployment bonus options. Four treatments incorporated two bonus offer amounts and two qualification periods. The two bonus offers that were tested included a low offer (three times the weekly benefit amount, or an average of $500) and a high offer (six times the weekly benefit amount, or an average of $1,000). The two qualification periods that were tested included a short period (6 weeks) and a long period (12 weeks) beginning with the bonus offer date. One additional treatment tested a bonus offer that declined gradually from the high offer amount over a 12-week qualification period, thus giving claimants an incentive to become reemployed as quickly as possible within the 12-week period.

A special element of the demonstration was the offer of a job-search workshop. The workshop was offered in combination with the five bonus offers, in the expectation that some claimants might not be able to take full advantage of the bonus because they lacked job-finding skills. To help isolate the added benefits of offering the job-search workshop, the demonstration included a sixth treatment

in which claimants were offered the most generous bonus (a high bonus amount and long qualification period) but not the job-search workshop.

Another important element of the demonstration design was that the bonus offer was to be offered only to UI-eligible claimants who were not job-attached. For this reason, claimants who had a definite recall date within 60 days or who obtained jobs exclusively through their union were excluded from the demonstration. A few special categories of claimants were also excluded. In order to apply these eligibility requirements, the bonuses were offered one or two weeks after the initial UI application, when claimants signed for their waiting week or their first week of benefits.

Claimants who started a job within the qualification period could receive a bonus if they maintained employment for 16 consecutive weeks. Bonus were not paid to claimants who were recalled by their pre-layoff employers or to claimants who were determined to be ineligible for UI benefits prior to their reemployment.

THE IMPLEMENTATION OF THE DEMONSTRATION

Demonstration-eligible claimants were selected and bonus offers were made between July 1988 and October 1989, a period in which the Pennsylvania economy was quite strong (employment was growing, and the average unemployment rate was 4.5 percent). Over this period, 15,005 eligible claimants were selected and assigned to treatment and control groups. Of these claimants, 14,086 signed for the waiting week or received a first payment and were thus eligible to receive a bonus offer if they were assigned to a treatment group. These claimants were located in 12 offices throughout the state. The 12 offices were selected randomly to represent the UI population in the state.

The demonstration procedures were, in general, implemented as planned. Findings on eligibility determination, bonus offers, the job-search workshop, and bonus claims and payments are as follows:

Eligibility Determination

- Eligible claimants were correctly identified and were assigned randomly to the treatments and the control group. These eligible claimants constituted about half of the UI-eligible population. The most important screening criterion was the exclusion of claimants who had a definite recall date. Use of this screening criterion yielded eligible claimants that had shorter average job tenure and were less likely to have worked in manufacturing industries than were ineligible claimants.

- An analysis of the post-UI labor-market experience of eligible and ineligible claimants indicates that the selection criteria successfully directed bonus offers to claimants who generally were not job-attached.

Bonus Offers

- Ninety-four percent of the treatment group members who signed for a waiting week or first compensable week attended orientation and received a bonus offer. These offers were provided in a timely fashion (an average of about 17 days after the benefit application date).

- Based on site observations, we believe that claimants were largely offered the correct bonus parameters. Most claimants also appeared to understand the bonus offer, suggesting that the extent to which the bonus offer was understood was likely comparable to what would exist in an ongoing program.

Job-Search Workshop

- The participation rate for the demonstration job-search workshop was very low (under 3 percent). This low participation rate was due primarily to the general lack of interest among claimants, rather than to implementation problems.

- The low participation rate in the workshop may be due directly to the strong economy in which the demonstration operated and to the fact that the workshop was offered early in an individual's unemployment spell. Moreover, the fact that the bonus was offered at the same time that the workshop was offered may have provided a disincentive to participate in the workshop.

Bonus Claims and Payments

- Seven to eight percent of the claimants with short qualification periods and 10 to 14 percent of the claimants with long qualification periods received a bonus payment. The highest bonus receipt rates occurred for the most generous treatments. Overall, 11 percent of claimants received bonuses averaging $906.

- About 20 percent of the claimants who filed a claim for a bonus were judged to be ineligible, primarily because their job did not start within the qualification period, or because the individual was not eligible for UI. Other individuals who appeared to be eligible when they first became reemployed did not work for the required 16 weeks and thus did not receive a bonus.

- An important question is whether the bonus receipt rate found in the demonstration is comparable to what could be expected in an ongoing program. At most, about 6 percent of the claimants might have been eligible for a bonus for which they did not apply, making the maximum bonus receipt rate 17 percent.

IMPACTS ON UI RECEIPT

The demonstration treatments were expected to reduce UI benefit receipt among eligible claimants by inducing them to find reemployment quickly. We examined this hypothesis by comparing UI receipt among claimants who were assigned to one of the bonus treatments with UI receipt among claimants who were assigned to the control group. Our estimates based on these comparisons show that most of the bonus offers that were tested reduced UI receipt significantly over the benefit year.

The most generous bonus offer--the highest bonus amount for the longest qualification period--had the greatest impact on benefits, reducing average UI receipt by about 0.8 weeks, or by $130. In addition, this most generous bonus also reduced the proportion of claimants who exhausted their benefits. The more limited bonus offers--a smaller bonus amount, a shorter qualification period, or

a bonus that declined over time--reduced UI receipt by an average of about a half a week, or by $80, per claimant. These bonus offers did not affect the likelihood that benefits would be exhausted.

These estimated impacts of the bonuses did not vary significantly among most subgroups. However, impacts were significantly greater among claimants from manufacturing industries than among claimants from nonmanufacturing industries.

Further analysis indicated that the bonus offers significantly increased the rate of exit from UI during the bonus qualification periods. Moreover, most of the bonus offers reduced UI receipt more among relatively short-term claimants than among longer-term claimants, who were likely to exhaust their UI benefits.

IMPACTS ON EMPLOYMENT AND EARNINGS

Because the bonus offers reduced UI receipt, we expected to observe an increase in employment and earnings among claimants assigned to the treatments, and we did find some evidence that this increase occurred. Data from UI wage records showed that the treatments had no impact on employment, but that they had a generally positive but insignificant impact on earnings. Despite the lack of statistical significance of the earnings impact, the magnitudes of the estimates, which were generally between $0 and $200 for the year following benefit application, were consistent with the estimated impacts on UI receipt. The largest impacts occurred for the most generous bonus offer (a high bonus amount and long qualification period), for which we also observed the largest reduction in UI receipt.

Estimates based on the interview data provided somewhat stronger evidence that the treatments increased the employment and earnings of claimants. The interview-based estimates showed that the treatments had a positive but insignificant effect on employment, and that they had a positive and significant effect on earnings. According to these estimates, the bonus offers increased earnings by $200 to $550 in the year after the benefit application date; again, the most generous bonus offer had the largest impact. The interview data also suggested that the treatments increased earnings because they increased the likelihood of full-time employment.

There is no evidence that the bonus offers prompted claimants to take less desirable jobs in an effort to qualify for the bonus. None of the treatments had a negative impact on the hourly wage rate in the first post-unemployment job. In addition, the first post-unemployment jobs held by bonus-eligible claimants were similar to their pre-unemployment jobs in many respects, including their weekly wage rate.

BENEFIT-COST ANALYSIS

The bonus offers generally yielded net benefits to claimants and to society as a whole. Claimants responded to the bonus offers by giving up benefits that were approximately equal to the bonus payments that they received. Consequently, because claimants also experienced greater employment and earnings from having been offered the bonuses, they received net benefits from the bonus program. Society also received net benefits from the bonus program because the earnings gains exceeded the relatively low administrative costs of the program.

Although the bonus offers generated net benefits to claimants and to society, they were not cost-effective from the perspective of the UI system. The costs of administering and paying reemployment bonuses in the Pennsylvania demonstration generally exceeded the reduction in UI receipt caused by the bonus offers. The bonus offers thus yielded modest net losses for the UI trust funds. Nevertheless the taxes on increased earnings generated net benefits to the government as a whole for two of the bonus offers (the low bonus amount and long qualification period, and the high bonus amount and short qualification period). The government either broke even or incurred only a modest loss for the other treatments.

I. INTRODUCTION AND DEMONSTRATION DESIGN

The Unemployment Insurance (UI) program provides short-term income support to involuntarily unemployed individuals while they seek work. Historically, the UI program has used administrative work-search requirements and employment-service referrals to the Job Service (JS) to promote rapid reemployment. In recent years, however, considerable policy interest has been expressed in providing additional job-search assistance and other employment services to UI claimants, including additional monetary incentives for claimants to seek work on their own. These additional monetary incentives could be provided in the form of a reemployment bonus--a lump-sum benefit paid to those who become reemployed or self-employed quickly. This "reemployment bonus" concept provides very different incentives than the regular UI system, which pays benefits to claimants for the weeks they remain unemployed.

The Pennsylvania Reemployment Bonus Demonstration provides a test of the reemployment bonus concept to support policy development along four dimensions:

- The extent to which a bonus offer can accelerate the job-search activities of claimants and encourage them to become reemployed more rapidly
- The magnitude of any reduction in the amount of UI benefits received and whether the savings in UI benefits exceed the cost of the bonuses
- The effect of changes in the structure of the bonus offer on employment, earnings, UI receipt, and cost-effectiveness
- Program targeting and implementation issues

In general, the goal of the demonstration was to provide policymakers with sufficient information to design an optimum bonus offer.

This chapter discusses how the Pennsylvania Reemployment Bonus Demonstration was designed to inform policy development. Section A provides an overview of the design of the demonstration and discusses how the design was developed according to expectations about the effect of a

reemployment bonus on the behavior of claimants and based on the findings of previous reemployment bonus demonstrations. Subsequent sections provide more details on the treatment design (Section B), the sample design (Section C), and the operational design (Section D). Section E outlines the remainder of the report.

A. OVERVIEW OF THE DEMONSTRATION DESIGN

In its simplest form, a reemployment bonus plan consists of a fixed amount for the bonus and a fixed qualification period within which a claimant must find a job to receive the bonus. An analysis of the potential effects of this type of bonus suggests that individuals who, in the absence of a bonus offer, would have been unemployed longer than the length of the qualification period may shorten their unemployment durations to receive the bonus, thus generating savings for the UI system. However, individuals who would have become reemployed during the qualification period even without a bonus offer would receive a "windfall" without altering their behavior at all. In fact, the additional income provided to such individuals by the bonus could even induce them to lengthen their period of unemployment slightly.

Changes in the parameters of the bonus offer--the bonus amount and qualification period--are likely to influence these potential effects. Increasing the bonus offer should induce more individuals to shorten their unemployment period to receive a bonus, but would also increase the size of any windfall payments. Increasing the length of the qualification period will have similar effects. Given this discussion, it is uncertain whether a reemployment bonus offer will reduce the average duration of unemployment and whether any reduction in UI payments will outweigh the cost of bonus payments.

Previous demonstrations undertaken in Illinois and New Jersey showed that a reemployment bonus plan can change the behavior of claimants and can be a cost-effective approach from the standpoint of the UI system (see Woodbury and Spiegelman, 1987, for the results of the Illinois demonstration, and Corson et al., 1989, and Anderson et al., 1991, for the results of the New Jersey

demonstration). However, each of these demonstrations tested only one bonus plan, consisting of (1) a specified bonus amount and (2) a qualification period. Thus, while these demonstrations provide a great deal of encouraging information, their results cannot be used to explore the sensitivity of program outcomes to changes in the *generosity* of the bonus plan.

Both the Pennsylvania demonstration and a demonstration undertaken concurrently in Washington State were designed specifically to build on these earlier demonstrations and to provide an empirical basis for exploring how the behavior of claimants and the cost-effectiveness of a bonus are affected by differences in the generosity of the bonus and the eligibility criteria for the bonus program. Thus, the Pennsylvania demonstration design called for testing several alternative bonus plans systematically and carefully to provide evidence to policymakers on relative "outcomes."

The Pennsylvania demonstration consisted of six treatments. The first four incorporated two benefit amounts (a high amount and a low amount) and two qualification periods (a long period and a short period) that were thought to bracket the policy-relevant range of such parameters. The high and low benefit amounts and the long and short qualification periods were combined into four treatments as follows:

		Qualification Period	
		Short	Long
Bonus Amount	Low	Treatment 1	Treatment 2
	High	Treatment 3	Treatment 4

The twofold advantage of testing all four treatment plans was (1) that participants in each plan could be compared with claimants who were not offered a bonus (the control group), thus testing the direct effect of each plan, and (2) that participants in different plans could be compared with each other, thus testing the relative effectiveness of the plans. Therefore, this design provided a framework within which the responses of claimants to differences in the reemployment bonus plan

could be tested directly. For example, participants in treatment 4 were offered the most generous and lengthy plan, and were expected to exhibit the largest response to the offer of a bonus; however, this treatment was also expected to be the most expensive alternative. Thus, comparing the responses of claimants to treatment 4 with the responses of claimants to the other three treatments would enable us to assess the cost-effectiveness of a relatively generous bonus offer, as well as the implications of the alternative cost-saving modifications embodied in treatments 1 through 3. Furthermore, with simple extrapolation assumptions, we would be able to estimate the response of claimants to bonus amounts and qualification periods that could not be tested directly.

A concern with the fixed-length qualification periods embodied in treatments 1 through 4 is that, while they may give claimants an incentive to shorten what might otherwise be long spells of unemployment, they do not serve as a direct incentive to shorten unemployment spells to less than the full duration of the bonus qualification period. To provide just such an incentive, the New Jersey UI Reemployment Demonstration Project tested a declining bonus plan--the value of the bonus declined gradually over the course of the qualification period. A declining bonus was also included in the Pennsylvania demonstration as treatment 5. The qualification period and the initial bonus amount for this group were set at the level of treatment 4, but the bonus amount declined over the qualification period. In addition to changing the incentives of claimants to engage in rapid, productive job search, the declining bonus feature of treatment 5 may represent another way to lower the costs of the reemployment bonus plan.

Finally, a special element of the Pennsylvania demonstration was the offer of job-search assistance (JSA). A reemployment bonus gives claimants an incentive to seek reemployment relatively quickly, but some claimants might not be able to take full advantage of the incentive because they lack job-finding skills. Accordingly, special JSA services (in the form of an intensive workshop and assessment) were offered to claimants who were selected for treatments 1 through 5. To help isolate the added benefits of offering JSA, the demonstration withheld the offer of JSA to

some claimants. Specifically, these claimants were offered the same bonus plan as was offered to participants in treatment 4, but were not offered job-search assistance. This plan was designated as treatment 6. Thus, a comparison of the responses of claimants in treatments 4 and 6 would indicate the added effect of the offer of JSA on claimants.

Relative to the demonstration design requirements necessary to evaluate the benefit and cost implications of alternative bonus plans, the requirements necessary to examine the implications of targeting are few. Basically, the demonstration applied only a limited number of sample eligibility criteria, so that the effects of the demonstration treatments could be tested for as broad a group of UI claimants as possible, as well as for subgroups of the overall sample. The purpose of the subgroup analysis was to enable us to determine the benefits and feasibility of targeting selected types of claimants for a future program.

B. TREATMENT DESIGN COMPONENTS

The previous section introduced the major features of the treatment design, and discussed how that design is appropriate for developing UI reemployment bonus policy. This section describes these features--the bonus amount, the qualification period, and the job-search workshop--in greater detail (Table I.1 summarizes the design). It also discusses individual eligibility requirements and eligibility requirements for the bonus.

1. Bonus Amount

The first decision associated with the bonus amount was how the bonus amounts should be set for each claimant. Based on our review of the options, we chose to tie the bonus amounts to the UI weekly benefit amounts (WBAs) of individual claimants.[1] This policy provides a relatively uniform set of incentives across claimants in terms of their remaining UI entitlements; that is, the bonus amounts reflect a fairly uniform percentage of the expected costs of future benefits that the UI

[1]Another option was to offer a fixed bonus amount regardless of a claimant's individual WBA.

TABLE I.1

TREATMENT AND CONTROL GROUP PROGRAM PARAMETERS

Group	Bonus Amount	Qualification Period	Workshop Offer
0 (Control Group)	0	0	No
Treatment 1	Low (3 x WBA[a])	Short (6 Weeks)	Yes
Treatment 2	Low (3 x WBA)	Long (12 Weeks)	Yes
Treatment 3	High (6 x WBA)	Short (6 Weeks)	Yes
Treatment 4	High (6 x WBA)	Long (12 Weeks)	Yes
Treatment 5	Initially High (6 x WBA) But Declining	Long (12 Weeks)	Yes
Treatment 6	High (6 x WBA)	Long (12 Weeks)	No

[a]The WBA is the UI weekly benefit amount.

system would pay to claimants if they continued to be unemployed. At a more practical level, a future program operating through the UI system would probably base benefits on the weekly benefit amount.

The second decision associated with the bonus amount was the amount of money to be offered. The amounts were to be large enough to prompt claimants to reduce their spells of unemployment, but not so large that the costs of the bonus treatments outweighed the savings of the reduced spells. On the basis of the Illinois and New Jersey experiences, we set the low bonus amount at three times the WBA, and the high bonus amount at six times the WBA. These bonus offers averaged $500 and $997.[2]

2. Qualification Period

Decisions about alternative lengths of bonus qualification periods represent a tradeoff between (1) enhancing the bonus plan participation of otherwise long-term UI claimants by offering them a sufficiently long qualification period and (2) avoiding the added windfall costs that are inherently associated with a longer qualification period. To test the sensitivity of this tradeoff, the Pennsylvania demonstration adopted two qualification periods: a 12-week qualification period (measured from the date of the bonus offer) as the "long" option, and a period of half that length, or 6 weeks, as the alternative.[3] Moreover, participants in treatment 5--the declining bonus plan--were offered the high bonus amount (that is, six times the WBA) for the first three weeks of the treatment; thereafter, the bonus amount declined by 10 percent of the original amount each week, until its value fell to zero after 12 weeks.

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[2]Over the course of the demonstration, the average WBA was $165. Before the dependent allowance, the maximum WBA in Pennsylvania was $252; after January 1, 1989, the maximum WBA was raised to $266.

[3]A 12-week qualification period was also adopted in the New Jersey demonstration; an 11-week period was adopted in the Illinois demonstration.

3. Job-Search Assistance Services

The JSA component of the demonstration included a job-search workshop and an individualized assessment session. The workshop was designed by the state Job Service (JS) staff to provide claimants with the information, skills, and techniques necessary to locate suitable employment within a reasonable time period. Specifically, the workshop--to be conducted over a 14- to 16-hour period--covered such topics as self-evaluation techniques to ascertain job skills and goals, methods for locating, researching, and contacting employers, and steps involved in planning a job-search campaign. Claimants who completed the workshop were encouraged to schedule time for an assessment session. These sessions provided an opportunity for claimants to meet individually with the workshop leader to discuss their job-search plans and concerns. Claimants were not required to attend the demonstration job-search workshop and individualized assessment session, which was consistent with the Pennsylvania UI program policy of not imposing specific job-search requirements on claimants.

4. The Selection of Claimants

Claimants who applied for UI benefits in the demonstration offices and who were monetarily eligible for UI were selected to be assigned to one of the six treatments or the control group.[4,5] Claimants were selected and assigned once each week through a rigorous random selection process in which monetarily eligible claimants who applied for UI benefits in the past week were first screened according to five sample eligibility criteria:

1. ***Benefit Application Date.*** Although the benefit application date generally corresponds to the Sunday prior to the actual date of application, the Pennsylvania UI system permits back-dating applications if claimants had been eligible for benefits during previous weeks. Since an operational goal of the program was to offer bonuses to

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[4]To receive a bonus, claimants were to be eligible for UI benefits. However, since decisions about nonmonetary eligibility are often made after monetary eligibility decisions, claimants were required to meet only monetary eligibility rules to be selected for the demonstration. They were to be both monetarily and nonmonetarily eligible for UI in order to receive a bonus.

[5]For operational simplicity, only regular UI claimants and those who filed as former state employees were included in the demonstration.

claimants as soon as possible after they became eligible for UI, claimants whose benefit application dates were more than two weeks prior to their selection for the demonstration were not eligible for the demonstration.

2. ***Transitional Claims.*** Claimants who file transitional claims would most likely be excluded from an ongoing bonus program, since they would have already received an offer of a bonus in the previous benefit year.[6] Hence, these claimants were excluded from the demonstration.

3. ***Union Attachment.*** Individuals who accept employment solely through unions form another group whose incentives may differ from those of other claimants due to the nature of the union-employee relationship. Consequently, claimants who reported accepting employment exclusively through unions were not eligible for the demonstration.

4. ***Employer Attachment.*** Claimants who are employer-attached, as evidenced by a specific recall date, clearly expect to be recalled and, according to previous research, have an extremely high probability of being recalled. The bonus incentives are largely irrelevant for such individuals, and, furthermore, a bonus offer might interfere with established employer-employee relationships. Thus, claimants who expected to be recalled *and* had a specific date within 60 days after their UI application were excluded from the demonstration.

5. ***Labor Disputes.*** Current state and federal regulations prohibit JS staff from providing services to claimants who have been separated from their job due to a labor dispute. For this reason, such claimants were excluded from the demonstration.

5. Requirements for Bonus Receipt

Individuals who secured a job within the bonus qualification period were eligible to receive a bonus if they met certain additional criteria. Five additional criteria were imposed to serve as prudent constraints on the receipt of the bonus, and to help generate UI program savings from the bonus offer:

1. ***Job Start Date.*** Bonus claimants were required to begin work in their new jobs (including self-employment) during the qualification period, which began with the date on which they were offered the bonus. This rule reinforced the objective of providing bonuses for relatively short spells of unemployment and UI receipt.

[6]Transitional claims are those in which a claimant who is receiving benefits reaches the end of his or her benefit year and can at that time establish a new benefit year and continue to receive benefits.

2. ***Reemployment with Separating Employers.*** Bonuses were not paid to claimants who were recalled by their immediate pre-layoff employers. The purpose of the bonus was to encourage unemployed workers to search for work more diligently and to accept suitable employment more rapidly than would be the case otherwise. In contrast, a recall by a previous employer is largely outside of the control of workers, and bonuses paid for such recalls would represent windfall gains to recipients. However, a worker could qualify for a bonus if he or she was recalled to the pre-layoff employer after an initial period of employment with another employer.

3. ***Full-Time Employment.*** All bonus-qualifying jobs were required to be full-time. For the demonstration, a job was considered to be full-time if the worker was employed 32 or more hours per week and did not collect UI benefits while he or she was employed in that job.

4. ***Continuous Employment.*** Because the offer of a reemployment bonus might prompt some claimants to accept poorly suited jobs just to claim the bonus, safeguards were established to ensure that the new jobs had more than short-term potential. Specifically, bonus claimants had to work in their new jobs for 16 weeks before they received their bonuses. Claimants were allowed to change jobs during this period, as long as their employment was not interrupted for more than 5 days, and they did not claim UI benefits for any week in the 16-week period.

5. ***UI Eligibility.*** Demonstration claimants were required to maintain both monetary and nonmonetary eligibility for UI up to the point at which they became employed. Therefore, their UI entitlement could not have been reduced to zero for any reason. Furthermore, they could not have received a duration disqualification for UI benefits due either to a late-arising separation reason or to a nonseparation reason. Nonduration disqualifications (for example, for "able and available" issues) did not affect the bonus status of claimants.

C. SAMPLE DESIGN

This section describes the sample design for the Pennsylvania demonstration. In section 1, we describe the allocation of eligible claimants among the six treatments and the control group, and we discuss our overall sample size objectives. In section 2, we discuss the selection of the 12 demonstration offices.

1. Sample Allocation and Sample Size

We defined two types of design goals for the Pennsylvania demonstration: the ability to detect cost savings for the specific bonus treatments of policy interest and the ability to measure the impact of alternative treatment designs, such as bonus amounts, on UI program costs. To address the first

goal, we identified three treatments as having direct policy relevance--the 12-week, low bonus plan (treatment 2), the 6-week, high bonus plan (treatment 3), and the 12-week, declining benefit schedule plan (treatment 5). For each of these treatments, we chose a cost savings of $150, or one week of UI benefits, as the level of UI impacts that we wanted to detect at a minimum of 80 percent power for a one-tail test at the 95 percent confidence level. The $150 level was consistent with the New Jersey and Illinois results.

To address the second goal, we established three specific objectives pertaining to the impact of changing the characteristics of the bonus plan: (1) to detect a $150 impact of providing a $900 change in the bonus amount, controlling for the qualification period and workshop offer, (2) to detect a $150 impact of moving from a 12-week qualification period to either a 6-week or a declining bonus, controlling for other characteristics, and (3) to detect a $150 impact of removing the workshop offer.[7]

We then developed a formal sample allocation model based on these design objectives, on assumptions about the magnitude of the response to the bonus offers, on the cost of individual treatments, and on an overall budgetary constraint for the treatments.[8] The sample allocation obtained from this model provided 3,000 control and 10,120 treatment plan members, allocated to the specific treatments as shown in Table I.2.[9]

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[7]The $900 figure was used because it equaled the expected average high bonus amount (6 times an average weekly benefit amount of $150). Because of increases in weekly benefit amounts, the actual high bonus offer averaged $997.

[8]The behavioral relations and the cost estimates were based on the New Jersey and Illinois findings. The overall budget constraint was $1.5 million--the amount available for the bonus payments.

[9]This design provided sufficient statistical power to achieve all of our design objectives except for detection of the effect of the workshop offer. A $150 workshop effect was detected with this design with 70 percent instead of 80 percent power.

TABLE I.2

TREATMENT AND CONTROL GROUP TARGET SAMPLE SIZES

Group	Target Sample Size
0 (Control Group)	3,000
Treatment 1: Low bonus, short qualification period	1,030
Treatment 2: Low bonus, long qualification period	2,240
Treatment 3: High bonus, short qualification period	1,740
Treatment 4: High bonus, long qualification period	1,590
Treatment 5: Initially high but declining bonus, long qualification period	1,740
Treatment 6: High bonus, long qualification period, no workshop	1,780
Total	13,120

2. The Selection of Local Offices

In order to generalize the results of the demonstration to the entire state, UI claimants were selected randomly from claimants at local unemployment offices throughout Pennsylvania. However, due to the operational burden and costs of conducting the demonstration program at every local office in the state, the most cost-effective way to meet this objective was first to select a random sample of local UI/JS offices, and then to select a random sample of UI claimants from each of the selected offices. This process was undertaken in a manner which ensured that each eligible claimant in the state had an equal probability of selection into the demonstration sample.

More specifically, local offices were clustered according to the average duration of UI receipt (a major outcome of interest) and UI/JS region within the state. Twelve clusters with approximately equal-size UI caseloads were formed, and one office was selected randomly from each cluster to participate in the demonstration.[10,11] The following 12 offices were selected:[12]

1. Coatesville
2. Philadelphia - North
3. Philadelphia - Uptown
4. Reading
5. Lancaster

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[10]Our analysis suggested that selecting 12 offices was sufficient to hold the design effect--the loss of statistical precision due to clustering the sample into a small number of offices--to approximately 10 percent. That is, we expect that the true standard errors of estimates exceed those calculated from individual claimant observations by about 10 percent. However, because of the small size and uncertainty associated with this estimate, we have not taken account of the design effect in our estimates of standard errors. Thus statements about the statistical significance of our estimates apply to the demonstration as it operated in the 12 sites included in the study.

[11]Offices were selected randomly with a probability of selection proportional to caseload size.

[12]Throughout this report the local offices are ordered according to the UI/JS regions defined by the state. Coatesville and the two Philadelphia offices are in Region 1; Reading is in Region 2; Lancaster is in Region 3; Lewistown is in Region 4; Butler, Connellsville, and McKeesport are in Region 6; Erie is in Region 7; and Pittston and Scranton are in Region 8.

6. Lewistown

7. Butler

8. Connellsville

9. McKeesport

10. Erie

11. Pittston

12. Scranton

D. OPERATIONAL DESIGN AND PROJECT ORGANIZATION

To implement the demonstration design, central and local office demonstration staff were required to:

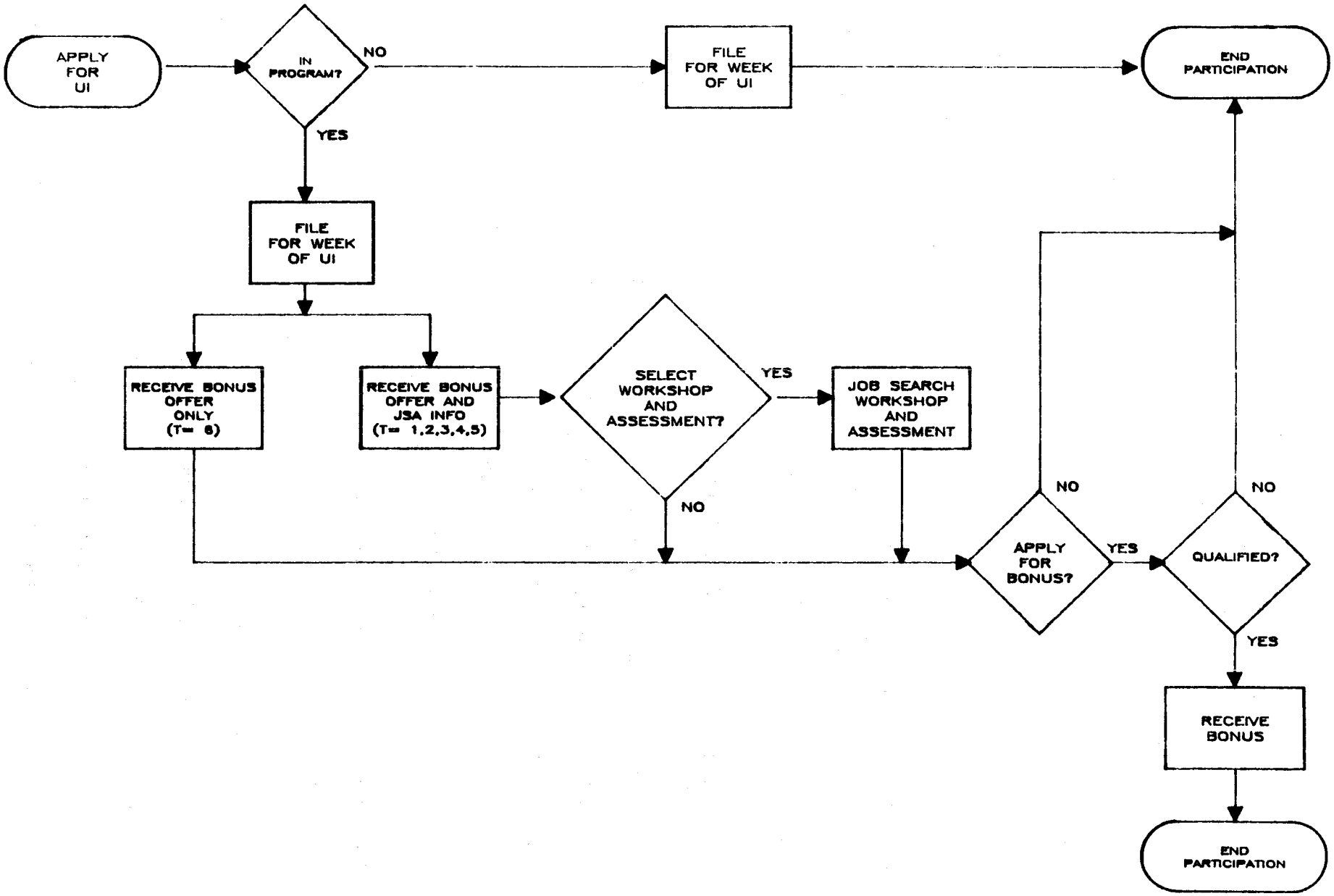
- Select demonstration-eligible claimants and assign them to the treatment and control groups
- Offer the reemployment bonus and job-search workshop to the selected claimants
- Conduct the demonstration workshops and assessment sessions
- Process bonus claims and pay bonuses
- Update the demonstration data system
- Provide project support and oversight

This section describes these functions in greater detail. It also describes the roles of central and local office staff.

1. The Operational Design

Figure I.1 illustrates the flow of claimants through the demonstration. The process began when claimants first applied for UI benefits. At that time, claimants completed the standard Pennsylvania UI application and eligibility review forms, just as they would in the absence of the demonstration. However, local office staff were instructed to follow special procedures to ensure that the data items

FIGURE I.1: CLAIMANT FLOW



their waiting week.

In the period between the application date and this filing date, application and claims data, which had been entered in the state's computer system, were passed from the state computer to a special demonstration computer, which was then used to initiate the process of sample screening, selection, and random assignment. Once the screening, selection, and assignment criteria were applied, information was passed back to the state computer, and local office "stops" were placed on the UI system records to indicate the selected treatment group members.

When claimants returned to the local office to file for a week of unemployment, the UI interviewers took their claims and referred those with demonstration "stops" to the co-located Job Service (JS) office for demonstration orientation. During this one-on-one orientation session, JS staff informed treatment group members of the specific bonus amount that was being offered, the period of time within which they were to obtain a job to receive the bonus, the other conditions to be satisfied to claim the bonus, and the procedures to be followed to claim the bonus.[13] Claimants who were eligible for the demonstration job-search workshop and assessment (treatment groups 1 through 5) were also told about those opportunities and were scheduled for the workshop if they were interested. As mentioned earlier, participation in the JSA component of the demonstration was not mandatory for claimants.

[13]Claimants also received an individualized packet of materials that described the bonus offer.

Claimants who appeared to be eligible were sent a second bonus claim form, which they submitted after 16 consecutive weeks of work. This second form was also reviewed by central office staff, who examined the claimant's UI claims information to verify the claimant's eligibility for UI and his or her UI status after starting the job. Staff also verified the claimant's employment by contacting the employer. Once these forms were processed, central office staff sent the claimants a letter to inform them of the outcome of this review process. Claimants who satisfied all the conditions received the bonus payment.

2. Organizational Structure

Central office and local office UI and JS staff were responsible for performing the demonstration functions. Central office staff provided overall project oversight, operated and maintained the project data base, processed bonus applications, and made the bonus payments. In these roles, central office staff performed the weekly sample selection process, which entailed transferring data on new applicants to the demonstration data base, applying the eligibility screens, assigning eligible claimants randomly to the treatment and control groups, and transferring this information to the state mainframe computer. They also updated the demonstration data base on a weekly basis with data on the status of each individual's UI claim.

At the local office level, UI staff were responsible for taking initial UI claims and obtaining all the information necessary to determine eligibility for the demonstration. Local office UI staff also identified treatment group members when they filed for a week of unemployment and referred them

III discusses sample selection, Chapter IV the bonus offer, Chapter V the job-search workshop, and Chapter VI bonus receipt. Chapters V and VI also discuss the claimants' participation in the workshop and their receipt of the bonus, respectively.

The next three chapters analyze the effects of the bonus offers. Chapter VII examines their impacts on the receipt of UI, and Chapter VIII examines their impacts on employment and earnings. Chapter IX uses the impact estimates and separate cost estimates to examine the benefits and costs of the reemployment bonus. The final chapter, Chapter X, provides a policy-oriented summary that compares the results of the Pennsylvania demonstration with the results obtained in other reemployment bonus demonstrations.

Five appendixes provide additional analyses and information. Appendix A reports the results of the claimant interviews and analyzes interview nonresponse. Appendix B presents an analysis of the duration of the initial unemployment spell. Appendix C examines whether displacement occurred during the Pennsylvania demonstration. Appendix D compares estimates of earnings based on wage-records with those based on interview data. Finally, Appendix E provides tables that show the results of the regressions underlying the estimates reported in the body of the report.

when to accept employment. A knowledge of the characteristics of the eligible population, combined with an understanding of these economic conditions, helps us assess the generalizability of the demonstration findings.

Economic conditions in Pennsylvania were relatively good during the demonstration. As discussed in Section A, employment was rising in all sectors except manufacturing, and the state unemployment rate was low. However, the unemployment rate did begin to rise toward the end of the demonstration period. The local economies of the demonstration sites generally reflected these favorable statewide conditions, although, as discussed in Section B, a few sites experienced relatively higher or lower unemployment rates and longer or shorter unemployment durations than the average.

Our examination of the characteristics of the demonstration-eligible population in Section C shows that, as intended, the demonstration directed bonus offers toward claimants who did not have expected dates of recall or who were otherwise not job-attached. Consequently, the demonstration-eligible population was younger than the insured-unemployed population in general. As also shown in Section C, the UI population of the sites selected for the demonstration was similar to that of the state, suggesting that the results of the demonstration should be generalizable to Pennsylvania as a whole.

A. STATEWIDE ECONOMIC CONDITIONS

During the 1980s, economic conditions in Pennsylvania paralleled those of the nation. After a recession in the early 1980s that reduced manufacturing employment substantially, the state and

of the 1980s began to slow during the last half of 1989 (roughly corresponding to the last half of the intake period for the demonstration), and this slowdown continued into 1990. As in the early 1980s the reduction in manufacturing employment was most prominent.

B. THE CHARACTERISTICS OF THE SITES

Table II.1 provides data on the 11 counties in which the 12 demonstration offices were located.[1] These data show that, on average, the counties in which the sites were located were more densely populated and had a higher proportion of employment in manufacturing than the state as a whole. The UI offices in the sites were also larger than average. These differences reflect the fact that offices with larger claims loads had a higher probability of being selected for the demonstration than did offices with smaller claims loads (see the discussion in Chapter I).

The data in the table also show that, while most sites faced relatively favorable economic conditions, unemployment rates and the average duration of UI did vary among the sites. Since we expect that these economic conditions may have affected responses to the reemployment bonus, we

[1]Much of the descriptive information presented in this section was obtained from the Commonwealth of Pennsylvania, Department of Labor and Industry, *Annual Planning Information Report, Fiscal Year 1990*, Fall 1989. Note that county-level data may not fully reflect the employment environment for the municipalities in which the demonstration offices were located. However, the county data provided the best available characterization of the labor markets within which demonstration claimants, who may have lived throughout the county, would be searching for work.

TABLE II.1

THE CHARACTERISTICS OF THE DEMONSTRATION SITES

County:	Chester	Philadelphia		Berks	Lancaster	Mifflin	Butler	Fayette	Allegheny	Erie	Luzerne	Lackawanna		
Demonstration Office:	Coatesville	Phila-North	Phila-Uptown	Reading	Lancaster	Lewistown	Butler	Connellsville	McKeesport	Erie	Pittston	Scranton	All Sites	Statewide
Population (1986) (1,000s)	339	1,643	1,643	321	394	46	151	156	1,374	279	331	223	5,257	11,888
Population/Square Mile	447	12,080	12,080	373	413	112	192	196	1,889	347	372	484	693	265
Unemployment Characteristics														
Unemployment Rate (1989)	2.4	5.1	5.1	3.9	3.3	6.6	4.8	7.4	4.0	5.1	6.0	5.3	4.9	4.5
UI claims:														
Number of initial claims (1989)	5,308	9,128	5,409	9,887	9,408	4,666	5,615	3,183	5,139	9,458	6,070	7,682	80,943	406,243
Average weeks duration (1989)	13.5	16.4	17.0	11.3	11.3	10.7	15.6	18.0	17.0	15.8	13.5	12.7	14.4	14.7
Employment Characteristics														
Percent Manufacturing Employment (1989)	21.7	12.1	12.1	31.6	31.8	40.9	26.4	18.1	12.6	30.6	22.1	26.4	24.9	20.9
Percent Change in Employment (1986-1989)														
Total	11.3	0.4	0.4	8.2	10.3	10.6	15.6	7.9	7.0	8.2	9.3	5.5	8.6	7.4
Manufacturing	-17.0	-11.8	-11.8	1.5	1.2	9.8	10.5	10.9	-3.8	0.1	-4.8	-3.0	-0.6	0.0
Nonmanufacturing	22.9	2.4	2.4	11.7	15.0	11.2	17.5	7.2	8.7	12.2	14.1	8.9	12.0	9.6
Percent Change in Employment (1988-1989)														
Total	6.2	-1.3	-1.3	3.3	3.0	3.5	4.0	3.1	2.5	3.1	3.0	2.8	3.0	2.1
Manufacturing	1.5	-5.2	-5.2	0.5	-1.3	4.7	3.2	-0.3	-1.8	-1.5	-1.8	3.7	0.2	-1.0
Nonmanufacturing	7.6	-0.7	-0.7	4.6	5.1	2.7	4.2	3.9	3.2	5.3	4.4	2.5	3.9	2.9
Standard of Living														
Average Annual Wages (1988)	24,227	25,035	25,035	21,156	20,011	17,474	19,947	16,619	22,782	20,095	17,455	17,545	20,213	21,488
Percent on Public Assistance (1988)	1.9	15.7	15.7	3.4	2.6	4.6	3.6	13.2	6.8	7.3	4.8	4.4	6.2	5.9

SOURCES: Population data are from the County and City Data Book, 1988. Data on the unemployment rate are from the U.S. Department of Labor, Bureau of Labor Statistics. The UI claims data and the percent on public assistance were provided by the Pennsylvania Department of Labor and Industry, Bureau of Research and Statistics. The employment characteristics are from Commonwealth of Pennsylvania, Department of Labor and Industry, UC Covered Employment and Wages, by County and Industry, 1986-1989. Average annual wages are from Commonwealth of Pennsylvania, Department of Labor and Industry, Annual Planning Information Report, Fiscal Year 1990, Fall 1989.

NOTE: The data in this table are for the county in which a demonstration site is located, except for data on UI claims, which are for the demonstration office.

organize the remainder of this discussion by grouping the sites according to four unemployment-rate, UI-duration configurations:[2]

- Low unemployment rate, short duration sites
- High unemployment rate, short duration sites
- Moderate unemployment rate, long duration sites
- High unemployment rate, long duration sites

As will be clear below, these categories also happen to characterize groups of offices in large labor markets and, in most cases, offices in the same geographic area.

1. Low Unemployment Rate, Short Duration Sites (Coatesville, Reading, and Lancaster)

All the sites in this group had strong economies during the demonstration period.[3] They had low unemployment rates and low UI durations, along with above-average employment growth, despite an above-average proportion of employment in manufacturing.[4] (Their manufacturing employment was diverse, which may have provided economic stability to these areas.)

Chester County, in which Coatesville is located, experienced both a large population growth (16 percent between 1980 and 1988) and a large employment growth in the last half of the 1980s. Service and retail industries provided the most new jobs, while the largest growth in percentage terms occurred in construction and transportation and utilities. Although manufacturing employment declined in the latter half of the 1980s, nonmanufacturing employment more than compensated for the declines. Indeed, several manufacturing industries also grew during this period, and employment

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[2]It should be noted that none of the sites had an extremely high unemployment rate during the demonstration. We use "high," "moderate," and "low" here relative to the average for this demonstration.

[3]All the sites in this group are located in the southeast part of the state.

[4]Reading's unemployment rate was higher than the rates in the other two areas, but we included it in this category because it shared many other characteristics with Coatesville and Lancaster.

in manufacturing as a whole increased during the time period of the demonstration. In addition to the employment opportunities available through internal growth, this county's proximity to Philadelphia and its suburbs affords its residents additional job opportunities.

Berks County, in which the Reading office is located, had a relatively large percentage of its employment in manufacturing (32 percent) in 1989. The leading manufacturing industry during the late 1980s was electronic equipment (AT&T was the largest employer in the county in 1989), but a number of other manufacturing industries employed over 1,000 individuals. Although manufacturing employment declined during the early and mid-1980s, most manufacturing industries showed small increases between 1986 and 1989.[5] Nonmanufacturing employment also showed steady growth during the last half of the 1980s.

Like Berks County, Lancaster County also had a relatively large percentage of employment in manufacturing (32 percent), and its manufacturing industries were diverse (the largest industries in 1989 were food and kindred products and fabricated metals). The county experienced growth in manufacturing each year between 1983 and 1988, nearly regaining the employment levels that existed before the recession in the early 1980s. The small reduction in manufacturing employment in 1989 was due primarily to declines in the instruments industry. Overshadowing the small manufacturing gains was a large increase in nonmanufacturing employment, particularly in the trade and service industries. Increases in the county population (of 14 percent between 1980 and 1988) both influenced and supported this economic growth.

2. High Unemployment Rate, Short Duration Sites (Lewistown, Pittston, and Scranton)

The counties in which Lewistown, Pittston, and Scranton are located had above-average unemployment rates in 1989, but the local demonstration offices had relatively short UI durations.[6]

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[5]The textile and apparel industry showed the largest decline in employment during this period.

[6]Lewistown is located in central Pennsylvania, and Pittston and Scranton are located in the northeast part of the state.

One possible explanation for this pattern is that a relatively large proportion of unemployed individuals in these areas may have been on temporary layoffs. Indeed, staff of these local offices indicated that they serve many claimants who experience seasonal unemployment, as well as claimants who are routinely laid off for short periods (for example, for one to three weeks, as in holiday layoffs). Another possible explanation is that claimants could find jobs relatively easily once they became unemployed. This latter explanation appears more likely for Lewistown than it does for Pittston and Scranton, given differences in the economic climates of these areas.

Mifflin County, in which Lewistown is located, is a rural county with a high percentage of employment in manufacturing (41 percent) relative to both the demonstration average and the state average. The largest manufacturing industries were industrial machinery (for example, farm equipment) and apparel and textiles. However, even the largest employers in the county generally had small operations, each employing fewer than 1,000 workers. The county experienced employment growth in most manufacturing and nonmanufacturing industries during the late 1980s. Although the largest percentage growth was in services, manufacturing growth generated more new jobs.

Pittston (in Luzerne County) and Scranton (in Lackawanna County) share the same labor-market area (defined by the Scranton/Wilkes-Barre Metropolitan Statistical Area). Although the proportion of manufacturing employment in these counties was only slightly higher than the state average in 1989, manufacturing provided more jobs than any of the primary nonmanufacturing industries (including wholesale and retail trade and services). The apparel industry dominated the manufacturing sector in Luzerne County (representing 21 percent of manufacturing employment). The manufacturing industries in Lackawanna County were more diverse; the leading industries were electronics, apparel, and printing and publishing. Luzerne County experienced a decline in manufacturing employment between 1988 and 1989, with the largest loss of jobs in the apparel industry. Manufacturing employment grew in Lackawanna County during the same period. During the demonstration, overall employment grew at or above the state level in both counties.

3. Moderate Unemployment Rate, Long Duration Sites (both Philadelphia Sites, McKeesport, Erie, and Butler)

With the exception of Butler, the sites with moderate unemployment rates and longer than average durations were located in or near the three largest labor markets in Pennsylvania--Philadelphia, Pittsburgh, and Erie.[7]

Philadelphia has a diverse employment base in both the manufacturing and nonmanufacturing sectors. However, many industries in both sectors experienced declines during the late 1980s. In manufacturing, nearly all industries declined during this period, with the largest number of jobs lost in apparel and textiles. These large declines in manufacturing were accompanied only by small increases in the nonmanufacturing sector. In fact, the service industry was the only major nonmanufacturing industry that experienced employment growth. These patterns reflect both the national shift towards a service economy, as well as the movement of businesses from urban Philadelphia to the suburban areas that surround the city.

As with Philadelphia, Allegheny County (in which both Pittsburgh and McKeesport are located) has a diverse employment base. However, the economy of McKeesport in particular has traditionally been tied to coal-mining and steel industries, which declined substantially during the 1980s. Reflecting this trend, McKeesport local office staff indicated that they served a large proportion of dislocated steel workers during the demonstration. The loss in manufacturing employment was accompanied by moderate growth in the nonmanufacturing sector, particularly in the service industry.

Constituting almost 31 percent of all employment, manufacturing also plays a major role in the economy of Erie County. During the late 1980s, large declines in durable manufacturing (for example, in instruments and related products) were counterbalanced by increases in other manufacturing and nonmanufacturing industries. Seasonal employment has also traditionally affected patterns of unemployment in this county, particularly in agriculture, lake shipping, construction, and

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[7]Philadelphia is located in the southeast part of the state, while the other three sites are located in the western part of the state.

tourism. Seasonal unemployment in these industries is likely to be relatively long term (up to several months), which may contribute to the long average UI duration experienced in the Erie office.

As with McKeesport, Butler County relies on the steel industry, with over 7 percent of all jobs in primary metals. However, in contrast to the other sites in this group, Butler County experienced a substantial growth in employment in most manufacturing and nonmanufacturing industries between 1986 and 1989. Staff in the Butler local office indicated that a few large plants closed during the demonstration, and that most unemployment was from layoffs by small companies. Seasonal unemployment was also important. As with Erie, the types of seasonal unemployment that were most common--construction, school workers, agriculture, and landscaping--were likely to be longer term.

4. High Unemployment Rate, Long Duration Site (Connellsville)

Connellsville, located in Fayette County, was the most depressed of the demonstration sites along most dimensions.[8] It had the highest unemployment rate (7.4 percent in 1989) and the longest UI duration (18 weeks) of all demonstration sites. It also had the lowest average wages, and a high percentage of its population was on public assistance. In addition, during the demonstration, the Connellsville UI/JS office served hundreds of individuals who had been permanently laid off due to the 1988 closing of a large Volkswagen assembly plant in adjacent Westmoreland County. However, employment growth in Fayette County was above the state average, with most of the growth in nonmanufacturing. Services (particularly health services) and retail trade were the two largest industries in 1989.

[8]Note that, although Fayette County is part of the Pittsburgh Metropolitan Statistical Area, this county is quite rural, and most parts of the county do not afford easy commuter access to employment opportunities in Pittsburgh.

C. THE CHARACTERISTICS OF THE ELIGIBLE POPULATION

Data on the characteristics of demonstration-eligible claimants at the sites show that 40 percent were female, 84 percent white, and 12 percent black, and over half were younger than age 35 (Table II.2).[9] In addition, the average weekly benefit amount of eligible claimants was $165, and the potential UI duration of the vast majority (99 percent) was 26 weeks.

Comparisons of the characteristics of the eligible claimants with the characteristics of the total insured-unemployed population at the demonstration sites show a number of differences that are due directly to the demonstration eligibility criteria. As one would expect, fewer eligibles had expected dates of recall (11 percent versus 43 percent among the insured-unemployed population), and fewer eligibles accepted only union work (1 percent versus 8 percent among the insured-unemployed population). Fewer eligibles also had weekly benefit amounts at the maximum and more eligibles were younger than age 35 than was true of the overall UI population. These latter differences were also due to the eligibility criteria. For example, individuals on temporary layoff are likely to be from manufacturing jobs and thus likely to have higher weekly benefit amounts than individuals not on temporary layoff.

A comparison of the characteristics of the statewide insured population with the characteristics of the insured population in the demonstration sites shows only minor differences. One exception is that 43 percent of the insured population in the demonstration sites expected to be recalled, compared with 50 percent statewide. Since claimants who expected to be recalled were generally excluded from the demonstration, this difference is not likely to be important. Thus, we conclude that the demonstration sites appear to be representative of the statewide UI population.

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[9]The characteristics of the eligible and insured-unemployed populations over all the demonstration sites are weighted estimates, where the characteristics of each site are weighted according to the share of each site in the overall analysis sample. The weights are roughly equal for each site, since the sites were chosen to represent clusters of sites with equal-size insured populations. The small differences in weights by site arise from the constraints inherent in the sampling process (see the discussion in Chapter III).

TABLE II.2

THE CHARACTERISTICS OF THE ELIGIBLE POPULATION, BY LOCAL OFFICE

	Coatesville	Phila-North	Phila-Uptown	Reading	Lancaster	Lewistown	Butler	Connellsville	McKeesport	Erie	Pittston	Scranton	Eligible Population at Demonstration Sites[a]	Insured-Unemployed Population at Sites[a]	Statewide Insured-Unemployed Population[b]
Demographic Characteristics															
Percent Female	43.5	41.7	39.1	39.8	38.9	46.7	38.1	34.7	36.9	37.9	41.8	39.9	39.8	38.3	38.7
Ethnic Composition (percent)															
White	86.5	63.4	18.3	86.3	88.5	99.6	99.6	98.9	89.9	93.4	100.0	98.5	84.2	84.4	86.5
Black	11.0	17.7	70.5	6.2	5.5	0.2	0.2	1.0	9.9	5.8	0.0	0.9	11.8	11.1	9.8
Hispanic	2.0	15.2	10.9	7.1	4.7	0.1	0.1	0.0	0.0	0.6	0.0	0.1	3.6	3.7	1.5
Other	0.6	3.7	0.3	0.5	1.3	0.1	0.2	0.1	0.2	0.2	0.0	0.5	0.7	0.7	0.6
Missing	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	1.6
Age (Percent)[c]															
Younger than 35	46.6	52.4	59.0	52.6	51.3	59.2	55.4	53.6	49.7	54.5	51.5	55.0	53.3	47.3	48.6
35-54	42.7	36.5	33.2	37.6	38.7	33.3	36.1	37.7	39.1	36.4	36.4	33.1	36.8	40.4	38.6
55 or older	10.6	11.1	7.8	9.8	9.9	7.5	8.5	8.5	11.2	9.2	12.1	12.0	9.9	12.3	12.9
Mean Age	37.7	36.4	34.7	35.9	36.3	33.7	35.2	35.1	36.9	35.7	36.5	35.8	35.9	37.6	38.6
Employment Characteristics															
Reason for Separation (Percent)															
Laid-off	49.8	58.8	49.7	51.8	49.0	63.2	62.3	68.5	56.5	58.2	63.4	63.3	57.5	68.9	n.a.
Quit	3.5	6.2	11.0	5.2	4.7	2.5	6.2	7.2	6.4	6.5	4.1	5.1	5.8	3.3	n.a.
Fired	16.3	12.2	21.7	18.6	22.5	14.3	12.2	7.0	15.0	13.4	8.5	12.3	14.7	8.2	n.a.
Retired	0.1	0.3	0.6	0.2	0.1	0.0	0.2	0.1	0.2	0.2	0.0	0.1	0.2	0.1	n.a.
Other	30.3	22.5	17.1	24.2	23.7	20.0	19.1	17.2	21.9	21.8	24.0	19.1	21.8	19.5	n.a.
Has Date of Expected Recall (Percent)	12.6	0.9	3.0	16.5	10.1	8.7	19.7	15.5	10.0	6.6	19.1	14.6	11.3	43.0	49.5
Accepts Union Work Only (Percent)	0.0	0.1	5.6	3.0	0.1	0.0	0.7	0.0	1.3	0.4	0.0	0.4	1.0	8.2	n.a.
UI Entitlement[d]															
Weekly Benefit Rate (Percent)															
$0-$50	1.0	1.1	1.7	1.5	1.0	4.9	3.3	4.5	3.9	2.9	3.2	2.9	2.6	1.7	n.a.
$51-$100	9.8	15.0	21.0	12.9	10.5	30.8	25.3	30.2	26.1	21.3	21.1	20.0	19.9	16.4	n.a.
$101-$150	15.6	25.1	31.2	24.3	21.7	28.8	23.1	25.6	22.9	23.7	30.6	27.3	24.8	22.2	n.a.
$151-$200	18.2	22.0	20.5	21.7	22.7	13.6	16.0	13.3	16.3	17.4	18.2	20.7	18.6	18.5	n.a.
$201-$250	15.1	14.3	10.3	14.4	15.9	9.7	12.4	10.2	10.1	12.6	11.7	11.7	12.5	13.8	n.a.
Over $250	40.3	22.4	15.3	25.3	28.2	12.3	19.9	16.2	20.7	22.1	15.3	17.5	21.7	27.4	n.a.

TABLE II.2 (continued)

	Coatesville	Phila-North	Phila-Uptown	Reading	Lancaster	Lewistown	Butler	Connellsville	McKeesport	Erie	Pittston	Scranton	Eligible Population at Demonstration Sites[a]	Insured-Unemployed Population at Sites[a]	Statewide Insured-Unemployed Population[b]
Mean Weekly Benefit (Dollars)	200	173	155	178	185	139	158	146	156	164	154	159	165	177	n.a.
Percent at the Maximum[e]	37.4	19.6	13.7	22.5	25.3	10.2	17.4	14.9	18.8	19.5	12.9	15.5	19.3	24.7	26.3
Mean Entitlement (Dollars)	5,199	4,501	4,014	4,822	4,813	3,613	4,087	3,777	4,041	4,251	3,983	4,127	4,280	4,587	n.a.
Potential Duration (Percent)															
16 weeks	0.5	0.8	1.7	1.0	0.8	1.4	1.0	1.5	1.4	1.0	1.4	1.2	1.1	0.9	0.9
26 weeks	99.5	99.2	98.3	99.0	99.2	98.6	99.0	98.5	98.7	99.0	98.6	98.8	98.9	99.1	99.0
Sample Size	3,974	5,612	4,980	4,469	3,670	921	2,901	1,347	3,040	6,165	1,236	3,203	41,518	82,900	n.a.

n.a.: Data not available.

[a]Estimates of the characteristics of the eligible and insured-unemployed populations over all sites are weighted averages of the site-specific characteristics, with the weights equal to the proportion of the analysis sample in each site.

[b]Data for the statewide insured-unemployed populations are based on a 1-percent statewide sample of monetarily valid claims whose benefit application dates were between January 1, 1988 and December 31, 1988. These data were provided by the Pennsylvania Department of Labor and Industry, Bureau of Research and Statistics.

[c]Age was calculated as of the claimant's benefit application date.

[d]These data represent the entitlement that was established at the time that the claimant was initially determined to be monetarily eligible for UC. Changes in entitlement are not reflected here. In addition, dollar amounts do not include the dependent allowance.

[e]The maximum weekly benefit rate was $252 through December 31, 1988, and $266 from January 1, 1989 through December 31, 1989.

The data in Table II.2 also allow us to examine differences in the characteristics of eligible claimants by site. While the sites were similar in terms of most demographic characteristics, ethnic composition varied widely among the sites. Most of the sites had a predominantly white eligible population. In contrast, the Philadelphia-Uptown office tended to serve a predominantly black population. Both Philadelphia offices also had a higher percentage of Hispanic claimants than did other offices in the demonstration.

Other small demographic differences also stand out. Lewistown had a larger percent of eligible females than the average (47 versus 40 percent). Lewistown and Philadelphia-Uptown also tended to have a younger-than-average eligible population, while Pittston and Scranton had a higher-than-average percentage over 55 years of age.

The employment characteristics and UI entitlement of the eligible populations also differed somewhat among the sites. Eligibles at the Butler and Pittston offices were more likely to expect to be recalled than were claimants in other offices, while eligibles at the Philadelphia offices were less likely. Indeed, staff at the Philadelphia offices indicated that it was quite common for area employers not to tell workers about recall plans, even when they expected to recall the workers. Claimants at the Philadelphia-Uptown office were more likely to have quit their jobs and more likely to have accepted only union work than were claimants at the other offices. Finally, eligible claimants at the Coatesville office had higher average UI entitlements than did claimants at other offices, while claimants at Lewistown and Connellsville had relatively low entitlements.

In summary, this comparison of the demonstration-eligible population and the total insured-unemployed population, both at the demonstration sites and statewide, suggests that the results of the demonstration should be generalizable to Pennsylvania as a whole.

III. SAMPLE SELECTION

The research design for the Pennsylvania Reemployment Bonus Demonstration required that UI claimants meet certain eligibility standards to be selected for the demonstration, and that these eligible claimants be assigned randomly to a treatment or to the control group. This sample selection process involved three operational steps. First, as part of the UI application process, local office staff collected the information necessary to identify eligible claimants. Second, central office staff used these data to identify eligible individuals. Finally, central office staff selected the demonstration sample randomly from among program eligibles, and assigned them randomly to the treatments and the control group.

In the first three sections of this chapter, we describe this screening, selection, and assignment process in more detail. We show that about one-half of UI-eligible claimants met the demonstration eligibility criteria, and that the most important screening criterion was the exclusion of individuals with a definite recall date. We also report final sample sizes and conclude that the sample selection process was implemented successfully.

In the final two sections of the chapter, we discuss two analytic issues. In Section D, we compare the characteristics of eligibles and noneligibles to determine whether the screening process correctly directed demonstration services to individuals who were not job-attached. We conclude that this goal was achieved. In Section E, we examine whether the randomization process was effective at generating comparable treatment and control groups. We also conclude that this process was successful.

A. THE COLLECTION OF SCREENING DATA

Monetarily eligible claimants were selected for the demonstration according to five eligibility screens.[1] These screens excluded (1) claimants whose benefit application dates were more than two weeks prior to their selection, (2) claimants who filed transitional claims, (3) claimants who reported obtaining jobs solely through a union, (4) claimants who had a definite expected date of recall within 60 days after their UI monetary eligibility was established,[2] and (5) claimants who were separated from their job due to a labor dispute.

The information required to implement these screens was collected routinely through the UI application process. However, the local offices were provided with special demonstration-relevant instructions for processing the application forms, to ensure that the data were collected consistently. In particular, two items on the forms were critical to the demonstration: (1) whether the claimant reported obtaining his or her job only through a union, and (2) whether the claimant had an expected date of recall.[3] Due to the importance of these items, staff were expected to review them with claimants at the office and to confirm the recorded information verbally with claimants. The goal of these procedures was to distinguish between claimants who had a high probability of being recalled (that is, those whose employer gave them a definite date) and claimants for whom recall was less certain (that is, those who reached their own conclusions about their chances of recall, or whose employer may have given them an approximate recall date).

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[1]Only regular UI claimants and those who filed as former state employees were included.

[2]During the pilot phase, the demonstration excluded claimants whose expected date of recall was within 90 days. This criterion was later changed to 60 days because the 90-day rule appeared to be excessively restrictive.

[3]Claimants whose hours were reduced to part-time (rather than becoming fully unemployed) were to be assigned a recall date in the week following application. The effect of this short-term recall date was to screen these individuals out of the demonstration, since the bonus offer was deemed to be inappropriate for this group of claimants.

Other than some difficulty in adjusting to the demonstration procedures, the only problem experienced by the local offices in the screening process was recording expected recall status.[4] Our observations of local office operations indicated that, at every office, some staff recorded recall dates for claimants who did not report a recall date but who worked for an employer who had a regular pattern of layoffs.[5] This recording error appears to have occurred frequently in two offices--Connellsville and Lewistown--where, for certain periods of time, staff were systematically recording recall dates of less than one month after application for claimants in selected seasonal occupations (such as school-bus drivers and other occupations associated with school schedules), even when the claimants themselves reported a recall date of more than 60 days in the future. Because staff believed that these claimants would return to their employer, they screened the claimants out of the demonstration. In addition, staff in two offices--Pittston and Reading--seemed routinely to record recall dates for claimants who reported a "month" of recall, even if they could not identify a specific week.[6]

These problems in collecting information on recall status erred on the side of excluding eligible claimants rather than including ineligible ones.[7] Thus, the claimants who were selected for the demonstration largely met the screening criteria. The only potential problem is that the sample may

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[4]The most notable problem occurred at the Erie local office. Due to a backlog in applications, this office was entering the union and expected-recall information for some claimants a day or more after the application was initially entered in the state computer system. Hence, for the first month or more of the demonstration, these data were not available for some claimants during the demonstration screening process. Once identified, this problem was resolved.

[5]Staff were especially likely to record recall dates for claimants who had been laid off regularly by the same employer. In addition, staff were sometimes aware of company layoff schedules based on formal information provided to the local unemployment office by the company. In these cases, staff recorded recall dates.

[6]Local office staff received corrective training when these problems were discovered.

[7]Due to unsystematic errors in collecting the application data (at all offices), some ineligible claimants were selected for the demonstration. For example, some demonstration claimants told Job Service interviewers that they obtained their jobs through their union or that they expected to be recalled, yet the application data entered into the state computer system did not reflect these situations.

not be entirely representative of the target population if the included group differs systematically from the group erroneously screened out. Although we cannot assess the magnitude of this potential problem directly, we believe that it is minor, and that the demonstration screening procedures were implemented largely as designed.[8]

B. IMPLEMENTING THE DEMONSTRATION ELIGIBILITY CRITERIA

Local office staff entered information from UI application forms daily into the state computer system. Once each week, central office staff used this information to identify claimants who were eligible for the demonstration. A computer file was created on the state system to extract information on all claimants in the 12 demonstration offices who during the past week were determined to be monetarily eligible for UI and who had filed for regular UI. This file was transferred to the special demonstration data system (the Participant Tracking System), and the five eligibility screens were applied. This second stage of screening excluded claimants whose benefit application dates were more than two weeks prior to their selection, those who filed transitional claims, those who reported obtaining jobs solely through a union, those who had a definite expected date of recall within 60 days, and those who were separated from their job due to a labor dispute.

As shown in Table III.1, these five eligibility criteria excluded about half of the applicants in the 12 demonstration offices who were monetarily eligible under the regular state UI program. The eligibility criterion that had the greatest impact was the recall screen (individuals who were excluded because they received partial benefits are also included in this figure).[9] This screen excluded about

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[8]From the perspective of designing screening procedures in a future program, it is clear that local offices and individual staff persons were easily able to develop their own systems for entering recall dates. Moreover, claimants did not always provide information that reflected their situation. An alternative approach might be to determine eligibility based on a recall date obtained directly from employers (Pennsylvania currently collects this information from separating employers). However, this approach would lengthen the amount of time between the UI application and the bonus offer.

[9]Note that, because an individual could be screened out by more than one criterion, the sum of the individual effects exceeds the total excluded.

TABLE III.1

THE EFFECT OF ELIGIBILITY REQUIREMENTS
ON THE UI-ELIGIBLE POPULATION

Eligibility Restrictions	Percent Excluded
Benefit Application Date Is More Than 2 Weeks Prior to Potential Selection[a,b]	11.0
Transitional Claims	9.5
Obtained Jobs Through Union	8.1
Expected Date of Recall Is within 60 Days	35.5
Labor Dispute	2.6
All Screens Combined	49.7

NOTE: This table includes only claimants whose benefit application dates were after October 22, 1988. The base for the calculations consists of 81,063 new, monetarily eligible claims under the regular state UI program in the 12 demonstration offices for weeks in which the sample was selected during full operations. The calculations show the percentage excluded by each restriction independent of the effect of the other restrictions. The total effect of all the eligibility restrictions is less than the sum of the individual effects, since some individuals were excluded by more than one restriction.

[a]The benefit application date generally corresponds to the Sunday prior to the date on which the claimant applied for benefits.

[b]The date that each case was potentially selected was unavailable for the analysis. We estimated this percentage by determining the percent of cases for which the date of extraction from the mainframe was more than 11 days after the benefit application date. This approach may slightly underestimate the percentage of claimants excluded.

36 percent of UI-eligible applicants (three-quarters of all those excluded), a figure which is not surprising given the importance of manufacturing in the Pennsylvania economy.[10] The remaining eligibility screens had modest to small impacts on eligibility.

Table III.2 shows the impact of the eligibility criteria by local office and by UI-administrative region. As shown in this table, the effects of the five screens, individually and combined, varied substantially by office. The criterion that varied the most was the recall screen. The percent excluded by this screen ranged from about 5 percent at the two Philadelphia offices to over 70 percent at Lewistown and Pittston. The effects of the remaining screens also differed among the offices, although to a lesser degree. Much of the variation is due to differences in local or regional employment environments, although differences in the manner in which application information was collected may also have contributed to the cross-office differences (see the earlier discussion in Section A).

C. RANDOM SELECTION AND ASSIGNMENT

Once the screening step was completed, a weekly maximum number of individuals was selected randomly from each office from among the pool of demonstration-eligible claimants. The selected claimants were then assigned randomly to the treatments and the control group (by the last two digits of their Social Security numbers) according to specific proportions of the sample that were to be allocated to each treatment group. This process worked well; during the demonstration, 52 weekly samples were selected in all offices beginning on October 26, 1988. Just prior to this period, 15 weekly samples were selected in Pittston for a pilot test of operations. The overall target of identifying and selecting 13,779 claimants was achieved (Chapter I discussed the sample design). However, three aspects of this process should be noted.

[10]As indicated in Chapter II, manufacturing accounted for nearly 25 percent of all employment in counties in which demonstration offices were located. A similar screen in the New Jersey demonstration excluded about 13 percent of the claimants (Corson et al., 1989).

TABLE III.2

PERCENT OF UI-ELIGIBLE POPULATION EXCLUDED
FROM THE DEMONSTRATION SAMPLE, BY LOCAL OFFICE

	Region 1			Region 2	Region 3	Region 4	Region 6			Region 7	Region 8		
Eligibility Restrictions	Coatesville	Phila-North	Phila-Uptown	Reading	Lancaster	Lewistown	Butler	Connellsville	McKeesport	Erie	Pittston	Scranton	Total
Benefit Application Date is More Than 2 Weeks Prior to Potential Selection[a,b]	10.5	9.2	7.2	20.2	13.1	13.1	3.6	2.6	2.6	4.6	15.1	18.6	11.0
Transitional Claims	7.1	6.9	7.7	9.6	8.7	12.0	12.9	17.7	5.4	9.3	13.6	9.3	9.5
Obtains Jobs Through Union	2.5	19.0	10.7	8.0	2.1	3.9	12.5	17.6	10.4	4.7	6.3	4.7	8.1
Expected Date of Recall is within 60 Days	16.5	4.9	5.1	44.4	47.4	78.7	31.6	38.1	31.0	23.9	71.1	52.0	35.5
Labor Dispute	1.2	3.7	2.0	0.9	1.2	0.1	0.1	0.3	2.6	0.6	2.4	12.6	2.6
All Screens Combined	30.1	32.0	25.1	58.7	55.0	82.1	47.0	57.8	43.5	37.6	78.5	64.2	49.7
UI-Eligible Sample	5,631	8,197	6,599	10,748	8,014	5,091	5,443	3,179	5,334	9,686	4,242	8,899	81,063

NOTE: This table includes only those claimants whose benefit application dates were after October 22, 1988. The base for the calculations consists of new, monetarily eligible claims under the regular state UI program in the 12 demonstration offices for weeks in which the sample was selected during full operations. The calculations show the percentage excluded by each restriction independent of the effect of other restrictions. The total effect of all eligibility restrictions is less than the sum of the individual effects, since some individuals are excluded by more than one restriction.

[a]The benefit application date generally corresponds to the Sunday prior to the date on which the claimant applied for benefits.

[b]The date that each case was potentially selected was unavailable for the analysis. We estimated this percentage by determining the percent of cases for which the date of extraction from the mainframe was more than 11 days after the benefit application date. This approach may slightly underestimate the percentage of claimants excluded.

affected.

Second, the target analysis sample was to be distributed evenly by office and by week of sample selection (each office had an overall target of 1,122 claimants and a weekly target of 21 claimants). However, since some claimants who initially apply for benefits do not return to a local office to file further, a larger sample was selected to achieve the desired sample size for analysis. This larger sample ranged from 22 to 40 claimants per office per week, depending on the historical experience in each office.[12]

Third, the allocation of the sample by treatment group was revised twice during the demonstration.[13] One change was made because participation in the demonstration workshop was so low (see Chapter V) that the distinction between treatment 4 (which encompassed a workshop offer) and treatment 6 (which did not) was becoming blurred. Thus, as of July 1989, individuals who

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[11]More specifically, in order to be a member of the analysis sample, selected claimants must have filed either for the waiting week or for both the waiting week and the first compensable week.

[12]Maximum weekly sample sizes--ranging from 24 to 29--were initially assigned to each office based on historical data on the proportion of initial applicants who filed a claim for a week of unemployment. Throughout the demonstration, the maximums were adjusted on an office by office basis to respond to (1) the actual proportion of demonstration claimants who signed for a week of unemployment and (2) the ability of the local offices to meet the targets given their claim loads. Due to these adjustments, office sample sizes for certain weeks ranged from a low of 22 to a high of 40.

[13]A minor change in the sample selection procedure was made to increase the weekly office target number for the analysis sample from 21 to 31 for the last three weeks of intake, in order to increase the analysis sample.

Overall, 15,005 individuals were selected. Ninety-four percent (14,080) of these individuals filed for a week of UI and are included in the analysis sample. This overall number exceeds the target sample size of 13,779.

Table III.4 shows the distribution of demonstration eligibles, the sample that was selected, and the analysis sample by office. The analysis sample sizes vary by office due to the limited number of eligible claimants in some offices, and to differences in the proportion of those selected who signed for a week of UI and became members of the analysis sample. Overall, 3 of the 12 local offices--Lewistown, Connellsville, and Pittston--did not have a sufficient number of claimants to meet the target sample size of 1,122.

Finally, Table III.5 shows the distribution of the analysis sample by quarter and treatment. The distribution of the sample by treatment group varied over time, due primarily to the sample allocation changes for treatments 1 and 6. For this reason, simple treatment and control differences will be misleading if treatment impacts vary by quarter (for seasonal or other reasons). Thus, in most of our subsequent analysis, we control for the quarter in which sample members were selected.[14]

[14]We used dummy variables for the quarter of sample selection to control for the change in sample allocation. Since differences in treatment impacts by quarter may vary among treatments, we also examined models that interacted the treatment variables with the quarterly dummies. Since estimates of treatment impacts differed only slightly between the two specifications, we used the simpler specification (the one with quarterly dummies and no interaction terms) in this report.

3 - High bonus, short qualification period	2,048	1,910
4 - High bonus, long qualification period	1,877	1,771
5 - Initially high but declining bonus, long qualification period	2,011	1,860
6 - High bonus, long qualification period, no workshop	1,377	1,302
Total Treatment Group	11,410	10,694
Control Group	3,595	3,392
Total	15,005	14,086

NOTE: The sample includes all demonstration claimants, including those selected during the pilot phase.

[a]The demonstration job-search workshop was offered to treatment groups 1 through 5 only. Claimants were assigned to treatment group 6 during the pilot phase and for about two-thirds of the 52-week full-scale operational period.

Philadelphia-Uptown	4,980	1,472	1,296
Reading	4,469	1,346	1,285
Lancaster	3,670	1,297	1,228
Lewistown	921	871	834
Butler	2,901	1,254	1,186
Connellsville	1,347	1,118	1,050
McKeesport	3,040	1,311	1,245
Erie	6,165	1,312	1,238
Pittston	1,236	1,114	1,047
Scranton	3,203	1,327	1,249
Total	41,518	15,005	14,086

NOTE: The sample includes all eligible claimants, including those selected during the pilot phase.

Fourth Quarter 1988	20.5	17.1	19.5	20.9	20.5	18.1	31.3
First Quarter 1989	23.8	20.1	23.6	21.9	23.5	21.2	36.0
Second Quarter 1989	21.9	20.4	22.7	22.3	23.6	23.2	30.8
Third Quarter 1989	25.6	22.9	27.1	25.1	26.1	25.4	0.0
Fourth Quarter 1989	6.9	18.2	5.8	8.5	4.9	10.6	0.0
Total	100.0	100.0	100.0	100.0	100.0	100.0	100.0

NOTE: Calculations are based on the 14,086 observations in the analysis sample. Columns may not sum exactly to 100 percent due to rounding.

were largely job-attached and were thus unlikely to respond to a bonus offer by looking for a new job. For these claimants, bonus payments for recall would simply constitute expensive windfall gains.

In this section, we evaluate the effectiveness of the sample eligibility requirements at directing bonus offers to claimants who were relatively less job-attached. We examine this issue by comparing the pre-unemployment characteristics of and the post-unemployment outcomes for the eligible claimants who did not receive a bonus offer (the control group) with the characteristics of and outcomes for ineligible claimants.

The outcomes for eligible claimants and ineligible claimants presented in Table III.6 clearly suggest that the eligible claimants were less job-attached than ineligible claimants. First, ineligible claimants were much more likely than eligible claimants to return to their pre-unemployment prior to the follow-up interview. Over 80 percent of the ineligible claimants had returned to their pre-unemployment employer, compared with only 37 percent of the eligible claimants. Second, ineligible claimants were much less likely than eligible claimants to search for employment after their layoff. In the follow-up interview, less than 30 percent of the ineligible claimants reported looking for work after their layoff. In contrast, nearly 70 percent of the eligible claimants reported looking for work. Both of these outcome comparisons suggest that the eligibility requirements excluded the more strongly job-attached claimants, and thus directed the bonus offers to a group of claimants who would be expected to respond to the bonus incentives.

Pre-Unemployment Characteristics		
Average Pre-Unemployment Job Tenure (years)	4.6	7.6
Pre-Unemployment Industry (percent)		
Agriculture/forestry/fishing/mining	2.8	0.9
Construction	12.3	16.6
Durable manufacturing	13.4	32.4
Nondurable manufacturing	20.6	28.1
Transportation	6.6	7.4
Wholesale trade/retail trade	19.0	5.5
Finance/insurance/real estate services	23.4	8.7
Administration	1.7	0.5
Pre-Unemployment Occupation (percent)		
Managerial/professional	9.5	2.7
Technical	1.7	0.9
Sales	8.4	2.8
Administrative support	18.2	6.0
Service	12.1	4.1
Mechanical and repair	4.3	3.7
Construction and extractive	7.3	13.6
Precision production	2.6	4.6
Machine operators	16.9	37.5
Transportation	6.8	9.0
Handlers	9.9	14.7
Farming, fishing, and industry	2.3	0.5

Findings on pre-unemployment industry and occupation also support this conclusion. Over 60 percent of the ineligible claimants came from manufacturing industries, which typically use temporary layoffs and recalls to retain an experienced labor pool. In contrast, only about 33 percent of eligible claimants came from manufacturing industries. In addition, less than 10 percent of ineligible claimants came from financial, insurance, or real estate industries, in which temporary layoffs and recalls are uncommon, compared with over 20 percent of eligible claimants. In terms of pre-unemployment occupations, ineligible claimants were more likely than eligible claimants to work as machine operators, construction workers, or handlers, occupations in which temporary layoffs and recalls are frequent. Ineligible claimants were less likely than eligible claimants to have worked in administrative support or services (30 percent versus 10 percent), occupations in which temporary layoffs and recalls are less common. These findings provide additional evidence that the eligibility restrictions helped direct the bonus offers to claimants who would be expected to respond this type of reemployment incentive.

E. THE EFFECTIVENESS OF THE RANDOMIZATION PROCESS

One way to check the effectiveness of the randomization process is to compare the average characteristics of claimants across the treatments and the control group. If random assignment was carried out effectively, differences in the average characteristics of the treatment and control group participants should be small and, for the most part, not statistically significant. Table III.7 presents

		(1.0)	(0.9)	(9.9)	(0.9)	(0.9)	(1.1)
Hispanic (percent)	3.5	3.9 (0.6)	3.7 (0.5)	3.9 (0.5)	2.7 (0.6)	3.8 (0.5)	3.8 (0.6)
Age Less than 35 (percent)	53.5	53.1 (1.6)	54.7 (1.3)	56.4 ** (1.5)	56.4 ** (1.5)	53.2 (1.4)	52.7 (1.7)
Age Greater than 54 (percent)	10.9	10.9 (1.0)	10.1 (0.8)	10.6 (0.9)	11.0 (0.9)	11.5 (0.9)	9.4 (1.0)
Expected Recall (percent)	10.8	10.0 (1.0)	10.4 (0.8)	10.8 (0.9)	10.3 (0.9)	10.0 (0.9)	10.0 (0.9)
Weekly Benefit Amount (dollars)	164.1	165.5 (2.2)	166.4 (1.9)	167.1 (2.0)	165.1 (2.1)	167.8 * (2.0)	166.9 (2.3)
Base Period Earnings (thousands of dollars)	14.13	14.38 (0.34)	14.65 * (0.29)	14.36 (0.31)	14.32 (0.32)	14.70 * (0.31)	14.30 (0.36)
Sample Size	3,354	1,385	2,428	1,885	1,745	1,831	1,285

NOTE: Treatment-group means are regression-adjusted for differences in the timing of sample intake between treatment and control groups.

*Significantly different from the control group mean at the 90 percent level of confidence.
**Significantly different from the control group mean at the 95 percent level of confidence.

statistically significant at the 95 percent level, and none was significant at the 99 percent level. Although the 6 significant differences found at the 90 percent confidence level are one more than the expected number given random assignment and independent hypothesis tests, we believe that the number is consistent with random assignment, because not all of the 48 hypothesis tests are independent.[15]

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[15]There are two reasons that all 48 tests are not independent. First, the treatment-control differences for any given measure are not independent because each treatment group mean is compared with the same control group mean. If one of the treatment-control differences for a given measure was statistically significant, then the differences for the other treatment groups would also likely be significant. For example, if by some random chance the control group mean is a relative outlier for some measure, then the tests for all treatments are more likely to reveal significant treatment-control differences for that measure. Hence, the six treatment-control comparisons for this measure are affected by the same random outlier, and the tests are thus not independent.

Second, some of the characteristics measures are closely related, implying that the tests for different measures but for the same treatment plan are related. For example, the weekly benefit amount depends on base-period earnings. If the weekly benefit amount were significantly greater for some treatment plan relative to the control group, then it is likely that the base-period earnings would also be greater for that treatment group relative to the control group. Hence, the hypothesis tests for the two measures were not independent because the measures were not independent.

informed about the bonus amount and qualification period and the rules for obtaining and claiming a bonus. In addition, claimants in treatment groups 1 through 5 were to receive information on the demonstration workshop, and claimants who expressed interest were scheduled for the next session.

The validity of the demonstration results depends on whether these offers were made correctly and at the planned time. It also depends on whether the process used in the demonstration provided claimants with sufficient information that they understood the offer as thoroughly as they would in an ongoing program. Similarly, an important consideration is how the process might differ in an ongoing program.

This chapter addresses these questions. In Section A, we describe the bonus and workshop offer process in detail. Then, in Section B, we examine the extent to which treatment group members received and understood the offer. Section C provides conclusions about the bonus and workshop offer process. Overall, we find that 94 percent of claimants who should have been offered a bonus were offered one. Sixty-five percent of the claimants who were not offered a bonus collected fewer than three weeks of UI benefits. We also conclude that claimants generally received the correct offer, and that they appear to have understood the offer. Similarly, the workshop was also offered, as we address in more detail in the following chapter.

to facilitate the bonus and workshop offer process. In the first step, payment "stops" were placed in the state computer system on the individual claim records of treatment group members. These payment "stops" allowed local office UI staff to identify claimants who were to be offered the bonus. Claimants were identified when the system was used to process initial UI claims. In the second step, information sheets and bonus claim forms were generated and sent to the local offices. These forms were tailored to each selected treatment group member and were to be handed to eligible claimants during the orientation sessions. Two lists that contained claimant-specific bonus offer information were also produced for use by local office staff. The objective of this process was to ensure that "stops" were placed on the system and that the packages of orientation materials were received by the local offices before selected claimants signed for a week of UI.[1]

2. Identifying Treatment Group Members and Referring Them to Orientation

Local office UI interviewers were trained to look for the special demonstration "stop" when claimants signed for a waiting week or a first compensable week. When they noticed a demonstration "stop," they were to take the individual's UI claim, as they would ordinarily, and then refer the claimants to the Job Service (JS) for the orientation session. As part of this referral, claimants were

[1]Although most claimants signed for both their waiting week and their first compensable week about two weeks after applying for benefits, some claimants were scheduled to return to the local office after only one week to sign for their waiting week. These claimants could return to the office as early as the day after they were selected for the demonstration. Thus, a few individuals returned to the office prior to the setting of the stops. These individuals were offered the bonus the next time they returned to the office.

steps.

1. ***Verifying Eligibility and Determining the Correct Offer Parameters.*** Local office staff first verified that each claimant had signed for a waiting week or first payment within six weeks after his or her benefit application date. This procedure ensured that the bonus offer was made early in each individual's unemployment spell. The six-week cutoff date for each claimant was printed on the claimant lists generated by the central office, and staff were instructed to review that information prior to conducting each orientation session. The interviewers also reviewed the central office claimant lists to determine the correct bonus parameters to be offered (the bonus amount, the number of weeks in the qualification period, and workshop eligibility).

2. ***Providing the Bonus Offer.*** Staff at the local offices were guided by a script to provide a verbal explanation of the bonus amount, the qualification period, the rules for obtaining a bonus, and the process for claiming a bonus. The staff also gave each claimant a tailored Bonus Information Sheet that reiterated what the staff member had described. In addition, staff gave the claimant a form--the Notice of Hire--for initiating the bonus claim process when the claimant found a job. Staff training emphasized that interviewers provide the information on the bonus in a clear, correct, and encouraging manner.

3. ***Confirming That the Claimant Understood the Bonus Offer.*** The bonus offer script contained a series of questions to ensure that the claimant understood the bonus offer. These questions asked claimants to recite their bonus amounts and qualification dates and the bonus eligibility rules (or to point to the place on the Bonus Information Sheet where that information was provided).

4. ***Offering the Job-Search Workshop.*** The script to guide the orientation session included a section that described the workshop and encouraged the claimant to participate. This section was given to claimants eligible for the workshop. As discussed in more detail in the next chapter, the workshop offer was modified because claimants expressed little interest in the workshop during the first three months of full-scale operations. The

[2]"Stops" were kept on the system until claimants completed the orientation session, to ensure that all treatment group members who signed for a week of UI received information on the bonus program. In those occasional instances when staff were aware that a claimant did not report to JS after being referred, they were expected to mail the orientation materials to the claimant.

Local offices were given the option of conducting individual orientation sessions, in which a JS interviewer provided information only to one claimant at a time, or group sessions, involving more than one claimant at a time. Although most offices conducted individual orientation sessions, two offices--Butler and Connellsville--chose to conduct group sessions.[3]

On average, staff devoted about 10 minutes to completing the standard orientation steps, although the sessions sometimes lasted 20 to 25 minutes. The length of each session depended on the number of claimants in the session, the level of effort that was required to ensure that claimants understood the offer, and the personal style of the staff person who conducted the session.

B. ASSESSMENT OF THE BONUS AND WORKSHOP OFFER PROCESS

The demonstration was successful at providing bonus and workshop offers to the majority of treatment group members. Approximately 94 percent of the claimants who signed for a week of UI attended orientation. Variation among offices was modest--ranging from 91 percent at the two Philadelphia offices to 97 percent at Butler and Connellsville. Furthermore, the orientation occurred in a timely manner--an average of about 17 days after the benefit application date. The majority of claimants attended orientation within three weeks after their benefit application dates, and only 24 claimants attended orientation after the six-week point.

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[3]These offices adopted this approach several months into the demonstration, when they began to conduct group Benefit Rights Interviews (BRIs) with claimants who were signing for their waiting week or first compensable week of UI. The Butler and Connellsville offices found that channelling demonstration claimants directly into a group orientation session following the BRI, rather than conducting a series of individual sessions, was more efficient.

Second, staff errors and the refusal of claimants to participate accounted for the remaining two-thirds of nonparticipants. During the first few months of full-scale operations, several offices did not make JS referrals for claimants who did not appear to be eligible for the demonstration (for example, claimants who expected to be recalled or who had already returned to work). Staff also sometimes inadvertently overlooked the "stop" notation on the computer system. Finally, some claimants did not report to the JS even after being referred because they did not understand the referral or because they refused to stay for the session.[4]

Both site observations and data from the claimant interviews suggest that the orientation sessions were successful at providing the bonus offer and workshop information. Staff appeared to offer the correct bonus amounts and qualification dates, both verbally and by handing out the Bonus Information Sheets, and staff used the verification questions as they were written in the script. Staff also appeared to follow procedures in offering the workshops, although, in the early months of the demonstration, some offices were slow to offer the bonus.[5]

Staff reported that most claimants seemed to understand the bonus offer and that they had very few questions. Most of the questions seemed to call for a clarification of information and a verification that the offer was real. These staff perceptions were confirmed by the claimant interview

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[4]If the claimants arrived late in the day or could not stay for the session, offices rescheduled claimants for another day. Some of these claimants never returned to the office to complete the orientation sessions, accounting for some of the refusals to attend orientation.

[5]See the further discussion of the workshop in the following chapter.

probably explains the claimants' inability to remember the workshop offer.

The preceding discussion indicates that 94 percent of the claimants received a bonus offer, and that they appeared to understand it. Yet it is likely that a higher percentage of claimants would receive a bonus offer in an ongoing program--a factor that could affect the generalizability of the demonstration findings. However, over half (54 percent) of the claimants who did not attend orientation signed only for the waiting week and did not collect any UI benefits, and another 11 percent collected only one or two weeks of UI benefits. Thus, it is unlikely that a fuller knowledge of the bonus offer would affect the impacts on UI receipt. However, it might lead to a higher rate of bonus receipt, since some individuals who did not receive an offer could have been eligible for a bonus.

C. CONCLUSION

The discussion in this chapter leads to the following conclusions:

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[6]Non-English-speaking claimants might be expected to have had some difficulty in understanding the bonus offer, and there is some evidence that this was the case. For example, 18 percent of the Hispanic claimants who remembered the bonus offer felt that the explanation of the bonus was difficult to understand, compared with 6 percent of non-Hispanics. One possible reason for this finding is that efforts to provide non-English-speaking claimants with a translation of the bonus offer were quite limited. Most offices expected non-English-speaking claimants to bring their own translator to the office. If a demonstration claimant did not bring a translator and the office had a translator on staff, the bonus offer was translated by this staff person. However, if no translator was available, the JS staff person provided the offer in English and then encouraged the claimant to take the handouts to someone who could translate them for the claimant.

consistent manner. However, during the early months of the demonstration, some offices were slow at offering the workshop in a routine manner.

It is important to remember that exposure to the opportunities and requirements of the bonus would be more widespread in an ongoing program--a factor that has implications for the generalizability of the demonstration findings and for the design of an ongoing bonus program. As noted earlier, the fact that 65 percent of the claimants who did not receive a bonus offer collected fewer than three weeks of UI benefits suggests that a more in-depth knowledge of the bonus offer would not affect the impacts on UI receipt. However, it could affect the rate of bonus receipt.

In terms of design implications, it is likely that less information about the bonus would be required in a face-to-face interview, making it feasible for UI interviewers to provide the offer directly when a claimant signs for a week of UI. In addition, it might be feasible just to mail the bonus offer information to claimants, rather than requiring a face-to-face interview. Both of these alternative approaches would eliminate the need for the type of system used in the demonstration to refer claimants to JS and to monitor the compliance of claimants with that referral. The approach that is selected would appear to depend on the amount of information that must be provided verbally in order to ensure that claimants understand the bonus offer and its parameters, as well as on the availability of staff, and the relative costs of the different approaches.

provide claimants with the information and skills necessary to develop a structured job-search plan. As indicated in Chapter I, the demonstration workshop and assessment session--which were strictly voluntary--were to be offered to claimants in treatments 1 through 5 but not to claimants in treatment 6. The effectiveness of job-search assistance was to be evaluated on the basis of comparisons between the UI and employment outcomes of treatment group members who were offered this assistance with those who were not offered this assistance.

This chapter describes the demonstration job-search workshop and assessment session and considers the role that these components might play in an ongoing bonus program. In Section A, we begin with an overview of the workshop and assessment design. In Section B, we show that workshop and assessment session participation rates were very low (less than 3 percent), and we conclude that these low rates of participation were due primarily to a lack of interest among claimants rather than to implementation problems. Section B also examines the characteristics of claimants who participated, and concludes that claimants who chose to participate were likely to be individuals who were not job-attached. In Section C, we describe the implementation of the workshop and assessment sessions, and we report that participants generally responded favorably to the workshops. In Section D, we present our conclusions about the workshop and assessment components.

identifying, targeting, and researching potential employers. The third, "Contacting Employers," covered the application process, resume writing, initial employer contacts, and job interviews. The final module, "Job Search Campaign and Assessment," reviewed what was covered in the previous modules, and showed participants how to develop their own job-search plan.

These four modules contained six teaching techniques: instruction by the workshop leader, group discussion and interaction, workbook activities, videotapes on job search, exposure to local and state employment resources, and mock interviews. The designers of the workshop also made group interaction a key component of the workshop to enable participants to obtain support from others in the same situation. Group discussions and activities were expected to allow participants to share ideas and to expand their opportunities for employment information. The ideal workshop size was considered to be between 5 and 10 claimants, to facilitate the envisioned group discussion, interaction, and support. The trainer's guide presented key topics and suggestions for presentation, but each workshop leader was afforded the latitude to cover the material in a way that best suited his or her style. Despite this flexibility, all of the topics in the trainer's guide were expected to be covered.

In order to cover the workshop material in sufficient depth and breadth to afford participants a real chance of benefitting from them, the workshop was to be conducted as four group sessions, spread across four half days or two full days (14 to 16 hours in all). Local office staff were also

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[1]Both sets of materials were derived from previous workshops provided in Pennsylvania, from the workshop used in the New Jersey UI Reemployment Demonstration, and from other available materials.

C. Selecting a Job Goal

II. LOCATING EMPLOYERS

A. Identifying Sources To Find Employers

B. Selecting Sources

C. Networking

D. Targeting Employers

E. Researching Employers

III. CONTACTING EMPLOYERS

A. Application Completion

B. Resume Writing

C. Initial Employer Contacts

D. Job Interview

IV. JOB SEARCH CAMPAIGN/ASSESSMENT

Assessment sessions were to be held as soon as possible after the workshop.

B. WORKSHOP AND ASSESSMENT PARTICIPATION

In this section, we report data which show that rates of participation in the job-search workshop and assessment session were very low. We also consider the reasons for the low participation rates, and conclude that they were due to the fact that few claimants were interested in the workshops or the assessment sessions. We also examine the characteristics of claimants who participated in the workshops.

1. Workshop and Assessment Participation Rates

The data in Table V.1 show that participation in the job-search workshops and assessment sessions was very low. Overall, 11 percent of the individuals who were offered a workshop showed enough interest in the workshop to be scheduled for a session, but only 3 percent actually participated in one or more sessions. Only 1 percent completed the postworkshop assessment interview. These low participation rates occurred in all offices, and, although not shown in the table, they occurred among all treatment groups.

The low level of workshop participation may have been due to administrative problems that affected the delivery of services, but our evidence does not seem to support this case. As reported in the previous chapter, 94 percent of the claimants who should have received both a bonus offer and

PARTICIPATION IN THE DEMONSTRATION JOB-SEARCH WORKSHOP AND ASSESSMENT SESSION, BY OFFICE

	Coatesville	Phila-North	Phila-Uptown	Reading	Lancaster	Lewistown	Butler	Connellsville	McKeesport	Erie	Pittston	Scranton	All Offices
Claimant Participation													
Percent of Claimants Who Were Offered the Workshop Who:													
Showed Interest	20.9	4.8	24.3	4.4	14.5	14.0	14.3	1.5	15.7	11.2	8.7	0.5	11.3
Attended at least one session	2.6	1.2	3.7	1.3	3.9	3.4	2.7	0.7	3.5	4.6	3.4	0.1	2.6
Completed assessment	1.6	0.5	1.3	0.9	0.4	0.9	0.5	0.2	1.6	2.0	2.7	0.0	1.0
Number of Claimants Offered the Workshop[a]	757	736	794	803	807	534	772	677	754	783	670	757	8,844
Number of Workshops													
Scheduled	18	18	30	19	20	21	31	20	16	15	19	19	246
Scheduled, with Interested Claimants[b]	18	10	25	14	20	19	23	8	16	13	13	3	182
Held	10	4	11	6	19	9	10	5	12	9	7	1	103

NOTE: The sample consists of claimants in treatments 1 through 5 who completed an orientation session.

[a]The demonstration job-search workshop was to be offered to individuals in treatments 1 through 5 only. Nevertheless, 12 claimants in treatment 6 were offered the workshop and expressed interest in it. Because we do not know the number of claimants in treatment 6 who were offered the workshop but did not express interest, this table includes only claimants in treatments 1 through 5.

[b]This number represents a count of scheduled workshops in which at least one interested claimant signed up to attend.

a workshop offer did meet with the Job Service staff who made these offers. Data from the survey also indicate that most claimants who attended the orientation session remembered the bonus offer, while only about one-third remembered the workshop offer. However, the fact that the workshop offer was included in the orientation script suggests that the workshops were offered as planned. This conclusion is buttressed by observations made during site visits and by data on workshop scheduling (see Table V.1). These data indicate that workshops were scheduled every two to three weeks, yet over half of the scheduled workshops were canceled because not enough claimants were scheduled or because those who were scheduled failed to report.[2]

The low rate of participation in the workshop during the first three months of full-scale operations led to some changes in the workshop offer to encourage greater participation. Offices were required to schedule workshops on a weekly or biweekly basis, encouraged to conduct two full-day workshops rather than four half-day workshops, encouraged to conduct scheduled workshops even if only one claimant attended, and encouraged to provide pre-workshop reminders to claimants who signed-up and to contact no-shows for rescheduling.[3] Staff were also encouraged to "sell" the workshop enthusiastically, to provide a Workshop Information Sheet in addition to the verbal offer, and to inform claimants about the start-date of the next workshop prior to asking them whether they were interested. These revised guidelines to promote workshop participation generally took effect in April 1989. The efforts appear to have increased participation rates slightly during the second and

[2]Data on workshop participation indicate that participants started the workshop an average of 16 days after the bonus offer was made.

[3]Several of the offices initially had difficulty in conducting workshops due to the availability of staff and conference rooms. State staff worked with each of these local offices to resolve these problems.

third quarters of 1989, although participation rates remained very low to the end of the demonstration.[4,5]

Since implementation or other administrative problems do not appear to explain the low level of workshop participation, it seems likely that the main reason was the general lack of interest among claimants. The claimant interviews support this conclusion. It indicated that, among claimants who did not attend the workshop (but who remembered that it was offered), 39 percent had already become reemployed, were awaiting recall, or expected to become reemployed. Another 32 percent did not believe that the workshop would help them obtain a job. Other claimants stated that transportation and child-care problems prevented them from participating in the workshop.

Staff also reported that some claimants did not accept the workshop offer because they expected to be recalled (or had already returned to work), and that others believed that they already knew how to search for work and would not have trouble finding a job. The low level of claimant interest is not surprising, given that the state unemployment rate was low (4.5 percent) during the demonstration period and that the workshop was offered early in an individual's unemployment spell. Staff also indicated that some claimants felt that the workshop would limit the time available to them to look for work, suggesting that the bonus offer itself may have reduced interest in the workshop.

2. The Characteristics of Participants

Table V.2 (administrative records) and Table V.3 (interview data) report the characteristics of workshop participants (that is, claimants who attended at least one workshop session) and the

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[4]Participation rates throughout full-scale operations were as follows: 1.9 percent in fourth-quarter 1988; 2.6 percent in first-quarter 1989; 3.8 percent in second-quarter 1989; 2.1 percent in third-quarter 1989; and 1.4 percent in fourth-quarter 1989.

[5]Some offices that maintained good reminder and follow-up records found that these techniques were generally not very effective at encouraging greater participation. For example, the Butler and Erie offices found that during a reminder telephone call one or two days prior to the workshop only about half of the claimants who told staff that they would attend actually did attend. And only 2 of the 18 no-shows in the Philadelphia-Uptown office who were rescheduled (some more than once) ever attended.

TABLE V.2

CHARACTERISTICS OF WORKSHOP PARTICIPANTS VERSUS NONPARTICIPANTS
(Records Data)

	Workshop Participants	Nonparticipants
Demographic Characteristics		
Gender (percent)		
Female	52.8	40.4
Age		
Mean age	39.9	35.5
Race/Ethnicity (percent)		
White	86.0	84.7
Black	12.2	11.1
Hispanic	1.3	3.7
Other	0.4	0.5
Number of Dependents (percent)		
0	71.6	72.1
1	14.4	11.7
2 or more	14.0	16.3
Pre-UI Job Characteristics		
Mean Base-Year Earnings	16,496	14,482
Reason for Separation (percent)		
Laid-off	48.9	57.6
Quit	4.4	4.9
Fired	14.0	15.0
Retired	0.0	0.2
Other	32.8	22.2
Had Date of Expected Recall (Percent)	5.2	10.9
UI Entitlement		
Mean Weekly Benefit Amount	178.9	166.4
Percent at the Maximum Benefit	23.6	19.8
Sample Size	229	8,615

NOTE: Percentages may not sum to 100 due to rounding.

TABLE V.3

CHARACTERISTICS OF WORKSHOP PARTICIPANTS VERSUS NONPARTICIPANTS
(Interview Data)

	Workshop Participants	Nonparticipants
Demographic and Household Characteristics		
Highest Diploma or Degree Attained Required (Percent)		
Less than high school	8.5	16.2
High school/GED/vocational/ technical/business	75.0	73.9
College graduate	16.5	9.9
Spouse/Companion Worked at Time of UI Application (percent)	42.7	37.5
Household Income in Year Before UI Application		
Mean income	$23,696	$25,366
Mean Years of Work Experience	21.1	16.2
Pre-UI Job Characteristics		
Industry (percent)		
Agriculture/forestry/fishing/mining	0.0	2.5
Construction	4.0	12.1
Durable manufacturing	19.2	21.2
Nondurable manufacturing	6.3	14.0
Transportation/public utilities	6.2	7.7
Wholesale and retail trade	27.9	18.3
Finance/insurance/real estate/services	34.9	22.9
Public administration	1.5	1.3
Worked in a Seasonal Industry (percent)	5.5	18.7

TABLE V.3 (continued)

	Workshop Participants	Nonparticipants
Occupation (percent)		
Managerial/professional/technical/ sales	32.9	19.3
Administrative support	32.3	17.8
Service occupations	10.9	11.2
Mechanics/repairers/construction/ precision production	1.9	13.8
Machine operators/transportation/ handlers	22.1	35.9
Farming, forestry, and fishing	0.0	1.9
Received Severance Pay When Left Job (percent)	15.1	10.5
Mean Weeks between Pre-UI Job and Claim Date	2.0	2.7
Had Previous Layoff from Pre-UI Job (percent)	5.9	22.9
Sample Size	82	2,911

NOTE: Percentages may not sum to 100 due to rounding

characteristics of nonparticipants. The data in these tables indicate that participants were more likely to be female and were older than nonparticipants.[6] Participants were also more likely to have higher educational levels, higher weekly benefit amounts, and higher base-year earnings than non-participants. In addition, data on industry and occupation show that workshop participants were more likely than nonparticipants to have worked in white-collar occupations and less likely to have worked in nondurable manufacturing and construction.

These differences are consistent with the fact that participants were less likely than nonparticipants to be on temporary layoff (that is, to have a definite recall date). Individuals who expected to be recalled would have had little incentive to attend a workshop, and some may even have been recalled before the start of the workshop.

C. THE IMPLEMENTATION OF THE WORKSHOP AND ASSESSMENT SESSION

1. The Implementation of the Workshop

The workshop leaders used the trainer's guide as their primary resource and relied on most or all of the teaching techniques described earlier in Section A. Staff at several offices also supplemented the workshop manuals with additional handouts and materials, such as local employment brochures, newspaper clippings, and materials from other workshops in which they had participated in the past.

The low participation rate in the demonstration workshops--an average of only two to three participants, with a maximum of seven participants--limited any group interaction that was expected in the workshop. However, these low participation rates provided more flexibility than was expected to address the individual needs and interests of claimants. For example, some workshop leaders indicated that most participants were already skilled in basic job-search techniques. These claimants wanted to refine their existing skills and were interested in covering only selected topics. In response,

[6]All of the differences in the characteristics reported in this section are statistically significant at the 95 percent confidence level in a two-tail test.

about one-half of the local offices covered the standard workshop material briefly and then devoted more time to the components in which the participants were interested or needed help. The other half of the offices used the standard materials as a base, but covered only those topics in which claimants expressed interest.[7] In either case, this individual attention might not have been possible with larger groups.

Both the limited opportunity for group interaction and the narrow focus of topics also tended to contribute to shorter workshops than had been planned. The small group sizes also seem to have exerted some pressure on management to limit the time spent in the workshops. That is, local office managers expressed concern about allocating a staff person for 14 to 16 hours every other week, just to serve the small number of claimants who showed up for the workshop. In light of these factors, the workshops in about one-half of the offices typically lasted only one-half to one full day. Butler, Erie, Lewistown, Philadelphia-Uptown, and Pittston tended to follow the time guidelines more closely, although these offices also condensed the workshop when only one or two claimants attended.

2. The Implementation of the Assessment Session

The assessment session was expected to provide more individualized attention than would be possible in the group workshop setting, but, in practice, the workshops were often able to provide this level of attention given the small number of participants. Nevertheless, offices still offered workshop participants a separate one-on-one assessment session. These sessions were held an average of five days after the workshop, and lasted one-half to one hour, depending on the workshop leader and on the needs of claimants.

In the assessment sessions, the workshop leader reviewed the claimant's employment goals and a list of targeted employers. The leader also ascertained what the claimant had done to look for work since the workshop, discussed any job-search problems unique to that individual, and reviewed what

[7]At these offices, workshop leaders typically began the session by describing the entire set of available topics in a general manner, and then covered in more detail only those topics in which participants were interested.

the claimant had learned in the workshop. In some offices, the leader also reviewed the claimant's resume and/or conducted a mock interview if these had not been part of the workshop. Indeed, a few offices conducted the mock interviews to emulate real interviewing situations. For example, the workshop leader at Erie tried to arrange for another staff person to conduct the mock interview to enable claimants to gain interviewing experience with someone unfamiliar to them. The workshop leader at Philadelphia-North required that claimants come to the assessment session dressed for an interview, so that she could provide them with feedback on their appearance.

3. The Reaction of Participants to the Workshop

Participating claimants had a positive response to the workshops. The claimant interviews indicate that 39 percent of the workshop participants rated the workshop as "very useful." Another 42 percent rated the workshop as "somewhat useful." The remaining 19 percent felt that the workshop was "not very useful" or "not useful at all." In addition, the interview data indicate that workshop participants were more likely to say that they looked for work after their layoff (94 percent) than were nonparticipants who did not expect to be recalled (82 percent). Among those who searched for work, mean hours of job search per week were also higher among participants than among nonparticipants (16.9 hours versus 14.7 hours).

D. CONCLUSION

The analysis in this chapter indicates that:

- The participation rate for the demonstration job-search workshop was very low (2.6 percent), due primarily to the general lack of interest among claimants.
- Individuals who participated in the workshop were less likely than nonparticipants to be job-attached.

These results are not surprising given the strong economy within which the demonstration operated and given that the workshop was offered early in an individual's unemployment spell. Moreover, the

fact that the bonus was offered at the same time that the workshop was offered may have provided a disincentive to participate in the workshop.

Based on these findings, two considerations should guide decisions about whether a job-search workshop should be incorporated into an ongoing program. First, the usefulness of a job-search workshop depends on its effect on reemployment. While the low rate of workshop participation makes it impossible to measure this effect in this demonstration, the results from other demonstrations (for example, Corson et al., 1989, on the New Jersey UI Reemployment Demonstration Project) indicate that job-search assistance, including workshops and individualized assessments, can reduce weeks of UI and increase reemployment rates.

The second consideration is whether participation would be high enough to make implementation feasible. Given a participation rate of 2.6 percent, a single office would need to offer the workshop to 96 claimants per week (or about 5,000 claimants per year) in order to operate a workshop with a minimum of 5 participants on a biweekly basis. Given this participation rate, the workshop would not be feasible for the vast majority of all the local offices in Pennsylvania.[8]

This implementation problem could be addressed by making participation in the workshop mandatory, as it was in the New Jersey demonstration. An alternative approach that might also increase participation would be to offer the workshop both when a claimant begins collecting UI and when the bonus qualification period ends for claimants who are still unemployed. Claimants who initially expected to be recalled or who might initially have believed that they could obtain a job on their own might be more interested in a job-search workshop at this later point in time.

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[8]This estimate was based on the number of first payments at Pennsylvania local offices in 1989. In addition, we assumed that only about 50 percent of first payments would have been offered the workshop, since approximately 50 percent of UI applicants were screened out of the demonstration.

VI. BONUS CLAIMS

Claimants who were offered a reemployment bonus and who obtained a job that they believed qualified for a bonus could submit a claim for a reemployment bonus. In the first step of this bonus claim process, the claimant submitted a form when he or she first obtained a job. If the job appeared to meet the qualifying conditions, the local office then sent the claimant an additional set of forms, which the claimant was to submit after 16 weeks of work. The claimant then received a bonus if he or she met all qualifying conditions and when these conditions were verified.

In this chapter, we discuss this bonus claim process in more detail. In Section A, we describe the design of the bonus claims process. In Section B, we examine how the claims process was implemented, and conclude that the claims process worked satisfactorily. In Section C, we present data which show that 7 to 8 percent of the claimants with 6-week qualification periods received a bonus, while 10 to 14 percent of the claimants with 12-week qualification periods received a bonus. We also examine the characteristics of the individuals who received a bonus. Finally, we examine the degree to which claimants who appeared to be eligible for a bonus actually filed claims. We conclude that, at the maximum, 6 percent of the claimants who may have been eligible for a bonus did not apply for one.

A. DESIGN OF THE BONUS CLAIM PROCESS

The bonus claims process was intended to:

- Be easy for claimants to understand
- Enable staff to process claims and make payments to claimants in a timely manner
- Provide staff with adequate information to make eligibility decisions
- Provide adequate accounting controls for the bonus payments

A claimant who obtained a job during the bonus qualification period could claim a bonus by sending two sets of forms to the central office. One form--the Notice of Hire--was submitted when the claimant started to work on his or her bonus-qualifying job. This form reported the start date of the qualifying job, the number of hours worked per week on that job, and contact information for both the qualifying job and for the job held just prior to the initial receipt of UI benefits.

Each Notice of Hire was examined by central office staff to determine whether the claimant appeared to be eligible for a bonus, based on the information provided on the form.[1] If the claimant appeared to be eligible, he or she was sent a second set of forms--the Bonus Voucher and the Job Change Form--which were to be submitted 16 weeks after the start date of the qualifying job. The Bonus Voucher reported current employer contact information and average weekly hours and certified that the claimant met all of the other criteria for claiming a bonus. This form was all that was required if a claimant worked at only one job during the 16-week work period and was not self-employed.

Claimants who worked at more than one full-time job during that period were required to submit a Job Change Form in addition to the Bonus Voucher. The Job Change Form required that claimants record employer contact information, job start and end dates, and average weekly hours for all jobs during the 16-week period. This form allowed staff to determine whether the claimant engaged in continuous full-time employment during the work period.

Claimants who were self-employed at any time during the 16-week period were also required to submit proof of self-employment along with the Bonus Voucher (and the Job Change Form if necessary). Acceptable proof included a copy of an IRS Employer's Quarterly Federal Tax Return, an annual federal tax return showing self-employment income, an Employer's Initial Statement for UI, or a state or municipal business license.

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[1]Staff contacted claimants when more information was required. Staff also looked for any information provided by the claimant which indicated that he or she was ineligible (such as whether the job start date was beyond the qualification period, or whether the job averaged less than 32 hours per week).

The Bonus Voucher and accompanying materials were reviewed by central office staff to determine the claimant's eligibility for the bonus. In this step, staff first used the state computer system to determine whether the claimant was *ineligible* for the bonus because he or she was no longer eligible for UI or because he or she had claimed UI during the 16-week work period. If the claimant still appeared to be eligible for the bonus, staff verified the employment information by telephoning each employer listed by the claimant. Staff also mailed written verification forms to employers who refused to provide information by telephone, who were unable to be contacted by telephone, or whom staff felt may not have been checking their records during the call. However, most employers were willing to review their records and to provide information by telephone.

Finally, staff made an eligibility decision based on the information obtained, and entered that decision, as well as the employment information, into the demonstration's computer system. Staff used this computer system to produce a personalized letter to the claimant to indicate the eligibility decision. In addition, information to generate a bonus payment (Social Security number, name, address, and amount of payment) was sent by staff, if appropriate, to a special payment unit that produced the checks and mailed them to the claimant.

Claimants were allowed to appeal the bonus eligibility decision by filing an appeal with the unit that determined eligibility for the bonus. In most of the cases reviewed through this appeal process, the claimant's employer provided different information than did the claimant, leading to an ineligibility decision. If the claimant disputed the information provided by the employer, staff required that the employer verify the employment information in writing.[2]

[2]Very few of the decisions were appealed.

B. THE IMPLEMENTATION OF THE BONUS CLAIM PROCESS

Our analysis suggests that the four design goals for the bonus claim process were generally met. First, as discussed in the previous chapter, our interview data suggest that most claimants did understand the bonus offer and how to file a claim. In addition, few claimants who submitted forms completed them incorrectly or omitted requisite information. Second, bonus claims were processed quickly; Notices of Hire were processed within an average of 8 days, and Bonus Vouchers were processed within an average of 11 days. Third, the process seemed to provide staff with the necessary information to make eligibility decisions, although staff took additional verification steps when they felt uneasy about the information that was provided.[3] Finally, the system appeared to provide adequate accounting controls.[4]

Despite the success at meeting the design goals, the staff who processed the claims raised several issues. First, they felt that the verification of employment information was important in detecting potential fraud--particularly the verification of start and end dates of employment, which affected eligibility not only for the bonus, but also potentially for UI. However, the process of verifying employment via telephone calls with the employer may not be feasible in an ongoing program. Alternative procedures might include (1) requiring that the claimant submit a verification form to his or her employer, which could then be sent directly to the central office, or (2) requiring that claimants submit paystubs. No matter what procedures are used, the goal should be to obtain correct information in a timely fashion.

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[3]For example, staff sent written verification forms to employers who did not appear to review the claimant's records as they answered questions about the claimant's employment. Staff also reviewed the pattern of unemployment claims and sent letters to claimants to request more information about employment during gaps in UI receipt.

[4]Several mechanisms were in place to account for bonus claims. For example, the demonstration computer system calculated the bonus amount and printed it on a hard-copy list for transfer to the payment unit; the individual who entered a bonus payment eligibility code was also required to enter a personal identification code in the computer system; and the computer system was used to monitor bonus offers, payments, and outstanding obligations.

The verification of self-employment was also a difficult aspect of processing claims.[5] The state requested that self-employed claimants submit one of several forms of proof of self-employment, but these forms of proof were not always available or, at least, not available in a timely way (for example, tax forms). In these cases, staff either placed the bonus eligibility decision on hold until the tax forms were available or worked with the claimant to identify another reasonable source of verification.

Finally, the most difficult eligibility decisions occurred when the job ended just before the end of the 16-week work period or when a claimant was recalled. In the first case, the eligibility requirements stipulated that the claimant be employed continuously during the 16-week work period, but allowed the claimant to change jobs during the period as long as the interim between jobs was less than 5 days. Although someone who was laid off just 2 days prior to the end of the 16-week period was not actually working on the end date of the work period, he or she technically was not obligated to start new employment until after the 16-week period had elapsed. For this reason, such demonstration claimants were deemed to be eligible for the bonus, assuming that they met all other conditions.

In the second case, it was not always easy to determine whether an individual had been recalled by the separating employer. For example, one claimant started to work for the company that had *purchased* his separating employer. In another instance, a claimant applied for and obtained a job with his separating employer within another department. In both these cases, and in similar situations, the eligibility decision was often based on whether or not the employer defined the hire as a recall.

[5]About 2 percent of the bonus jobs entailed self-employment.

C. REEMPLOYMENT BONUS RECEIPT

1. The Rate of Bonus Receipt

About 15 percent of the claimants who were assigned to a treatment submitted a Notice of Hire (Table VI.1.).[6] As expected, this percentage was lower (by about 12 percent) among claimants who were offered a bonus with a 6-week qualification period, and was higher (by 14 to 20 percent) among those who were offered a 12-week qualification period (the difference was statistically significant). The highest rates occurred for the most generous treatments.

About 80 percent of the claimants who submitted a Notice of Hire were judged to be potentially eligible for a bonus, based on the information on the Notice of Hire. These individuals were sent the Bonus Voucher and Job Change forms. Eighty-eight percent returned these forms after 16 weeks. Almost all individuals who submitted a Bonus Voucher were then judged to be eligible for a bonus and received a payment.

Overall, 11 percent of the claimants received a bonus. This bonus receipt rate ranged from 7 to 8 percent for bonus offers with a 6-week qualification period to 10 to 14 percent for bonus offers with a 12-week qualification period.[7] Again, the highest rate occurred for the most generous bonus offer.

Most claimants who were judged to be ineligible (at either the Notice of Hire or Bonus Voucher stage) were not eligible for UI or had a job start date that occurred after the qualification period. These two reasons accounted for, respectively, 37 and 30 percent of the ineligible cases. Another

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[6]The base used for the calculations in the table consists of claimants in the analysis sample. Since 6 percent of these claimants did not actually receive a bonus offer, the rates at which a Notice of Hire was submitted and a bonus was received by those offered a bonus is roughly 6 percent higher than the figures shown in the table. For example, 15.9 percent of the claimants who were offered a bonus submitted a Notice of Hire, compared with 15 percent of the full analysis sample.

[7]The differences in bonus receipt rates between the 6- and 12-week bonus offers were statistically significant.

TABLE VI.1

REEMPLOYMENT BONUS CLAIM RATES,
BY TREATMENT GROUP

	Treatment Group						
	1 Low Bonus, 6 Weeks	2 Low Bonus, 12 Weeks	3 High Bonus, 6 Weeks	4 High Bonus, 12 Weeks	5 Declining Bonus, 12 Weeks	6 High Bonus, 12 Weeks, No Workshop	Total
Percent Who Submitted a Notice of Hire	11.8	14.8	12.3	17.9	14.3	19.5	15.0
Percent Who Were Potentially Eligible Based on the Notice of Hire	8.7	12.6	10.0	14.9	12.4	16.1	12.3
Percent Who Submitted a Bonus Voucher	7.1	11.1	8.7	13.4	10.5	14.5	10.8
Percent Who Were Paid a Bonus	6.9	10.7	8.3	13.2	10.0	13.9	10.5
Sample Size	1,395	2,456	1,910	1,771	1,860	1,302	10,694

NOTE: The analysis sample is the base used in the table.

15 percent were ineligible for a bonus because their job was with their pre-UI employer. The remaining ineligible cases were individuals who did not work full-time or did not work for 16 weeks.

2. Bonus Payments

Bonus payments averaged $561 for the low-bonus treatments and $1,124 for the high-bonus treatments (see Table VI.2). Interestingly, these payments were higher than the average bonus *offers* to the entire sample, which were $500 for the low-bonus and $1,000 for the high-bonus treatments, suggesting that individuals who received higher bonus offers were more likely to collect a bonus than those who received lower bonus offers. The initial bonus offer for the declining benefit treatment averaged $1,008, compared with an average payment of $892. The payment amount was lower in this case because only 45 percent of the payments were for the full amount. Twenty-five percent were for 80 or 90 percent of the full amount, and the remainder were for lesser percentages.

The average time between the bonus offer and the start date of the bonus job occurred at about the mid-point of the qualification period for the bonus offers of fixed amounts (that is, at 3 and 6 weeks for the 6-week and 12-week qualification periods, respectively). However, the average time between the bonus offer and the start date of the bonus job was only 4 weeks for the declining bonus offer, which also had a 12-week qualification period. This finding suggests that the declining bonus offer may have encouraged individuals who responded to the offer to find and accept jobs somewhat more quickly than did those who responded to the fixed bonus offers.

3. Characteristics of Claimants Who Received a Bonus

Table VI.3 (the records data) and Table VI.4 (the interview data) report the characteristics of bonus recipients and nonrecipients. Recipients were more likely than nonrecipients to be male than female and to be white than black or Hispanic. In addition, recipients had higher base-period incomes than nonrecipients, were more likely to be college graduates and less likely to be high school dropouts, and were more likely to be employed in managerial, professional, technical, or sales

CHARACTERISTICS OF BONUS PAYMENTS,
BY TREATMENT GROUP

	Treatment Group						
	1 Low Bonus, 6 Weeks	2 Low Bonus, 12 Weeks	3 High Bonus, 6 Weeks	4 High Bonus, 12 Weeks	5 Declining Bonus, 12 Weeks	6 High Bonus, 12 Weeks, No Workshop	Total
Mean Bonus Payment (dollars)	571	558	1,134	1,109	892	1,136	906
Mean Weeks from Bonus Offer to Job Start Date	2.9	5.8	3.2	5.8	4.1	6.0	4.9
Percent Self-Employed	1.0	2.7	1.3	1.3	1.6	2.2	1.8
Number of Bonus Payments	96	262	160	233	186	181	1,118

TABLE VI.3

CHARACTERISTICS OF BONUS RECIPIENTS VERSUS NONRECIPIENTS
(Records Data)

	Paid a Bonus	Not Paid a Bonus
Demographic Characteristics		
Gender (percent)		
Female	35.8	41.1
Age		
Mean age	34.7	35.7
Race/Ethnicity (percent)		
White	94.3	83.6
Black	4.2	11.9
Hispanic	1.2	4.0
Other	0.4	0.6
Number of Dependents (percent)		
0	68.1	72.2
1	12.1	11.8
2 or more	19.9	16.0
Pre-UI Job Characteristics		
Base-Year Earnings		
Mean base-year earnings	$18,437	$13,983
Reason for Separation (Percent)		
Laid off	56.1	58.0
Quit	1.9	5.3
Fired	15.5	15.1
Retired	0.0	0.2
Other	26.6	21.4
Has Date of Expected Recall (percent)	4.4	12.2
UI Entitlement		
Mean Weekly Benefit Amount	$187	$164
Percent at the Maximum Benefit	29.2	18.9
Sample Size	1,118	8,959

CHARACTERISTICS OF BONUS RECIPIENTS VERSUS NONRECIPIENTS
(Interview Data)

	Paid a Bonus	Not Paid a Bonus
Demographic and Household Characteristics		
Highest Diploma or Degree Required (Percent)		
Less than high school	6.1	17.2
High school/GED/vocational/ technical/business	72.7	73.9
College graduate	21.2	8.9
Spouse/Companion Worked at Time of UI Application (percent)	41.2	37.5
Household Income in Year Before UI Application		
Mean income	$30,647	$24,665
Mean Years of Work Experience	16.3	16.5
Pre-UI Job Characteristics		
Industry (percent)		
Agriculture/forestry/fishing/mining	1.9	2.6
Construction	10.0	12.0
Durable manufacturing	25.0	20.6
Nondurable manufacturing	10.1	14.2
Transportation/public utilities	7.0	8.1
Wholesale and retail trade	18.6	18.2
Finance/insurance/real estate/services	26.7	22.9
Public administration	0.7	1.5
Worked in a seasonal industry (percent)	14.8	18.8
Occupation (percent)		
Managerial/professional/technical/ sales	34.2	17.9
Administrative support	20.6	18.1
Service occupations	6.4	11.3
Mechanics/repairers/construction/ precision production	15.2	13.5
Machine operators/transportation/ handlers	22.1	37.1
Farming, forestry, and fishing	1.5	1.5

	Paid a Bonus	Not Paid a Bonus
Received Severance Pay When Left Job (percent)	9.3	20.8
Mean Weeks between Pre-UI Job and Claim Date	1.9	2.7
Had Previous Layoff from Pre-UI Job (percent)	11.2	24.3
Sample Size	996	2,432

occupations and less likely to be employed in machine operator, transportation, or handler occupations.

These differences are probably due in part to the fact the groups who were less likely to receive a reemployment bonus also tend to have difficulty in finding jobs. For example, a recent study of UI recipients (Corson and Dynarski, 1990) found that the time from job loss to reemployment was longer for females than males, for blacks than whites, and for high school dropouts than high school or college graduates. Since the differences in bonus receipt by education and occupation were quite large, a further reason for the differences in bonus receipt may be differences in the capacity of individuals to understand the bonus offer and the claims process or their ability to respond to the bonus offer. However, evidence from the claimant interview suggests that most individuals who were given a bonus offer said that they understood it. Evidence in Chapter VII also shows few significant differences in bonus impacts by subgroup. In particular, the impacts were similar for females and males and blacks and whites.

4. Assessment of the Bonus Receipt Rate

We indicated earlier that about 11 percent of the individuals who were offered a reemployment bonus received one. An important question is whether this bonus receipt rate is comparable to what could be expected in an ongoing program, or whether individuals' unfamiliarity with a reemployment bonus led to a lower rate of receipt than would be observed in an ongoing program. A higher rate of bonus receipt might occur in an ongoing program because more individuals would respond to a bonus offer than in the demonstration, or because more individuals who become reemployed within the bonus qualification period would file for a bonus than in a demonstration.

We cannot examine the first possibility, but Table VI.5 reports data to examine the extent to which claimants who appeared to be eligible for a bonus did not file a claim. These data indicate that

TABLE VI.5

POTENTIAL BONUS ELIGIBILITY, BY TREATMENT GROUP

	Treatment Group						
	1 Low Bonus, 6 Weeks	2 Low Bonus, 12 Weeks	3 High Bonus, 6 Weeks	4 High Bonus, 12 Weeks	5 Declining Bonus, 12 Weeks	6 High Bonus, 12 Weeks, No Workshop	Total
1. Percent Who Stopped Receiving UI within Bonus Qualification Period	29.8	38.8	31.4	40.0	38.0	41.4	36.7
2. Percent Who Satisfied Condition 1 Who Did Not File a Notice of Hire	19.8	27.1	22.0	25.6	27.0	24.7	24.7
3. Percent Who Satisfied Conditions 1 and 2 Who Started a Job within the Qualification Period	12.6	21.9	15.4	20.6	20.4	17.4	18.6
4. Percent Who Satisfied Conditions 1, 2, and 3 Who Were Not Recalled	6.5	11.4	6.8	9.0	10.5	6.9	8.9
5. Percent Who Satisfied Conditions 1, 2, 3, and 4 Who Worked Full Time	4.2	8.2	5.1	6.9	8.2	4.7	6.5
6. Percent Who Satisfied Conditions 1, 2, 3, 4, and 5 Who Worked 16 Consecutive Weeks	4.2	7.9	4.0	5.8	7.3	4.0	5.8

about 37 percent of the claimants stopped collecting UI within the bonus qualification period.[8] About one-third of these claimants filed a Notice of Hire, leaving a pool of 25 percent of the total who might have been eligible for a bonus that they did not collect.

To investigate this possibility, we sequentially applied the bonus eligibility conditions based on data from the follow-up interview. One eligibility condition that the lack of data prevented us from applying was that the individual remain eligible for UI. With this caveat in mind, we found that about three-quarters of these individuals did not appear to be eligible for a bonus, largely because they were recalled by the prior employer. This percentage is identical to the percentage of claimants in the interview who said that they were employed within the qualification period, but who did not file a claim because they thought that they were ineligible for the bonus.

Thus, a maximum of about 6 percent of the claimants might have been eligible for a bonus for which they did not apply.[9] If this estimate is accurate, the bonus receipt rate could have been as high as 17 percent, rather than the 11 percent that we found. However, since ineligibility for UI was one of the primary reasons that claims were denied, and since some individuals would not file in an ongoing program, it is likely that the rate of bonus receipt would fall between the 11 percent observed in the demonstration and our estimate of 17 percent.

[8]Individuals who did not collect any benefits are included in this category, as are individuals who did not receive a bonus offer but who stopped collecting benefits within the qualification period.

[9]This rate ranged from 4 percent in several of the treatments to 8 percent.

VII. IMPACTS ON UI RECEIPT

The demonstration treatments were expected to reduce UI benefit receipt among eligible claimants by inducing them to find reemployment quickly. Our analysis of the impacts on UI outcomes tested whether the treatments reduced the level of UI receipt and, if so, the extent of the reduction. Our general approach for measuring impacts on UI receipt was to compare UI receipt among claimants who were assigned to one of the bonus treatments with UI receipt among claimants who were assigned to the control group. Our estimates based on these comparisons demonstrate that most of the bonus offers that were tested reduced UI receipt significantly over the benefit year. The most generous bonus offer--the highest bonus amount for the longest qualification period--had the greatest impact on benefits, reducing average UI receipt by about 0.8 weeks, or $130. In addition, this most generous bonus was the only one that significantly reduced the proportion of claimants who exhausted their benefits. The more limited bonus offers--a smaller bonus amount, a shorter qualification period, or a bonus that declined over time--reduced UI receipt by an average of about a half a week, or $80, per claimant.

We used two approaches to extend our evaluation of the net impacts of the demonstration on UI receipt. First, we examined how the bonus offers affected the rate at which claimants exited from UI at various points in time. As expected, the bonus offers significantly increased the rate of exit from UI during the bonus qualification periods. We also found evidence that most of the bonus offers reduced UI receipt more among relatively short-term claimants than among longer-term claimants, who were likely to exhaust their UI benefits. Second, we estimated impacts for subgroups of claimants to determine the types of claimants who were affected to the greatest extent by the bonus offers. Our findings show that the estimated impacts did not vary significantly among most subgroups. However, impacts were significantly larger among claimants from manufacturing industries than among claimants from nonmanufacturing industries.

Our analysis is presented in four sections. In Section A, we discuss our estimation methodology. In Section B, we present our estimates of the basic treatment impacts on UI receipt. In Sections C and D, we present our estimated impacts on UI exit rates and on UI receipt by subgroup.

A. METHODOLOGY

We used four measures of UI benefit receipt for our primary analysis of the impact of the treatments on UI receipt: (1) the number of weeks for which each claimant was paid benefits in the benefit year, (2) the dollar amount of UI benefits paid to claimants in the benefit year, (3) whether claimants exhausted their benefits, and (4) the number of weeks in the initial UI spell. We considered both the total weeks paid in the benefit year and the weeks in the initial UI spell, to distinguish between impacts which led to temporary withdrawals from UI and impacts which had longer-term consequences for insured unemployment. We also used the initial UI spell to investigate the timing of the treatment impacts, as discussed in Section C of this chapter.

Our regression model for estimating the impacts of the bonus offers on UI receipt contained binary indicators for the five treatments. Table VII.1 describes each of the treatments that were tested and specifies the number of claimants in the analysis sample who were assigned to each treatment. We chose to pool the claimants assigned to treatments 4 and 6 into a single treatment, which we refer to as treatment 4. We combined these groups because so few claimants participated in the job-search workshop that there was effectively no difference between treatment 4, in which a large bonus offer for a long qualification period was combined with the offer of a job-search workshop, and treatment 6, in which the same bonus offer and qualification period were not attached to a job-search workshop offer.[1] Hence, we view these two treatments as identical in estimating the impacts on UI receipt.

[1]See Chapter V for a discussion of participation in the job-search workshop.

TABLE VII.1

BONUS PLANS AND SAMPLE ALLOCATION

Treatment	Bonus Amount	Qualification Period	Number of Participants in Analysis Sample[a]
0 (control group)	0	0	3,392
1	Low (3 x WBA)	Short (6 weeks)	1,395
2	Low (3 x WBA)	Long (12 weeks)	2,456
3	High (6 x WBA)	Short (6 weeks)	1,910
4 [b]	High (6 x WBA)	Long (12 weeks)	3,073
5	Initially High (6 x WBA) but Declining	Long (12 weeks)	1,860
Total			14,086

[a]The analysis sample includes claimants who filed a benefit claim within at least 6 weeks after their benefit application date.

[b]Treatment 4 in this table includes both claimants assigned to treatment 4 and claimants assigned to treatment 6. The two treatments differed only in that treatment 4 included the offer of the job-search workshop, while treatment 6 did not. However, only an extremely small number of claimants attended the job-search workshop. Hence, the two treatments were treated as identical for the purpose of estimating impacts.

A more sophisticated impact analysis was based on the parameters of the bonus offers. Since the bonus offers varied primarily along two dimensions--the amount of the bonus offer and the length of the qualification period--we can analyze the effect of varying the bonus amount and the duration of the bonus offer on UI receipt. We carried out this analysis by replacing the treatment group indicators in our regressions with a set of explanatory variables that controlled for the principal components by which the bonus offers varied--short or long duration of the offer, low or high bonus amount, and whether the amount of the offer declined over time. The models that we used to derive these estimates implicitly imposed restrictions on the impact estimates that were not imposed by the treatment-based models, and we tested these restrictions as part of the analysis.

The regressions also contained variables to control for the individual characteristics of claimants, the timing of sample selection, and the UI office to which claimants reported. We used regression-adjusted estimates to control for these factors, for two reasons.

First and most important, we used the regressions to control for the timing of sample selection because the proportion of claimants assigned to different treatment groups varied over time (see Chapter III). The distributions of claimant assignments over time reflect a revised sample design strategy in which relatively fewer claimants were allocated to treatment 4 and relatively more claimants to treatment 1 beginning in the third quarter of 1989.[2] Hence, claimants were not assigned randomly to the treatments according to the timing of sample selection. If the effectiveness of the treatments also varied over time, the variation in the allocation of the sample would affect simple treatment-control differences, biasing simple estimates of the impacts of the treatments.

The regression-adjusted estimates also allowed us to control for any existing between-group variation and within-group variation in the characteristics of claimants in measures of the impacts of the treatments. Despite random assignment, the average characteristics of claimants differed among the treatment groups, as shown in Table III.7 in Chapter III. While these differences were largely

[2]The reasons for and the details of the changes in sample allocation are discussed in Chapter III. Table III.1 shows these distributions.

statistically insignificant and thus consistent with random assignment, regressions that control for these differences improve the accuracy of our impact estimates. The regressions also control for within-group variation in the characteristics of claimants. These adjustments allowed us to derive more statistically precise estimates of the impacts of the treatments.

B. ESTIMATED IMPACTS ON UI RECEIPT

We present two types of impact estimates in this section. First, we present the impact of the individual treatments on UI receipt. Based on this analysis, we also test whether different treatments had significantly different impacts on UI receipt and whether we can combine the treatments in a way that helps us interpret the impact estimates. Second, we measure how changes in the parameters of the bonus offers affected the estimated impact on UI receipt.

1. Impacts on UI Receipt by Treatment Group

The regression-adjusted impact estimates presented in Table VII.2 demonstrate that the treatments generally reduced UI benefit receipt among claimants. All of the treatments reduced both weeks and dollars of benefits significantly (at the 90 percent confidence level), with the exception of the declining bonus offer, which had a negative estimated impact on benefits that was not significant.[3] Treatment 4, the most generous bonus offer, had the greatest impact on UI benefits, reducing UI receipt by about 0.8 weeks during the benefit year, or by $130 in paid benefits. These impacts represent over 5 percent of the total UI benefits received by control group members, and both were statistically significant at the 99 percent confidence level. Treatment 4 also reduced the proportion of claimants who exhausted their benefits during the benefit year, by nearly 1.5 percentage points.

[3]As specified in Table VIII.2, we used one-tail tests of the significance of the treatment impacts. We generally used one-tail tests for the estimates presented in this chapter when we expected that an estimated coefficient would have a particular sign, and we used two-tail tests if we were uncertain about the expected sign. In each table, the footnotes specify the hypotheses that were tested.

TABLE VII.2

ESTIMATED IMPACTS OF THE TREATMENTS ON UI OUTCOMES
(Standard Errors in Parentheses)

Treatment	Impacts on UI Outcomes			
	Weeks of Benefits Received in Benefit Year	Dollars of Benefits Received in Benefit Year	Rate of Benefit Exhaustion (Percent)	Weeks of Benefits Received in Initial UI Spell
1 - Low bonus, short qualification period	-.65 **	-103 **	0.0	-.21
	(.34)	(58)	(1.4)	(.33)
2 - Low bonus, long qualification period	-.36 *	-69 *	0.1	-.55 **
	(.28)	(48)	(1.2)	(.27)
3 - High bonus, short qualification period	-.44 *	-99 **	0.0	-.37
	(.30)	(52)	(1.3)	(.29)
4 - High bonus, long qualification period	-.82 ***	-130 ***	-1.4 *	-.68 **
	(.26)	(46)	(1.1)	(.26)
5 - Initially high but declining bonus, long qualification period	-.33	-61	1.3	-.08
	(.30)	(53)	(1.3)	(.30)
Control Group Mean	14.94	2,387	27.7	12.51

NOTE: The sample includes 13,913 observations for whom we have data on both demographic characteristics and UI receipt. The explanatory variables contained in the regressions include treatment indicators, cohort indicators, office indicators, and demographic and economic variables. The full regression estimates are contained in Table E.1 of Appendix E.

*Significantly less than zero at the 90 percent confidence level in a one-tail test.
**Significantly less than zero at the 95 percent confidence level in a one-tail test.
***Significantly less than zero at the 99 percent confidence level in a one-tail test.

The other four bonus offers, which were more limited than the bonus offer in treatment 4, had smaller impacts on UI receipt. The estimated reductions in weeks of UI receipt generated by these four bonus offers ranged from a third of a week for treatment 5 (the declining bonus amount) to almost two-thirds of a week for treatment 1 (low amount, short duration). The finding for treatment 1 is anomalous because it suggests that the least generous of the five bonus offers had the second largest impact on UI receipt. When measured in dollars of benefits, the impacts of the four limited bonus offers were again less than the impact of treatment 4, ranging from $61 for treatment 5 (a declining bonus) to $103 for treatment 1 (low amount, short duration). The four limited bonus offers had no significant impact on the exhaustion of UI benefits.

The final column of numbers in Table VII.2 shows that four of the five bonus offers had a smaller impact on weeks of benefits in the initial UI spell than on weeks of benefits for the full benefit year.[4] The estimated impacts of two of the treatments, 1 and 5, were much smaller for the initial spell than for the benefit year. One result of these differences is that the anomaly that treatment 1 had the second largest impact over the benefit year did not occur for the initial spell--treatment 1 had only the fourth largest impact on the initial UI spell. Only treatment 2 (low amount, long duration) had a greater impact for the initial spell than for the benefit year. Overall, the greatest impacts on weeks of benefits in the initial spell occurred for the constant amount, long-duration bonus offers (treatments 2 and 4).

The differences between the impacts on weeks in the initial spell and the impacts on weeks in the benefit year are difficult to evaluate. One possibility is that the treatments induced claimants to take more stable jobs, reducing the probability that the treatment group members received additional UI benefits later in the benefit year. This effect would make the impacts for the benefit year greater than the impacts for the initial spell, as was the case with four of the bonus offers, because treatment-group members who have taken more stable jobs are less likely to experience a second UI spell later

[4]Claimants were treated as ending their initial UI spell at the point that they stopped receiving full payments for at least three consecutive weeks.

in the benefit year. At the very least, the differences between the two sets of estimates suggest that treatment-group members did not take temporary jobs in order to hasten reemployment and thus qualify for a reemployment bonus.

While most of the regression-adjusted impacts for individual treatments presented in Table VII.2 were significantly less than zero, they did not differ significantly from each other. In hypothesis tests of the equality of each pair of impacts on weeks of UI receipt in the benefit year, we failed to reject the hypothesis that the impacts were equal to each other. However, given the available sample sizes, we may simply not have the statistical power necessary for detecting differences between the treatment impacts.

Using an alternative grouping of treatments, we found that the impact of treatment 4--the most generous bonus offer--on weeks of UI receipt was significantly greater than the average impact of the other four bonus offers. We derived this result by combining the four limited bonus offers into a single group, which increased the statistical power of the impact comparison. The estimates demonstrate that the impact of treatment 4, which reduced average UI receipt by .82 weeks, was nearly double the impact of the four more limited bonus offers combined, which reduced average UI receipt by .42 weeks. The difference between the two estimates was statistically significant at the 90 percent confidence level. This finding suggests that, on average, limiting the bonus offer by shortening the qualification period, by reducing the bonus amount, or by specifying a declining bonus significantly lessened the impact of the bonus offer on UI receipt.

2. Impacts of the Bonus Parameters on UI Receipt

The design of the Pennsylvania demonstration provides an opportunity to estimate the impacts of the parameters of the bonus offers on UI receipt. Using an alternative parameterization of the treatments, we can directly analyze the effect of varying the bonus amount (the price effect) and the duration of the bonus offer (the duration effect). We base our estimates of these effects on a model that contains four binary variables that define the treatment groups: one that indicates whether or

not the individual received any bonus offer, and three variables that indicate whether or not the amount of the offer was limited, whether the duration of the offer was limited, or whether the amount of the offer declined over time.

The estimates in model (1) of Table VII.3 suggest that offering the most generous reemployment bonus reduced weeks of UI receipt significantly, while limiting the parameters of the offer along any single dimension lessened the impact of the offer on UI receipt, but not significantly. We can interpret the estimated effects of limiting the bonus amount and duration as the price and duration effects, respectively. Both the price effect and the duration effect were small, and neither effect was significantly greater than zero at the 90 percent confidence level. The estimated effect of limiting bonus payments through a declining benefit schedule was to lessen the bonus impact by slightly over a third of a week, but again the estimated coefficient was not significantly greater than zero.

Although model (1) provides more direct estimates of the price and duration effects than does the basic treatment-based model of the previous sections, it is also more restrictive. Model (1) implicitly assumes that the price effects were the same for both the short- and the long-duration offers, and that the duration effects were the same for both values of the bonus amount. But the treatment-based impacts presented previously in Table VII.2 imply that these restrictions may not be valid. For example, the estimated impacts presented in Table VII.2 imply that decreasing the bonus amount *reduced* UI receipt for the long-duration offer by about half a week, but decreasing the bonus amount *increased* UI receipt for the short-duration offer by about a fifth of a week.[5] Thus, the two implied price effects were quite different and did not even have the same sign.[6] Despite this difference between the two price effects, we were not able to reject the hypothesis that the two

[5]The price effect for the long-duration offer is calculated as the impact of treatment 4 minus the impact of treatment 2. The price effect for the short-duration offer is calculated as the impact of treatment 3 minus the impact of treatment 1.

[6]The two estimates of the price effect had opposite signs due to the relatively large impact that was estimated for treatment 1, the least generous bonus offer.

TABLE VII.3

ESTIMATED IMPACTS OF THE BONUS PARAMETERS
ON WEEKS OF UI BENEFITS
(Standard Errors in Parentheses)

	Model	
	(1)	(2)
Binary Indicators		
Received a Bonus Offer	-.71 ***	
	(.25)	
Amount of Bonus Offer Was Limited	.21	
	(.23)	
Duration of Bonus Offer Was Limited	.09	
	(.23)	
Amount of Bonus Declined Over Time	.38	.38 *
	(.30)	(.28)
Continuous Variables		
Amount of Bonus Offer (thousands of dollars)		-.33
		(.29)
Duration of Bonus Offer (weeks)		-.029
		(.026)

NOTE: Estimates are regression-adjusted. The sample includes 13,913 observations for whom we have data on both demographic characteristics and UI receipt. The explanatory variables contained in the regressions include treatment indicators, cohort indicators, office indicators, and demographic and economic variables. The full regression estimates are contained in Table E.2 of Appendix E. We also estimated both models (1) and (2) using two-limit Tobit procedures to control for the grouping of observations at zero weeks of benefits and at 26 weeks of benefits. The Tobit estimates were similar to the least squares estimates shown in this table, so we did not present them.

*Significantly less or greater than zero at the 90 percent confidence level in a one-tail test.
**Significantly less or greater than zero at the 95 percent confidence level in a one-tail test.
***Significantly less or greater than zero at the 99 percent confidence level in a one-tail test.

effects were equal.[7] Hence, our statistical tests did not deny the validity of the restrictions placed on the price and duration effects by model (1), but we believe that estimates based on this parameterization should be evaluated carefully in light of the difference between the two implied price effects.

When we used continuous variables to control directly for the amount and duration of the bonus offers, both continuous variables had a negative impact on benefit weeks, as shown in model (2) of Table VII.3. Although the individual coefficients on the amount and duration of the bonus offers were not statistically significant, the two coefficients were jointly significant at the 95 percent confidence level. The estimates imply that, other things being equal, a thousand-dollar increase in the amount of the bonus offer reduced average benefit receipt by about a third of a week. This impact was similar to the implied impact of extending the duration of the offer by 10 weeks, which reduced benefit receipt by somewhat less than a third of a week. Making the bonus amount declining rather than constant lessened the impact of the most generous bonus on average benefit receipt by more than a third of a week.[8] This impact was statistically significant at the 90 percent confidence level.

C. THE TIMING OF THE IMPACTS ON UI RECEIPT

In this section, we consider the impact of the bonus offers on the rate at which claimants stopped receiving UI benefits. Figure VII.1 shows the *conditional* UI exit rate (or "hazard rate") for each

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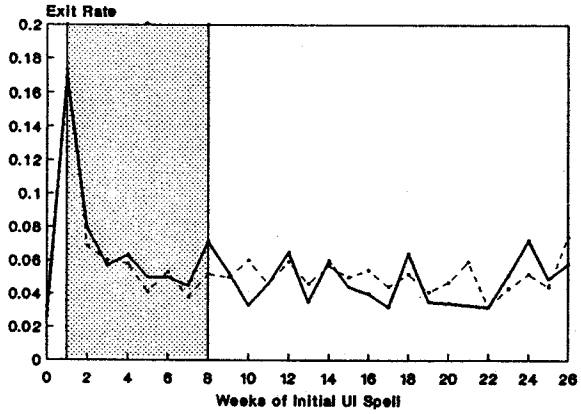
[7]We tested for the equality of price effects by specifying the null hypothesis that the impact of treatment 4 minus the impact of treatment 2 is equal to the impact of treatment 3 minus the impact of treatment 1. We were not able to reject the hypothesis that the two effects were equal at the 90 percent level of confidence. It can be shown that given the design of the bonus offers the test for the equality of the price effects is equivalent to a test of the equality of the duration effects. Hence, the equality of the price effects and the equality of the duration effects represent only a single restriction on the estimates of the treatment impacts.

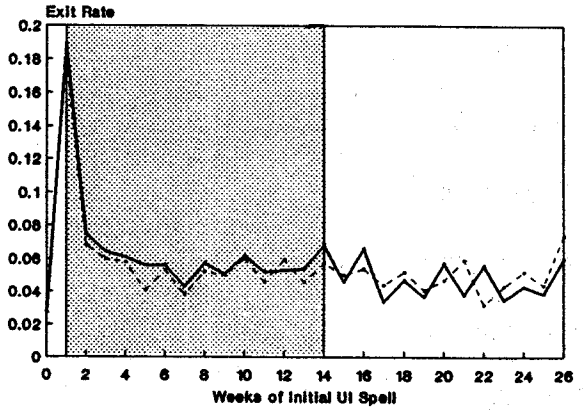
[8]We also tested for the interaction between the bonus amount and the bonus duration, but the coefficient on the interaction term was small and not statistically significant.

FIGURE VII.1
CONDITIONAL UI EXIT RATES BY TREATMENT GROUP AND CONTROL GROUP

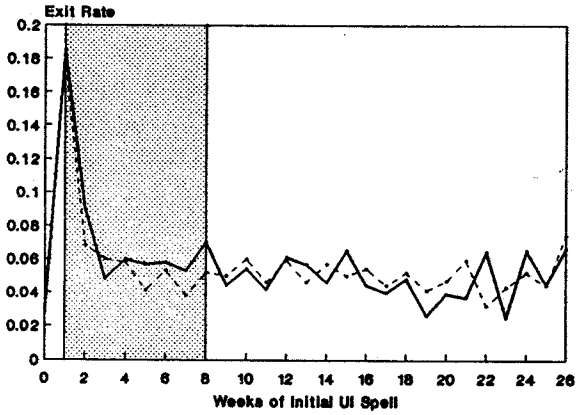
TREATMENT 1 AND CONTROL GROUP



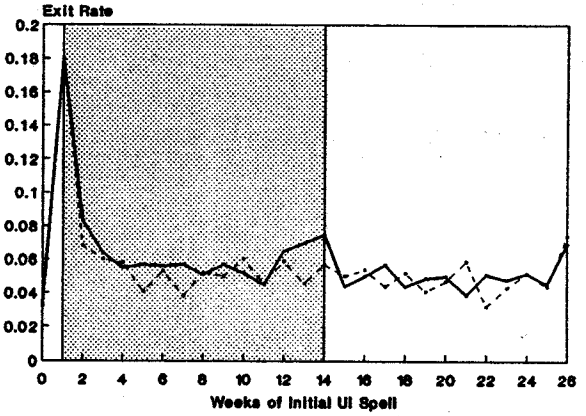
TREATMENT 2 AND CONTROL GROUP



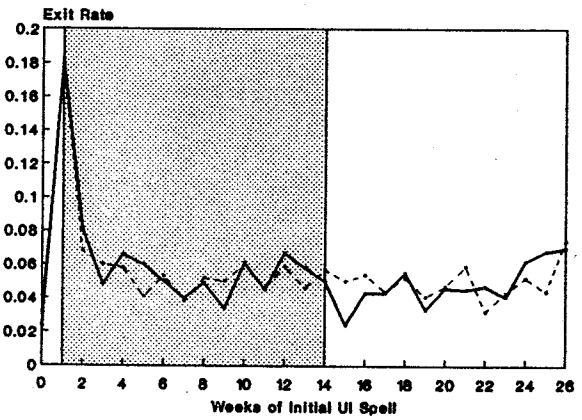
TREATMENT 3 AND CONTROL GROUP



TREATMENT 4 AND CONTROL GROUP



TREATMENT 5 AND CONTROL GROUP



TREATMENT ——————

CONTROL - - - - - - - - - - - - -

NOTE: The shaded areas indicate the bonus qualification period for each treatment.

week in the initial UI spell among the treatment and control groups.[9] The conditional UI exit rate for each week is equal to the number of claimants who ended their initial UI spell during the week as a percentage of the claimants who were still receiving UI at the beginning of the week.[10] The exit rates were relatively high in the first few weeks of the initial spell, and then settled down to around 5 percent.

Table VII.4 groups the conditional UI exit rates for the control group over four larger blocks of time. We chose these blocks of time in order to focus on the exit rates during the bonus qualification periods. The first row of the table corresponds to the bonus qualification period for the short-duration bonus offers and the first half of the qualification period for the long-duration offers, and the second row corresponds to the second half of the qualification period for the long-duration offers. The findings presented in Table VII.4 confirm that the exit rate was high in the weeks just after the claim date--over 45 percent of the original claimants exited UI in the first eight weeks after the benefit application date. In the subsequent six-week periods, the exit rate remained steady at just over 25 percent of the claimants who entered each period.

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[9]Claimants were treated as having exited UI if they stopped receiving full payments for at least three consecutive weeks. Because we treated the waiting week as the first week in the initial UI spell, most claimants had a potential initial UI spell of 27 weeks--the waiting week plus 26 compensable weeks. Most claimants also had an initial spell of at least one week, because individuals were eligible for the demonstration only if they served a waiting week. The only claimants whose initial spell was zero weeks were those who received at least three weeks of partial payments from the beginning of their UI spell.

[10]In our calculation of the conditional exit rate, we treated claimants who exited UI because they exhausted their benefits the same as we did claimants who voluntarily exited UI. We believe that this approach is valid because we analyze exit rates only for the first 26 weeks of the initial UI spell, and 99 percent of the claimants had 26 weeks of potential benefits and would not have exhausted their benefits during this period. The remaining one percent had 16 weeks of potential benefits, and they could have exhausted benefits at 17 weeks into the initial spell. Since these claimants represented such a small portion of our sample, controlling for the exhaustion of benefits among these claimants would not have had a noticeable impact on our findings.

The conditional exit rate is equivalent to what is often referred to as a "hazard rate." Kiefer (1988) discusses the application of hazard rates in economic research.

We also estimated the impact of each treatment on conditional exit rates based on treatment-control differences for each block of time designated in Table VII.4. For each period, the base sample for calculating the exit rates was defined as individuals who were still receiving benefits at the beginning of the time period. We interpret the differences in exit rates between the treatment groups and the control group as the effect of the treatment on the probability of exiting UI during the period among claimants still receiving benefits at the beginning of the period.[11]

Table VII.4 demonstrates that the largest impacts of the treatments on UI exit rates generally occurred during the first eight weeks of the initial UI spell, when all treatment-group members were eligible for a reemployment bonus (as indicated by the shaded area in the table). All treatments had a positive impact on the exit rate in this early period; three of those impacts were significantly greater than zero at the 95 percent level of confidence. The largest impacts occurred for treatments 3 and 4, the two treatments that offered the highest bonus amount. Both treatments increased the exit rate by 3 percentage points or more.

Although few of the estimated impacts in subsequent periods were statistically significant, the pattern of the estimates is generally consistent with the design of the treatments. For example, in the period from week 9 to week 14, the two treatments whose impacts were positive were treatments in which claimants could still qualify for a reemployment bonus. Treatment 4, which offered the highest bonus during this period, had the largest impact, increasing the exit rate by 3.4 percentage points. Another long-duration offer, treatment 2, increased the exit rate during week 9 to week 14

[11]We used treatment-control differences rather than regression-based estimates to analyze conditional exit rates because we wanted to measure the total impact of the bonus offers on the exit rates. As claimants exited UI over time, the characteristics of the sample of claimants who were still receiving UI changed. Because the treatments affected exit behavior, they also implicitly affected the characteristics of the claimants still receiving UI at any point in time. Hence, at the beginning of each period, any differences in the characteristics of treatment and control groups were caused largely by the impact of the treatment on the types of claimants who exited UI in previous periods. To capture the full impact of the treatment on the current exit rate, we did not adjust for these differences in characteristics that were caused by the treatment. In addition, when we derived alternative estimates based on controlling for these differences, it did not affect our general findings about exit rates.

TABLE VII.4

ESTIMATED IMPACTS OF THE TREATMENTS ON CONDITIONAL UI EXIT RATES
BY LENGTH OF INITIAL UI SPELL
(Standard Errors in Parentheses)

Length of Initial UI Spell, in Weeks	Conditional UI Exit Rate for Control Group Members (Percent)	Treatment Impacts on Exit Rate (Percent)				
		1 Low Bonus, Short Qualification Period	2 Low Bonus, Long Qualification Period	3 High Bonus, Short Qualification Period	4 High Bonus, Long Qualification Period	5 Declining Bonus, Long Qualification Period
0-8	45.6	1.8 (1.6)	3.0 ** (1.3)	3.9 *** (1.4)	3.1 ** (1.2)	1.1 (1.5)
9-14	27.9	-2.2 (2.0)	1.8 (1.7)	-1.1 (1.8)	3.4 ** (1.6)	-0.4 (1.8)
15-20	25.6	-3.1 (2.2)	0.1 (1.9)	-1.8 (2.0)	0.5 (1.8)	-3.4 * (2.0)
21-26	26.7	-0.7 (2.5)	-2.4 (2.2)	-0.1 (2.4)	0.3 (2.1)	2.6 (2.4)

NOTE: These impacts are based on the difference in UI exit rates between the treatment group and the control group for each time period, based on the sample of claimants still receiving benefits at the beginning of the time period. The shaded areas indicate which impacts occurred during periods in which claimants were eligible to receive a reemployment bonus.

*Statistically significant at the 90 percent confidence level in a two-tail test.
**Statistically significant at the 95 percent confidence level in a two-tail test.
***Statistically significant at the 99 percent confidence level in a two-tail test.

by almost 2 percentage points. The declining bonus offer, treatment 5, appears to have had no impact on the exit rate during week 9 to week 14, a period in which treatment-group members qualified for a lower bonus amount.

Figure VII.2 shows that the simple estimated impacts on weekly conditional UI exit rates that are implied by Figure VII.1 are also consistent with the design of the bonus offers. These impacts are based on simple differences between the weekly UI exit rates for the treatment and control groups. Figure VII.2 demonstrates that the bonus offers had a generally positive impact on exit rates during the qualification period. In addition, the impacts tended to be relatively high either right before or right at the end of the qualification period, suggesting that many treatment-group members increased their job-search efforts at the end of the qualification period in an effort to qualify for the bonus just before their eligibility expired.

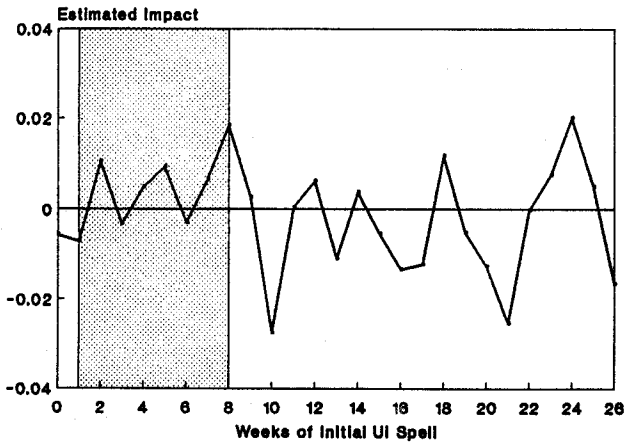
After the end of the bonus qualification periods for all treatments, the estimated impacts on the conditional UI exit rates were negative in many cases, as shown in Table VII.4. Although most of these impacts were not statistically significant, the negative signs on the estimates suggest that to some extent the *cumulative* exit rate of the control group "caught up" to the cumulative exit rate of the treatment groups following the respective qualification periods.

We can consider this "catching up" phenomenon by examining the cumulative exit rates for the treatment and control groups directly. The cumulative exit rates are shown in Figure VII.3. The cumulative exit rate rises quickly in the early weeks of the initial UI spell, and then increases at a slower rate in later weeks. Nearly 80 percent of the claimants had exited UI by week 26.

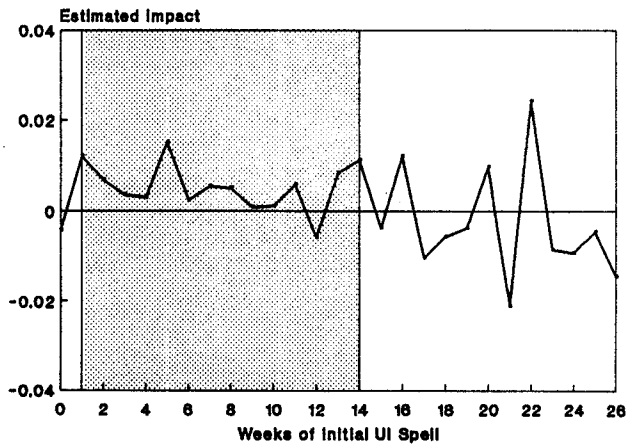
The estimates presented in Table VII.5 and Figure VII.4 confirm that the impacts of the treatments on cumulative exit rates tended to decay after the end of the bonus qualification period. For example, Table VII.5 shows that, although treatment 3 had a significant impact on the proportion of claimants who exited UI in the first 8 weeks of their initial benefit spell, it did not increase the proportion of claimants who exited by the end of week 26. The estimates presented in Figure VII.4,

SIMPLE TREATMENT IMPACTS ON CONDITIONAL UI EXIT RATES

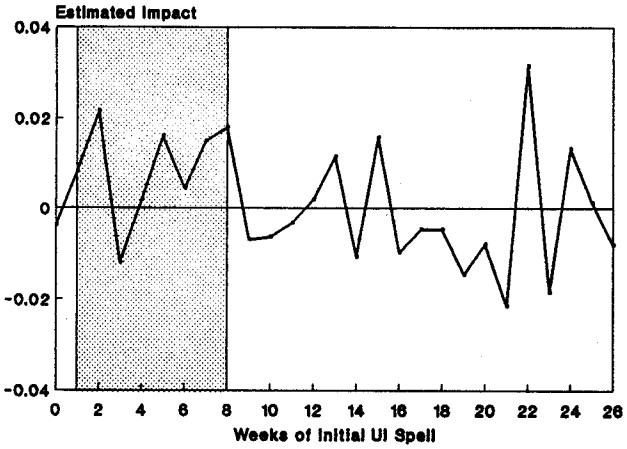
TREATMENT 1



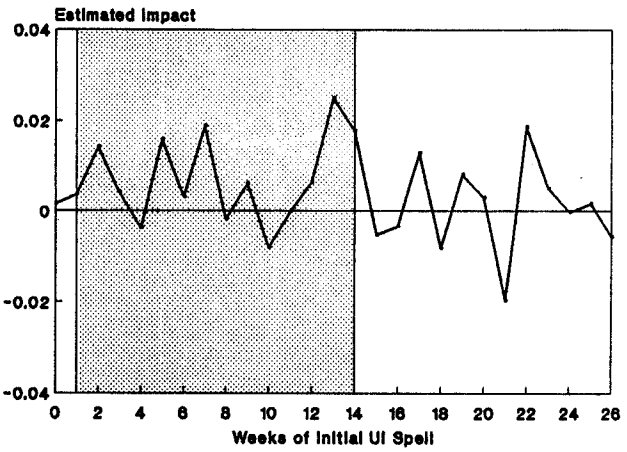
TREATMENT 2



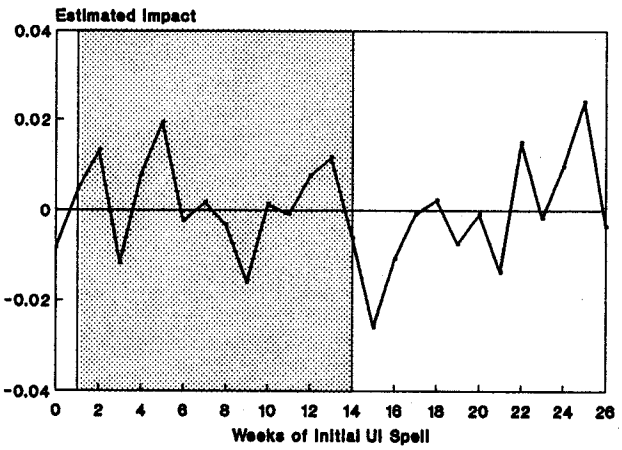
TREATMENT 3



TREATMENT 4



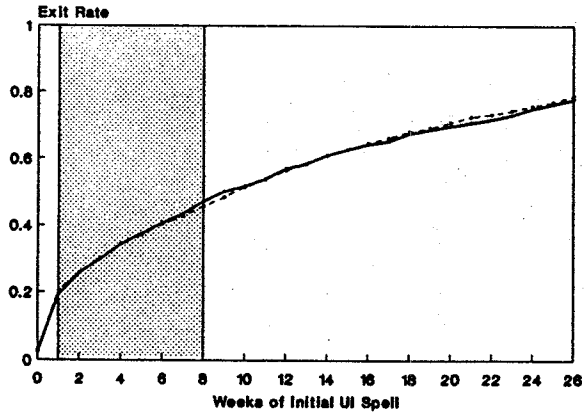
TREATMENT 5



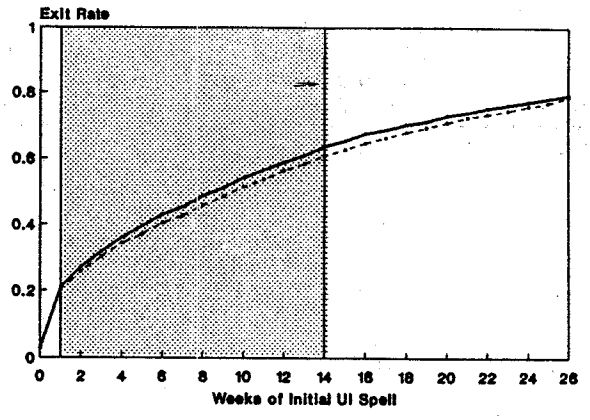
NOTE: The estimated impact is equal to the difference between the conditional exit rate for the treatment group and the conditional exit rate for the control group. The shaded areas indicate the bonus qualification period for each treatment.

FIGURE VII.3
CUMULATIVE UI EXIT RATES BY TREATMENT GROUP AND CONTROL GROUP

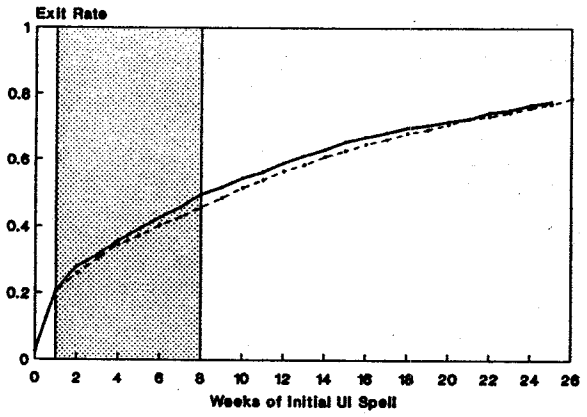
TREATMENT 1 AND CONTROL GROUP



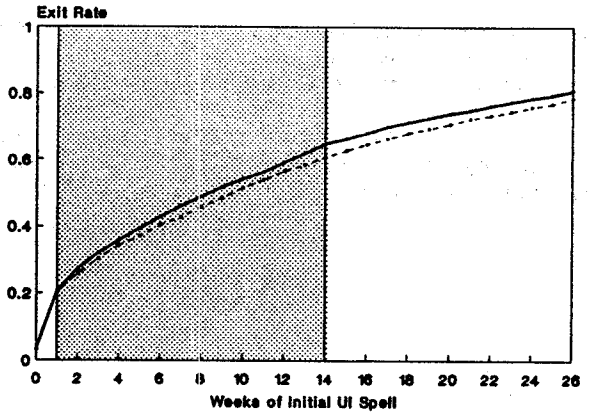
TREATMENT 2 AND CONTROL GROUP



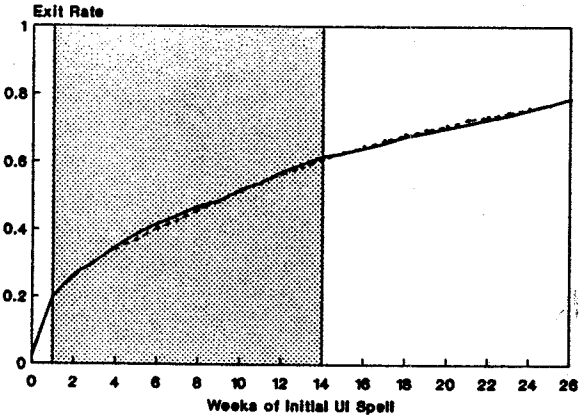
TREATMENT 3 AND CONTROL GROUP



TREATMENT 4 AND CONTROL GROUP



TREATMENT 5 AND CONTROL GROUP



TREATMENT ——————

CONTROL - - - - - - - - - - - - - -

NOTE: The shaded areas indicate the bonus qualification period for each treatment.

TABLE VII.5

ESTIMATED IMPACTS OF THE TREATMENTS ON CUMULATIVE UI EXIT RATES AT POINTS IN THE INITIAL UI SPELL
(Standard Errors in Parentheses)

Week	Proportion of Control Group Members Who Have Exited UI (Percent)	Treatment Impacts on Exit Rate (Percent)				
		1 Low Bonus, Short Qualification Period	2 Low Bonus, Long Qualification Period	3 High Bonus, Short Qualification Period	4 High Bonus, Long Qualification Period	5 Declining Bonus, Long Qualification Period
8	45.6	2.2 * (1.6)	2.6 ** (1.3)	3.7 *** (1.4)	3.6 *** (1.2)	1.2 (1.4)
14	60.8	1.2 (1.5)	2.7 ** (1.3)	2.2 * (1.4)	3.7 *** (1.2)	1.0 (1.4)
20	70.8	0.2 (1.4)	2.1 ** (1.2)	0.9 (1.3)	2.2 ** (1.1)	-0.2 (1.3)
26	78.7	0.1 (1.3)	0.8 (1.1)	0.6 (1.1)	1.4 * (1.0)	0.7 (1.2)

NOTE: These impacts are based on linear probability models of the cumulative exit rates. The explanatory variables used in the model include treatment indicators, cohort indicators, site indicators, and demographic and economic variables. The full regression estimates are contained in Table E.3 of Appendix E.

* Significantly greater than zero at the 90 percent confidence level in a one-tail test.
** Significantly greater than zero at the 95 percent confidence level in a one-tail test.
*** Significantly greater than zero at the 99 percent confidence level in a one-tail test.

SIMPLE TREATMENT IMPACTS ON CUMULATIVE UI EXIT RATES

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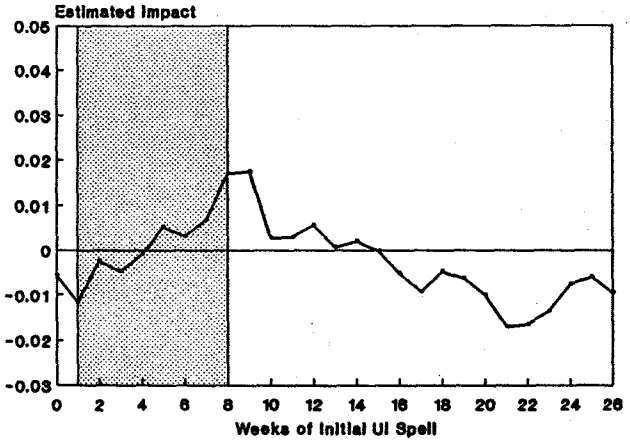
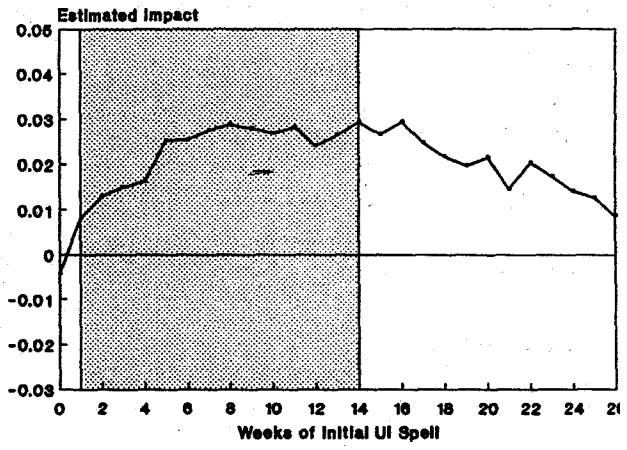
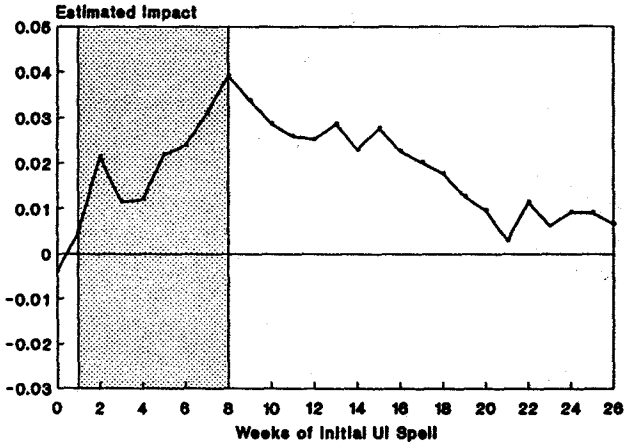


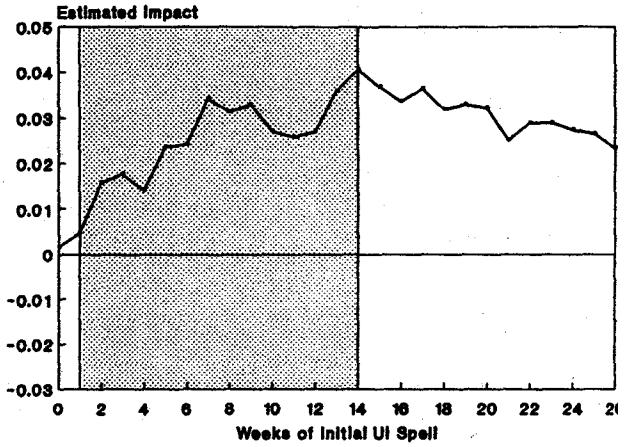
TREATMENT 2



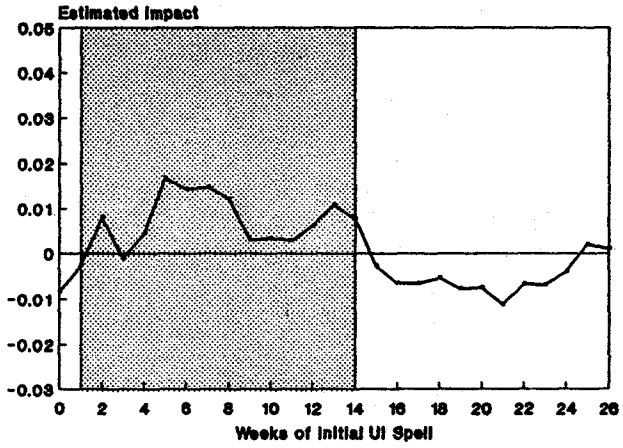
TREATMENT 3



TREATMENT 4



TREATMENT 5



NOTE: The estimated impact is equal to the difference between the cumulative exit rat for the treatment group and the cumulative exit rate for the control group. The shaded areas indicate the bonus qualification period for each period.

which are based on simple differences in the exit rates shown in Figure VII.3, show that the impact of treatment 3 reached a maximum at the end of the bonus qualification period (week 8). After the end of the qualification period, the estimated impact on the cumulative exit rate decayed over time as the exit rate for the control group began to catch up to the exit rate for the treatment group. At the end of the 26 weeks, the cumulative exit rates for the two groups were nearly identical. The impacts of most of the other treatments on the cumulative UI exit rate also decayed over time.[12]

The decay of the impacts of the treatments on the cumulative UI exit rate does not imply that the treatments had no impact on UI receipt. Rather, it demonstrates that much of the reduction in UI receipt caused by the treatments occurred because the treatment effect was concentrated among claimants who faced relatively short potential UI spells otherwise. The treatments do not appear to have had much of an impact on the UI experience of long-term unemployed claimants who were likely to exhaust their benefits in their initial UI spell.[13] This finding is consistent with the estimated treatment impacts on benefit exhaustion presented in Table VII.2, which demonstrated that most of the bonus offers did not affect the proportion of claimants who exhausted their benefits during the benefit year.

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[12]To some extent, we expected that the impacts would decay after the end of the qualification period. Bonus offers have an impact on UI receipt because they induce claimants to exit UI sooner than they would otherwise. The decay of the impact on the cumulative exit rate is determined by the type of claimants who reduce UI receipt in response to the bonus offer. For example, assume that treatment 1, whose qualification period ends in week 8, reduces the initial spell only for claimants who would have collected benefits for 9 weeks in the absence of the bonus offer. In this case, the cumulative exit rate would increase during the qualification period as these claimants exit UI sooner in response to the bonus offer. However, the cumulative exit rate after 9 weeks would not change, because, by assumption, the bonus offer has no impact on the behavior of claimants who collect benefits for more than 9 weeks. The impact of the bonus offer on the cumulative exit rate thus decays to zero by week 9. In the general case, the rate at which the impact on the cumulative exit rate decays depends on which claimants respond to the bonus offer by reducing their UI receipt.

[13]Decker (1989a) presents a similar finding based on the reemployment bonus in the New Jersey UI Reemployment Demonstration. However, the findings from the Illinois Claimant Bonus Experiment demonstrate that the reemployment bonus in that intervention did reduce the length of the initial UI spell among the long-term unemployed.

Treatment 4 was the only treatment that appeared to increase the proportion of claimants who exited UI in the first 26 weeks of their initial benefit spell, as shown in Table VII.5. The treatment increased the cumulative exit rate at week 26 by 1.1 percentage points, an impact which was small yet significant at the 90 percent confidence level. This impact is also reflected in Figure VII.4, which shows that the impact of treatment 4 on the cumulative exit rates did not decay all the way to zero by week 26. Our findings for treatment 4 are consistent with the estimates of the impacts of the treatments on the exhaustion of benefits, which demonstrated that only treatment 4 reduced the proportion of claimants who exhausted their benefits, but that the reduction was relatively small.

D. IMPACTS ON UI RECEIPT BY SUBGROUP

We estimated the impacts for subgroups by extending the regression model to include interaction terms in the regression equation. These interaction terms were equal to the product of the treatment indicators and the other control variables that we predicted would affect the size of the impact. We used linear combinations of the appropriate estimated parameters from this regression to estimate impacts by subgroup and to test for differences between subgroups (for example, males versus females). The estimates can be interpreted as the treatment impact on a given subgroup assuming that the value of all other characteristics variables for the subgroup members are set to their sample mean. For example, the impact for females was calculated according to the assumption that the average characteristics of females are the same as those of the full sample of claimants.

The treatment impacts on weeks and dollars of benefits, which are presented in Tables VII.6 and VIII.7, did not vary widely across age, race, or gender subgroups. Almost none of these impacts was significantly different for one subgroup than for another. While the lack of statistically significant differences may not be surprising given that the demonstration sample was not designed to be large enough to detect differences between subgroup impacts, even the point estimates did not differ greatly between subgroups. For example, Table VII.7 shows that the estimated impacts on dollars of benefits for females were similar to the comparable impacts for males. Impacts on weeks of

TABLE VII.6

ESTIMATED IMPACTS OF THE TREATMENTS ON WEEKS OF UI BENEFITS IN THE BENEFIT YEAR, BY SUBGROUP

Subgroup	Treatment 1 Low Bonus, Short Qualification Period	2 Low Bonus, Long Qualification Period	3 High Bonus, Short Qualification Period	4 High Bonus, Long Qualification Period	5 Declining Bonus, Long Qualification Period	All Treatments Combined	Number of Claimants in Subgroup
Gender							
Female	-1.20**	-0.76*	-0.56	-0.98**	-0.55	-0.82**	5,595
Male	-0.28	0.01	-0.38	-0.66*	-0.16	-0.32	8,318
Age							
Ages 25-34	-0.63	-0.42	-0.98**	-1.04***	-0.57	-0.75***	7,556
Ages 35-54	-0.48	0.02	-0.25	-0.55	-0.04	-0.27	4,872
55 or Older	-1.32	-0.76	-1.59*#	-0.33	0.07	-0.15	1,485
Race							
White	-0.42	-0.20	-0.47	-0.92***	-0.25	-0.49**	11,704
Black	-1.87*	-1.36	-1.11	-0.47	-1.05	-1.06*	1,623
Hispanic	-1.28	-0.12	2.23#	0.73	-0.08	0.31	506
Recall Status							
Expect Recall	-0.07	0.21	-1.08	-0.41	-0.15	-0.31	1,512
Do Not Expect Recall	-0.72**	-0.36	-0.37	-0.84***	-0.34	-0.54**	12,401
Industry							
Nondurable Manufacturing	-2.23**#	-1.30#	-2.69***###	-1.70**	-0.42	-1.58**##	1,525
Durable Manufacturing	-1.95**#	-2.33***###	-2.29***###	-1.07	-1.26	-1.71***###	2,068
Nonmanufacturing	-0.09	0.23	0.39	-0.54	-0.01	-0.06	10,320
Office[a]							
Group 1 (low unemployment rate, short average UI duration)	-0.67	-0.29	-0.04	-0.98*	0.53	-0.36	3,693
Group 2 (high unemployment rate, long average UI duration)	-0.26	0.16	0.37	0.59	-1.30	-0.01	1,040
Group 3 (high unemployment rate, short average UI duration)	-0.51	-0.45	0.26	-1.16**	0.09	-0.44	3,086
Group 4 (moderate unemployment rate, long average UI duration)	-0.78	-0.31	-1.20**	-0.72*	-0.87*	-0.74**	6,094

TABLE VII.6 (continued)

Subgroup	Treatment 1 Low Bonus, Short Qualification Period	2 Low Bonus, Long Qualification Period	3 High Bonus, Short Qualification Period	4 High Bonus, Long Qualification Period	5 Declining Bonus, Long Qualification Period	All Treatments Combined	Number of Claimants in Subgroup
Cohort							
Third Quarter 1988	-1.30	-4.86*##	0.44	-0.68	-0.42	-1.47	190
Fourth Quarter 1988	-2.09***#	-0.50	-1.03###	-1.04*	-0.38	-0.92**##	2,873
First Quarter 1989	-1.23*	-0.13	-1.08*###	-1.06**	-0.36	-0.81*##	3,036
Second Quarter 1989	-0.53	-0.53	-0.26##	-0.92*	-0.75#	-0.64#	3,231
Third Quarter 1989	0.84	-0.34	-0.50##	-0.33	-0.39	-0.21	3,241
Fourth Quarter 1989	0.21	1.32	2.45**	-0.31	1.52	1.06	1,071

NOTE: We ran statistical tests to determine (1) which subgroup impacts differed significantly from zero at conventional levels (*), and (2) w of the subgroup impacts differed significantly within the sets of subgroups (#)--for example, impacts for males were compared with imp for females. For characteristics with more than two subgroups, the tests are, respectively, comparisons with claimants ages 35 to comparisons with white claimants, comparisons with claimants from nonmanufacturing industries, comparisons with office group 2, comparisons with the final cohort (fourth-quarter 1989). The regressors also contained noninteracted regressors to control for base pe earnings, weekly benefit amount, and potential benefit duration.

[a]The office groups are organized as follows:
Group 1 includes Coatesville, Reading, and Lancaster.
Group 2 includes Connellsville.
Group 3 includes Lewistown, Pittston, and Scranton.
Group 4 includes Philadelphia-North, Philadelphia-Uptown, McKeesport, Butler, and Erie.

*Statistically significant at the 90 percent confidence level in a two-tail test.
**Statistically significant at the 95 percent confidence level in a two-tail test.
***Statistically significant at the 99 percent confidence level in a two-tail test.

#Significantly different than the impact for the reference group (see NOTE above) at the 90 percent confidence level.
##Significantly different than the impact for the reference group (see NOTE above) at the 95 percent confidence level.
###Significantly different than the impact for the reference group (see NOTE above) at the 99 percent confidence level.

TABLE VII.7

ESTIMATED IMPACTS OF THE TREATMENTS ON DOLLARS OF UI BENEFITS, BY SUBGROUP

Subgroup	Treatment 1 Low Bonus, Short Qualification Period	2 Low Bonus, Long Qualification Period	3 High Bonus, Short Qualification Period	4 High Bonus, Long Qualification Period	5 Declining Bonus, Long Qualification Period	All Treatments Combined	Number of Claimants in Subgroup
Gender							
Female	-101	-52	-99	-94	-70	-85	5,595
Male	-95	-60	-104	-145 **	-57	-97 **	8,318
Age							
Ages 25-34	-87	-75	-157 **	-124 **	-115	-114 **	7,556
Ages 35-54	-95	-29	-104	-148 *	-15	-85	4,872
55 or Older	-160	-54	183	-48	53	-4	1,485
Race							
White	-59	-46	-99 *	-143 ***	-31	-84 **	11,704
Black	-301	-207	-273 *	-76	-315 **	-210 *	1,623
Hispanic	-248	68	349	106	-19	59	506
Recall Status							
Expect Recall	41	69	-147	-121	-33	-53	1,512
Do Not Expect Recall	-114 *	-72	-96 *	-125 **	-66	-97 **	12,401
Industry							
Nondurable Manufacturing	-407 **##	-144	-370 **##	-177	-48	-208 *	1,525
Durable Manufacturing	-297 *#	-334 ***##	-363 ***##	-170	-277 **#	-275 ***##	2,068
Nonmanufacturing	-7	2	11	-90	17	-25	10,320
Office[a]							
Group 1 (low unemployment rate, short average UI duration)	-159	-101	-79	-194 **#	60	-105	3,693
Group 2 (high unemployment rate, long average UI duration)	102	72	18	124	-129	39	1,040
Group 3 (high unemployment rate, short average UI duration)	-123	-135	-100	-246 **#	-42	-143 *	3,086
Group 4 (moderate unemployment rate, long average UI duration)	-82	-13	-137 *	-63	-135	-81	6,094

TABLE VII.7 (continued)

Subgroup	Treatment 1 Low Bonus, Short Qualification Period	2 Low Bonus, Long Qualification Period	3 High Bonus, Short Qualification Period	4 High Bonus, Long Qualification Period	5 Declining Bonus, Long Qualification Period	All Treatments Combined	Number of Claimants in Subgroup
Cohort							
Third Quarter 1988	-421	-621 ##	-97	-368	-294	-378 #	190
Fourth Quarter 1988	-314 **#	-111 ##	-160 ###	-190 **	-105 #	-167 **###	2,873
First Quarter 1989	-196	-45 ##	-189 *###	-142	-98 #	-134 *##	3,309
Second Quarter 1989	-33	-126 ##	-53 ##	-131	-102 #	-99 ##	3,231
Third Quarter 1989	89	-78 ##	-186 *###	-88	-53 #	-74 ##	3,241
Fourth Quarter 1989	87	427	430 **	60	297 *	253 *	1,071

NOTE: We ran statistical tests to determine (1) which subgroup impacts differed significantly from zero at conventional levels (*), and (2) wh of the subgroup impacts differed significantly within the sets of subgroups (#)--for example, impacts for males were compared with impa for females. For characteristics with more than two subgroups, the tests are, respectively, comparisons with claimants ages 35 to comparisons with white claimants, comparisons with claimants from manufacturing industries, comparisons with office group 2, comparisons with the final cohort (fourth-quarter 1989). The regressions also contained noninteracted regressors to control for base per earnings, weekly benefit amount, and potential benefit duration.

[a]The office groups are organized as follows:
Group 1 includes Coatesville, Reading, and Lancaster.
Group 2 includes Connellsville.
Group 3 includes Lewistown, Pittston, and Scranton.
Group 4 includes Philadelphia-North, Philadelphia-Uptown, McKeesport, Butler, and Erie.

*Statistically significant at the 90 percent confidence level in a two-tail test.
**Statistically significant at the 95 percent confidence level in a two-tail test.
***Statistically significant at the 99 percent confidence level in a two-tail test.

#Significantly different than the impact for the reference group (see NOTE above) at the 90 percent confidence level.
##Significantly different than the impact for the reference group (see NOTE above) at the 95 percent confidence level.
###Significantly different than the impact for the reference group (see NOTE above) at the 99 percent confidence level.

benefits tended to be higher for females, as shown in Table VII.6, but the differences were neither large nor statistically significant. Claimants younger than age 35 years appear to have been affected by the bonus offers somewhat more than were older workers. The impacts on blacks tended to be higher than the impacts on whites, except for treatment 4, which appears to have had a greater impact on whites.

In general, the bonus offers appear to have had a greater impact on claimants who did not expect to be recalled to their pre-UI employer than on claimants who did expect to be recalled. Although this difference is neither large nor statistically significant, it is consistent with our expectation that the reemployment bonus would probably not induce many claimants who were waiting for recall to search for a new job. Part of the reason for the small overall difference in impacts between the recall and no-recall groups was that the estimated impacts for the two high bonus offers, treatments 3 and 4, were similar for the groups. Hence, the large bonus offers may have induced the claimants who expected to be recalled to search for a new job rather than to wait to be recalled, thereby reducing their UI receipt.

The bonus offers clearly had a greater impact on UI receipt among claimants from manufacturing industries than among claimants from nonmanufacturing industries. The final column in Table VII.6 shows that the combined treatments reduced UI receipt among claimants from manufacturing industries by more than one and a half weeks. This impact was significantly greater than the impact on claimants from nonmanufacturing industries, who appear to have been unaffected by the treatments.

To test how local economic environments affected the impacts of the bonus offers, we separated the 12 demonstration offices into four groups based on the local unemployment rate and the average UI duration of all UI claimants who applied to that office.[14] When we estimated impacts on weeks of benefits, the bonus offers had the greatest impact on UI receipt in areas whose local

[14]For a discussion of this categorization, see Section B of Chapter II.

unemployment rate was moderate and average UI duration was long, although differences in UI receipt across the office groups were not statistically significant. This finding makes sense, because claimants in these areas faced relatively long potential UI spells, but the moderate unemployment rate suggests that opportunities for reemployment were available to these claimants if they were induced to search for jobs more intensively. Hence, we would expect that the bonus offers would reduce UI receipt among claimants in these areas more readily than in areas where the unemployment rate was high and fewer reemployment opportunities were available.

The smallest impact occurred among the group that contains only Connellsville, which had both a high local unemployment rate and a long average UI duration. The small impact for Connellsville may have been due to the fact that claimants in this area faced relatively poor local economic conditions during the demonstration (as evidenced by the unemployment and UI duration figures), and these poor economic conditions restricted the ability of claimants to respond to the bonus offer.

This sensible pattern for the variation of impacts according to local economic conditions breaks down somewhat when we measure impacts on dollars of benefits, as shown in Table VII.7. As before, the smallest impact on UI receipt occurred in Connellsville, although the differences among sites were not statistically significant. However, the largest impact when measured in dollars of benefits occurred among group 3, the group of offices whose local unemployment rates were high and average UI durations were short. This impact occurred despite indications from local unemployment rates that opportunities for reemployment were limited in these areas.

Finally, the treatment impacts were greater among claimants who applied for benefits early in the demonstration than among claimants who applied for benefits later. Many of the treatment impacts in early quarters were significantly different from the impact for the fourth quarter of 1989, the final quarter of the demonstration. The wide variance in impacts over time was probably not due to variation in economic conditions, since they were unlikely to have changed enough during the demonstration to explain the differences in the treatment impacts. In addition, the variance in

impacts over time was probably not due to seasonal changes in economic conditions, because the impacts show no annual pattern. For example, the impact for the fourth quarter of 1988 is in the opposite direction of the estimated impact for the fourth quarter of 1989. Thus, the differences in the impact estimates across time must be due to unobserved differences in the type of claimants who entered the demonstration in each quarter or to differences in how the bonus offers were administered in each quarter.

VIII. IMPACTS ON EMPLOYMENT AND EARNINGS

The bonus offers in the Pennsylvania Reemployment Bonus Demonstration were expected to promote the rapid reemployment of claimants, and thus to have a positive impact on their employment and earnings following their application for UI benefits. In this chapter, we examine employment and earnings during the year following their benefit application to determine whether this impact occurred. We also examine the characteristics of the first post-unemployment job to evaluate the effect of the bonus offers on the type of jobs obtained by the claimants. We then use this information to discuss whether the bonus offers, by promoting rapid reemployment, induced claimants to accept jobs that were less desirable than those obtained by claimants who were not offered the bonus.

Because the bonus offers significantly reduced UI receipt, as demonstrated in Chapter VII, we expected also to observe an increase in employment and earnings. Given that bonuses were paid only to claimants who found reemployment, the bonus offers must have reduced UI receipt because they induced claimants to become reemployed more rapidly. If claimants who received bonus offers became reemployed more quickly, they should also have experienced greater levels of employment and earnings following their benefit application.

Data from the UI wage records and the follow-up interviews provide some evidence that the bonus offers increased the postapplication employment and earnings of claimants assigned to the treatments. The data from UI wage records show that the treatments had no impact on employment but had a generally positive but statistically insignificant impact on earnings. Despite the lack of statistically significant earnings impacts, the magnitudes of the estimates were consistent with the estimated impacts on UI receipt presented in Chapter VII.

Estimates based on the interview data provide somewhat stronger evidence that the treatments increased employment and earnings. The interview-based estimates demonstrate that the treatments

had a positive but insignificant effect on employment. However, the interview-based estimates show that the treatments increased postapplication earnings significantly in some cases. The interview data also suggest that the treatments increased earnings because they increased the probability of full-time employment.

Our analysis of the characteristics of post-unemployment jobs suggests that the bonus offers did not induce claimants to take less desirable jobs in an effort to qualify for the bonus. None of the treatments had a negative impact on the hourly wage rate in the first post-unemployment job. In addition, the first post-unemployment jobs held by bonus-eligible claimants were similar to the pre-unemployment jobs held by these claimants in many respects, including their weekly wage rate.

A. THE IMPACTS OF THE BONUS OFFERS ON EMPLOYMENT AND EARNINGS

This section examines the basic impacts on employment and earnings. Because we have two sources of data on employment and earnings--UI wage records and follow-up interviews--we begin our analysis by considering the advantages and disadvantages of each data source. We then present our main estimates of the impacts of the bonus offers on employment and earnings based on an analysis of the UI wage records, including the estimates for subgroups of claimants. We then present the findings based on the interview data.

1. Sources of Data on Employment and Earnings

Both the records data and the interview data have shortcomings for our employment and earnings analysis. An important shortcoming with the wage records is they are organized by calendar quarter, and thus could not be used to measure impacts that occurred *immediately* after the benefit application date. This inflexibility was deemed to be an important constraint, in light of previous studies of the reemployment bonus in the New Jersey UI Reemployment Demonstration which showed that the impact of the bonus offer occurred soon after the benefit application date (Corson et al., 1989; and Corson and Decker, 1990).

In addition to the inflexibility of wage records for measuring impacts that occurred immediately following the benefit application date, a variety of factors may have affected the accuracy of the wage records data. First, wage records may be inaccurate because employers misreport the wage rates or Social Security numbers of their employees. Second, the wage records also exclude the earnings of claimants who are employed outside the state or are employed outside the UI-covered sector (for example, those who are self-employed). Because such individuals would be included in our impact analysis of employment and earnings as if their employment and earnings were zero, the impact estimates would be biased toward zero. Third, earnings are reported in wage records when they were received, not when they were earned. For example, claimants may have received severance pay or pension pay-outs from their pre-UI employer after they applied for benefits. These payments would be misinterpreted as earnings from a post-UI job, overstating the earnings received by claimants following their benefit application date.

However, the interview data on employment and earnings also have shortcomings. First, as is the case with any interview data, the data extracted from follow-up interview responses contain measurement errors, due to such factors as faulty recall by respondents about dates or wage rates. Second, nonresponse bias is also a potential problem with the interview data.[1] We attempted to correct for potential nonresponse bias in our estimates based on the interview data, but since our corrections for nonresponse had almost no effect on the impact estimates we do not present the corrected estimates in this chapter. Finally, the size of the analysis sample was substantially smaller when we used the interview data because we interviewed only a subset of the full sample of claimants who participated in the demonstration.

Because UI wage records were available for the full analysis sample, we chose to use them as our primary source of earnings data. Hence, our main estimates of employment and earnings impacts were based on these data. However, we also used the interview data to examine the impacts of the

[1]Appendix A discusses our findings on the extent of nonresponse bias in our estimates.

bonus offers on employment and earnings. The interview data enabled us to detect any impacts that may have occurred shortly after the benefit application date. The estimates based on interview data provide somewhat stronger evidence that the treatments had a positive impact on employment and earnings, and these estimates suggest that part of the impact occurred shortly after the benefit application date.

2. Impacts on Employment and Earnings by Treatment Group

Our analysis of the impacts of the bonus offers on employment and earnings based on data from UI wage records focused on three quarterly measures: (1) whether claimants were employed, (2) the number of weeks that claimants were employed, and (3) the earnings received by claimants. We based each measure on the calendar quarters following the benefit application date for each individual. The first quarter that we examined was the quarter in which claimants applied for benefits. Although employment and earnings data for this quarter partly reflect experiences with pre-UI employers, random assignment implies that employment with and earnings from pre-UI employers during this quarter should not vary significantly across treatment groups. Hence, the majority of the between-group differences in employment or earnings in the quarter of benefit application should be attributable to the impact of the treatment on postapplication employment.

Estimates based on data from UI wage records fail to provide any evidence that the bonus offers enhanced the employment of claimants following the benefit application date. Table VIII.1 shows that only about half of the estimated impacts on the probability of employment had a positive sign, and that none of the impacts was significantly greater than zero. As demonstrated in Table VIII.2, the findings for weeks of employment were similar. Half of the estimated impacts on quarterly weeks of employment were positive, and the estimates were not significantly greater than zero at the 90 percent confidence level.

Similarly, the treatments do not appear to have significantly increased the earnings of claimants. The estimated treatment impacts on earnings were relatively small in all quarters and were often

TABLE VIII.1

ESTIMATED IMPACTS OF THE TREATMENTS ON THE PROBABILITY OF EMPLOYMENT
(Standard Errors in Parentheses)

Treatment	Period of Observation[a]			
	Quarter of Benefit Application	Quarter 1	Quarter 2	Quarter 3
1 - Low bonus, short qualification period	-0.8 % (1.2)	-1.6 % (1.6)	-0.6 % (1.5)	-2.8 % (1.4)
2 - Low bonus, long qualification period	-0.1 (1.0)	1.1 (1.3)	-0.5 (1.2)	-2.2 (1.2)
3 - High bonus, short qualification period	0.3 (1.0)	0.4 (1.4)	-0.4 (1.3)	0.4 (1.3)
4 - High bonus, long qualification period	-0.8 (0.9)	0.5 (1.2)	-0.1 (1.2)	-0.9 (1.1)
5 - Initially high but declining bonus, long qualification period	0.4 (1.1)	-1.3 (1.4)	0.1 (1.3)	-1.9 (1.3)
Control Group Mean	84.1 %	59.0 %	67.3 %	70.6 %

NOTE: The sample includes approximately 13,900 claimants for whom we have data on both demographic characteristics and employment, where the exact sample size depends on the period of observation. The explanatory variables contained in the regressions include treatment indicators, cohort indicators, office indicators, and demographic and economic variables. The full regression estimates are contained in Table E.4 of Appendix E.

[a]Quarters 1, 2, and 3 are the first, second, and third full calendar quarters after benefit application.

*Significantly greater than zero at the 90 percent confidence level in a one-tail test.
**Significantly greater than zero at the 95 percent confidence level in a one-tail test.
***Significantly greater than zero at the 99 percent confidence level in a one-tail test.

TABLE VIII.2

ESTIMATED IMPACTS OF THE TREATMENTS ON WEEKS OF EMPLOYMENT
(Standard Errors in Parentheses)

Treatment	Period of Observation[a]			
	Quarter of Benefit Application	Quarter 1	Quarter 2	Quarter 3
1 - Low bonus, short qualification period	-0.25	-0.22	-0.19	-0.46
	(0.15)	(0.17)	(0.18)	(0.18)
2 - Low bonus, long qualification period	-0.04	0.09	-0.07	-0.23
	(0.12)	(0.14)	(0.15)	(0.15)
3 - High bonus, short qualification period	-0.10	0.13	-0.08	0.00
	(0.13)	(0.15)	(0.16)	(0.16)
4 - High bonus, long qualification period	-0.14	0.14	0.09	-0.12
	(0.11)	(0.13)	(0.14)	(0.14)
5 - Initially high but declining bonus, long qualification period	0.02	-0.10	0.02	-0.19
	(0.13)	(0.16)	(0.16)	(0.16)
Control Group Mean	7.14	5.15	6.94	7.62

NOTE: The sample includes approximately 13,900 claimants for whom we have data on both demographic characteristics and employment, where the exact sample size depends on the period of observation. The explanatory variables contained in the regressions include treatment indicators, cohort indicators, office indicators, and demographic and economic variables. The full regression estimates are contained in Table E.5 of Appendix E.

[a]Quarters 1, 2, and 3 are the first, second, and third full calendar quarters after benefit application.

*Significantly greater than zero at the 90 percent confidence level in a one-tail test.
**Significantly greater than zero at the 95 percent confidence level in a one-tail test.
***Significantly greater than zero at the 99 percent confidence level in a one-tail test.

negative, as shown in Table VIII.3. Only one of the bonus offers, treatment 4, had a positive impact on earnings in each of the three full quarters following benefit application, but none of these quarterly impacts was significant at the 90 percent confidence level. Only treatment 3 had a significantly positive impact (at the 90 percent confidence level) on earnings in any quarter, increasing average earnings in the first *full* quarter after benefit application by $116, but even this impact could be attributable simply to chance.

Despite the lack of significant findings for employment impacts, the sum of the quarterly impacts was positive for four of the five treatments and generally consistent with the UI impacts discussed in Chapter VII. For example, in Chapter VII, we demonstrated that treatment 3 reduced average UI receipt by .44 weeks over the benefit year. If we use this impact to derive an expected impact on earnings based on the assumption that the .44-week reduction in UI receipt translates directly into a .44-week increase in employment, we find an expected impact of $121.[2] Our estimated impact of $137 for treatment 3 is quite close to this expected impact. The summed estimates for the other treatments are not as close to their expected impacts, but with the exception of treatment 1 they are positive and relatively small, as one would expect given the estimated UI impacts.[3]

However, treatment 1 was an outlier, with a fairly large negative impact on earnings over the period of observation. This finding was due largely to the estimate for quarter 3, which implies that treatment 1 reduced earnings in the third full quarter after benefit application by nearly $200. A negative impact of this magnitude seems implausible. Bonus offers would generate negative earnings impacts only if claimants took unstable jobs in an effort to receive the bonus, and then lost these jobs later and thus received less earnings in later periods. However, the UI impacts presented in Chapter

[2]We calculate the expected impact on earnings by multiplying .44 weeks by average weekly earnings in the based period, $276.

[3]We also tested the statistical significance of the treatment impacts on total earnings over the four quarters, and found that none of the impacts was significantly greater than zero.

TABLE VIII.3

ESTIMATED IMPACTS OF THE TREATMENTS ON EARNINGS
(Standard Errors in Parentheses)

Treatment	Period of Observation[a]				
	Quarter of Benefit Application	Quarter 1	Quarter 2	Quarter 3	Sum of Quarterly Impacts
1 - Low bonus, short qualification period	-$22	$12	-$85	-$191	-$286
	(99)	(83)	(83)	(82)	
2 - Low bonus, long qualification period	96	83	28	-4	203
	(82)	(69)	(69)	(68)	
3 - High bonus, short qualification period	-20	116 *	-14	55	137
	(89)	(74)	(74)	(73)	
4 - High bonus, long qualification period	-36	74	81	50	169
	(78)	(65)	(65)	(64)	
5 - Initially high but declining bonus, long qualification period	65	-62	2	34	39
	(90)	(75)	(75)	(74)	
Control Group Mean	$2,649	$1,698	$2,351	$2,605	$9,303

NOTE: The sample includes 13,913 claimants for whom we have data on both demographic characteristics and earnings. The explanatory variables contained in the regressions include treatment indicators, cohort indicators, office indicators, and demographic and economic variables. The full regression estimates are contained in Table E.6 of Appendix E.

[a]Quarters 1, 2, and 3 are the first, second, and third full calendar quarters after benefit application.

*Significantly greater than zero at the 90 percent confidence level in a one-tail test.
**Significantly greater than zero at the 95 percent confidence level in a one-tail test.
***Significantly greater than zero at the 99 percent confidence level in a one-tail test.

VII are not consistent with this story (or with the negative estimated earnings impacts). A further investigation of the impacts of treatment 1 on earnings failed to explain the negative estimates.[4]

Although the bonus offers did not increase earnings significantly for claimants in general, they increased earnings significantly for some subgroups of claimants. The estimates in Table VIII.4 show that the combined treatments had a positive and significant impact (at the 90 percent confidence level) on three subgroups of claimants.[5] These subgroups included claimants from nondurable manufacturing industries, claimants in the cohort that entered the demonstration in the first quarter of 1989, and claimants who applied for benefits at offices in areas whose unemployment rates were moderate and average UI durations were long.[6]

The size and statistical significance of these earnings impacts for claimant subgroups suggests that the demonstration had a substantial impact on earnings for at least some groups of claimants. The estimates for the industry subgroups imply that the combined bonuses increased the postapplication earnings of claimants by $891 if they were employed in a nondurable manufacturing industry prior to their unemployment spell. In addition to being significantly greater than zero, this impact was also significantly greater than the impact on claimants from nonmanufacturing industries, whose earnings were unaffected by the treatments. The impacts for office subgroups presented in Table VIII.4 show

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[4]We tried to address this issue by investigating outliers in the quarterly income measures. A number of claimants appeared to have had extremely high quarterly earnings, reaching levels of up to $100,000 in some quarters. A closer examination of the data on these claimants revealed that these high earnings levels represented severance pay or pension payouts that claimants received at some point after their job separation. However, removing claimants with extremely high quarterly earnings from the sample had almost no effect on the earnings impact estimates.

[5]As discussed in Chapter VII, we estimated impacts for subgroups by extending the regression model to include interaction terms in the regression equation. These interaction terms were equal to the product of the treatment indicators and the other control variables that we predicted would affect the size of the impact. We used linear combinations of the appropriate estimated parameters from this regression to estimate impacts by subgroups and to test for differences between subgroups (for example, males versus females).

[6]The offices whose local unemployment rates were moderate and average UI durations were long, which we refer to as group 4 in Table VIII.4, are generally offices in large urban areas, including Philadelphia-North, Philadelphia-Uptown, McKeesport, Butler, and Erie.

ESTIMATED IMPACTS OF THE TREATMENTS ON EARNINGS IN THE FIRST YEAR AFTER BENEFIT APPLICATION, BY SUBGROUP
(Dollars)

Subgroup	Treatment 1 Low Bonus, Short Qualification Period	2 Low Bonus, Long Qualification Period	3 High Bonus, Short Qualification Period	4 High Bonus, Long Qualification Period	5 Declining Bonus, Long Qualification Period	All Treatments Combined	Number Claiman in Subgrc
Gender							
Female	-178	300	146	178	3	100	5,595
Male	-219	121	179	241	58	87	8,318
Age							
Ages 25-34	-345	54	-23	113	-8	-43	7,556
Ages 35-54	-254	321	471	580 *	140	307	4,872
55 or Older	689	482	132	-457	-86	73	1,485
Race							
White	-228	184	169	226	47	89	11,704
Black	-404	451	268	205	187	177	1,623
Hispanic	554	-338	-272	-350	-1,002	-340	506
Recall Status							
Expected Recall	-348	-446	194	467	-383	-23	1,512
Did Not Expect Recall	-185	271	163	185	86	106	12,401
Industry							
Nondurable Manufacturing	742	1,567 ***###	1,909 ***###	582	-254	891 **##	1,525
Durable Manufacturing	-463	356	1,138 **###	-129	1,235 **##	384	2,068
Nonmanufacturing	-262	-96	-226	176	-199	-104	10,320
Office							
Group 1 (low unemployment rate, short average UI duration)	-817 *	-390	-840 **	198	-578	-411	3,693
Group 2 (high unemployment rate, long average UI duration)	666	-166	443	-309	54	22	1,040
Group 3 (high unemployment rate, short average UI duration)	-142	-7	425	448	-477	71	3,086
Group 4 (moderate unemployment rate, long average UI duration)	-11	708 **	596 *	198	663 **	419 *	6,094
Cohort							
Third Quarter 1988	-712	805	152	1,270	1,033	912	190
Fourth Quarter 1988	78 ##	455 ###	104	221	34	204 ###	2,873
First Quarter 1989	1,014 **###	733 *###	674 ##	469	316	631 **###	3,309
Second Quarter 1989	-445	378 ###	613 ##	-111	161	110 ##	3,231
Third Quarter 1989	-992 **	-94 ##	-273	579	-82	-118 ##	3,241
Fourth Quarter 1989	-1,762 ***	-1,980 **	-1,250 *	-888	-1,020	-1,438 ***	1,071

NOTE: The dependent variable is total earnings received during the calendar quarter of benefit application and the three full calendar quarters after benefit application. We ran statistical tests to determine (1) which subgroup impacts differed significantly from zero at conventional levels (*), and (2) which of the subgroup impacts differed significantly within the sets of subgroups (#)--for example, impacts for males were compared with impacts for females. For characteristics with more than two subgroups, the tests are, respectively, comparisons with claimants ages 35 to 54, comparisons with white claimants, comparisons with claimants from nonmanufacturing industries, comparisons with office group 2, and comparisons with the final cohort (fourth quarter 1989). The regressions also contained noninteracted regressors to control for base period earnings, weekly benefit amount, and potential benefit duration.

[a]The office groups are organized as follows:
Group 1 includes Coatesville, Reading, and Lancaster.
Group 2 includes Connellsville.
Group 3 includes Lewistown, Pittston, and Scranton.
Group 4 includes Philadelphia-North, Philadelphia-Uptown, McKeesport, Butler, and Erie.

*Statistically significant at the 90 percent confidence level in a two-tail test.
**Statistically significant at the 95 percent confidence level in a two-tail test.
***Statistically significant at the 99 percent confidence level in a two-tail test.

#Significantly different than the impact for the reference group (see NOTE above) at the 90 percent level in a two-tail test.
##Significantly different than the impact for the reference group (see NOTE above) at the 90 percent level in a two-tail test.
###Significantly different than the impact for the reference group (see NOTE above) at the 90 percent level in a two-tail test.

that the combined treatments increased the postapplication earnings of claimants by $419 if they applied for benefits in one of the five sites included in group 4. Finally, the cohort-specific impacts demonstrate that the combined treatments significantly increased the earnings of claimants who entered the demonstration in the first quarter of 1989, by $631. These subgroup impact estimates are consistent with the subgroup UI estimates presented in Chapter VII, which showed that the impacts of the treatments on UI receipt among these same subgroups were large and significant.

3. Alternative Estimates Based on the Interview Data

Estimates based on the interview data provide slightly stronger evidence than the records-based estimates that the treatments increased employment among the full sample of claimants. The interview-based estimates suggest that the treatments increased postapplication employment, but the estimated impacts were mostly insignificant. As shown in Table VIII.5, all five treatments had a positive estimated impact on the proportion of time employed in the first two quarters following benefit application.[7] However, only 2 of these 10 quarterly impact estimates were significantly greater than zero at the 90 percent confidence level. The estimated impacts in the latter two quarters varied considerably, with half of the estimates taking on negative values.

Conversely, the interview-based estimates suggest that at least some of the treatments significantly increased postapplication earnings. The interview-based impacts on earnings were positive in almost every quarter, as shown in Table VIII.6. Six of the 10 impact estimates in quarters 1 and 2 were greater than $100, and 5 of those 6 estimates were statistically significant at the 90 percent confidence level. The 10 quarterly earnings impacts in quarters 3 and 4 were also relatively large, but only one of the estimates was statistically significant at the 90 percent confidence level.

The magnitudes of the earnings impacts were consistently larger when they were based on the interviews rather than on the wage records. For example, the interview-based estimates in Table

[7]For the interview data, quarters were defined relative to the benefit application date.

TABLE VIII.5

ESTIMATED IMPACTS OF THE TREATMENTS ON THE PROPORTION OF TIME EMPLOYED, BASED ON INTERVIEW DATA
(Standard Errors in Parentheses)

Treatment	Period of Observation[a]			
	Quarter 1	Quarter 2	Quarter 3	Quarter 4
1 - Low bonus, short qualification period	1.7 % (1.8)	4.9 %** (2.3)	3.1 % * (2.2)	2.8 % (2.5)
2 - Low bonus, long qualification period	2.4 * (1.4)	1.2 (1.9)	-2.6 (1.8)	-2.7 (2.1)
3 - High bonus, short qualification period	1.7 (1.6)	0.2 (2.1)	-1.2 (1.9)	2.4 (2.2)
4 - High bonus, long qualification period	0.2 (1.4)	0.2 (1.8)	-0.6 (1.7)	-0.2 (1.9)
5 - Initially high but declining bonus, long qualification period	1.6 (1.6)	2.0 (2.1)	3.0 * (2.0)	1.3 (2.3)
Control Group Mean	25.1 %	54.3 %	68.3 %	73.1 %

NOTE: The sample includes 5,134 claimants for whom we have data on both demographic characteristics and employment. The explanatory variables contained in the regressions include treatment indicators, cohort indicators, office indicators, and demographic and economic variables. Observations are weighted to correct for the oversampling of bonus recipients. The full regression estimates are contained in Table E.7 of Appendix E.

[a]Quarters are defined relative to the benefit application date.

*Significantly greater than zero at the 90 percent confidence level in a one-tail test.
**Significantly greater than zero at the 95 percent confidence level in a one-tail test.
***Significantly greater than zero at the 99 percent confidence level in a one-tail test.

TABLE VIII.6

ESTIMATED IMPACTS OF THE TREATMENTS ON EARNINGS,
BASED ON INTERVIEW DATA
(Standard Errors in Parentheses)

Treatment	Period of Observation[a]				Sum of Quarterly Impacts
	Quarter 1	Quarter 2	Quarter 3	Quarter 4	
1 - Low bonus, short qualification period	$134 *	$287 **	$99	-$90	$430
	(90)	(134)	(142)	(174)	
2 - Low bonus, long qualification period	152 **	135	122	129	538
	(73)	(108)	(114)	(144)	
3 - High bonus, short qualification period	111 *	14	2	112	239
	(80)	(118)	(125)	(154)	
4 - High bonus, long qualification period	73	130 *	142 *	131	476
	(68)	(101)	(107)	(133)	
5 - Initially high but declining bonus, long qualification period	48	70	146	95	359
	(81)	(120)	(127)	(156)	
Control Group Mean	$990	$2,255	$2,816	$3,128	$9,189

NOTE: The sample includes 5,134 claimants for whom we have data on both demographic characteristics and earnings. The explanatory variables contained in the regressions include treatment indicators, cohort indicators, office indicators, and demographic and economic variables. Observations are weighted to correct for the oversampling of bonus recipients. The full regression estimates are contained in Table E.8 of Appendix E.

[a]Quarters are defined relative to the benefit application date.

*Significantly greater than zero at the 90 percent confidence level in a one-tail test.
**Significantly greater than zero at the 95 percent confidence level in a one-tail test.
***Significantly greater than zero at the 99 percent confidence level in a one-tail test.

VIII.6 imply that the impact of treatment 4 on earnings over the first four relative quarters after benefit application was nearly equal to $480. In contrast, the records-based estimated impact of treatment 4 on earnings in the calendar quarter of benefit application and in the following three calendar quarters was equal to less than $170, or about a third of the interview-based estimate (see Table VIII.3). The difference in estimates based on the data source is also evident for treatment 1, which had a negative records-based impact on earnings (-$286) but a positive interview-based impact on earnings ($430). These differences demonstrate that the interview data provide stronger evidence that the treatments increased postapplication earnings.

The interview-based estimates also show that a large part of the estimated impact on earnings occurred shortly after the benefit application date. Three of the five treatments had a significant impact on earnings during the first 13 weeks after benefit application. This finding may partly explain the differences between the records-based earnings impacts and the interview-based earnings impacts. As mentioned earlier, wage records data constrained our ability to isolate the impacts that occurred in the period shortly after benefit application, because the records are organized by calendar quarter. But the differences between the earnings impacts based on the two alternative data sources existed in later quarters as well, suggesting that the differential organization of the two data sets can explain only a small part of the differences between the two sets of impact estimates.[8]

The interview-based evidence presented in this section suggests that the treatments may have increased earnings significantly without increasing employment significantly. These two seemingly

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[8]We also redefined the survey data to organize the earnings measures by calendar quarter as they were in the wage records. When we estimated the earnings estimates from the interview based on these measures, they were clearly larger than the estimates based on the wage records in the full quarters after benefit application. Hence, it was not the difference in how the two sets of earnings measures were organized that created differences between the two sets of impact estimates.

The differences between the two sets of estimates may be due to in part to nonresponse in the interview sample. An investigation of the UI outcomes revealed that UI impacts were larger for respondents than for nonrespondents. A similar finding may exist for employment and earnings impacts. However, when we corrected for nonresponse in our regressions, it did not substantially affect the impact estimates.

contradictory findings would arise if the bonus offers increased the incidence of full-time employment but reduced the incidence of part-time employment. In this case, the bonus offers could increase the average number of hours worked by claimants without increasing the proportion of time employed per quarter, as was measured in Table VIII.5. Bonus offers would thus increase employment, but only when measured in terms of hours of employment. We investigated this issue directly by comparing the estimated impacts on the probability of employment in each quarter with the estimated impacts on the probability of full-time employment in each quarter.

The estimates presented in Tables VIII.7 and VIII.8 demonstrate that the bonus offers increased the probability of full-time employment more than they increased the probability of employment overall. The point estimates for the impacts on full-time employment (shown in Table VIII.8) were generally greater than the corresponding estimates for any employment (shown in Table VIII.7). The estimates for full-time employment were also significantly greater than zero in more cases than were the estimates for any employment. For example, all of the treatments significantly increased the probability of full-time employment in the first quarter after benefit application at the 90 percent confidence level. In contrast, only two of the treatments significantly increased the probability of any employment. In addition, all of the estimated impacts on full-time employment were greater than zero, while the majority of the estimates for any employment in quarters 3 and 4 were less than zero.

The differences between these two sets of estimates suggests that the bonus offers increased the probability of full-time employment but reduced the probability of part-time employment, thereby yielding little impact on the probability of employment overall. This effect may have occurred because claimants who were assigned to the treatments received a bonus only if they found full-time employment. The bonus offers may thus have induced claimants who would otherwise participate in part-time employment after their layoff to find full-time employment instead in order to receive the bonus.

TABLE VIII.7

ESTIMATED IMPACTS OF THE TREATMENTS ON THE PROBABILITY OF EMPLOYMENT, BASED ON INTERVIEW DATA
(Standard Errors in Parentheses)

Treatment	Period of Observation[a]			
	Quarter 1	Quarter 2	Quarter 3	Quarter 4
1 - Low bonus, short qualification period	3.6 %*	7.0 %***	2.8 %	3.1 %
	(2.6)	(2.4)	(2.2)	(2.5)
2 - Low bonus, long qualification period	2.4	0.0	-3.4	-3.2
	(2.1)	(2.0)	(1.8)	(2.0)
3 - High bonus, short qualification period	1.5	0.5	-1.4	1.7
	(2.3)	(2.2)	(1.9)	(2.2)
4 - High bonus, long qualification period	1.5	-0.4	-0.5	-0.1
	(2.0)	(1.8)	(1.7)	(1.9)
5 - Initially high but declining bonus, long qualification period	3.6 *	2.6	1.4	-0.4
	(2.4)	(2.2)	(2.0)	(2.2)
Control Group Mean	45.0 %	66.0 %	77.6 %	85.1 %

NOTE: The sample includes 5,134 claimants for whom we have data on both demographic characteristics and employment. The explanatory variables contained in the regressions include treatment indicators, cohort indicators, office indicators, and demographic and economic variables. Observations are weighted to correct for the oversampling of bonus recipients. The full regression estimates are contained in Table E.9 of Appendix E.

[a]Quarters are defined relative to the benefit application date.

*Significantly greater than zero at the 90 percent confidence level in a one-tail test.
**Significantly greater than zero at the 95 percent confidence level in a one-tail test.
***Significantly greater than zero at the 99 percent confidence level in a one-tail test.

TABLE VIII.8

ESTIMATED IMPACTS OF THE TREATMENTS ON THE PROBABILITY OF FULL-TIME EMPLOYMENT, BASED ON INTERVIEW DATA
(Standard Errors in Parentheses)

Treatment	Period of Observation[a]			
	Quarter 1	Quarter 2	Quarter 3	Quarter 4
1 - Low bonus, short qualification period	5.5 %**	6.2 %***	3.6 %*	1.4 %
	(2.6)	(2.6)	(2.4)	(2.8)
2 - Low bonus, long qualification period	3.4 *	0.9	0.0	0.3
	(2.1)	(2.1)	(2.0)	(2.3)
3 - High bonus, short qualification period	3.9 **	3.2 *	1.9	4.9 **
	(2.3)	(2.3)	(2.2)	(2.5)
4 - High bonus, long qualification period	2.9 *	1.3	0.7	0.2
	(1.9)	(1.9)	(1.8)	(2.1)
5 - Initially high but declining bonus, long qualification period	3.4 *	3.1 *	2.8 *	2.8
	(2.3)	(2.3)	(2.2)	(2.5)
Control Group Mean	35.7 %	54.3 %	63.6 %	68.9 %

NOTE: The sample includes 5,134 claimants for whom we have data on both demographic characteristics and employment. The explanatory variables contained in the regressions include treatment indicators, cohort indicators, office indicators, and demographic and economic variables. Observations are weighted to correct for the oversampling of bonus recipients. The full regression estimates are contained in Table E.10 of Appendix E.

[a]Quarters are defined relative to the benefit application date.

*Significantly greater than zero at the 90 percent confidence level in a one-tail test.
**Significantly greater than zero at the 95 percent confidence level in a one-tail test.
***Significantly greater than zero at the 99 percent confidence level in a one-tail test.

B. IMPACTS ON THE FIRST POST-UNEMPLOYMENT JOB

A potential negative effect of the bonus offers occurred if claimants who received a bonus offer sacrificed wages or other job characteristics in order to hasten reemployment and qualify for the bonus. In this section, we attempt to determine whether such an effect occurred in the Pennsylvania Reemployment Bonus Demonstration. We investigate this issue in two ways. First, we consider the post-unemployment jobs held by claimants who submitted a Notice of Hire and were eligible to receive a bonus. We examine the characteristics of these post-unemployment jobs and compare them with the characteristics of the claimants' pre-unemployment jobs. Second, for all claimants who found a job before the interview, we tested whether any of the bonus offers significantly reduced the hourly wage rate received in the first post-unemployment job.

The pre-unemployment and post-unemployment job characteristics summarized in Table VIII.9 suggest that bonus-eligible claimants did not take less desirable jobs in an effort to receive a reemployment bonus. The distribution of the post-unemployment weekly wage rate was similar to the distribution of the pre-unemployment weekly wage rate received by the claimants. In addition, bonus-eligible claimants appear to have worked somewhat fewer hours in their post-unemployment job than in their pre-unemployment job. For example, 35 percent of claimants worked more than 40 hours per week in the first job after their unemployment spell, versus 43 percent who worked more than 40 hours per week before their unemployment spell. This difference was statistically significant at the 99 percent confidence level. Hence, compared with their pre-unemployment job, bonus-eligible claimants appear to be have been working fewer hours but making about the same amount of money in their first post-unemployment job.

The distribution of jobs by industry and occupation among bonus-eligible claimants also changed only slightly after reemployment, suggesting that claimants did not abandon previous industries or occupations in order to receive a bonus. On average, bonus-eligible claimants tended to move out of nondurable manufacturing somewhat (from 23 percent of pre-unemployment jobs to 19 percent

TABLE VIII.9

COMPARISON OF PRE- AND POST-UNEMPLOYMENT JOB CHARACTERISTICS OF BONUS-ELIGIBLE CLAIMANTS
(Percent)

Characteristic	Pre-Unemployment Job Characteristics, All Treatment Groups Combined	Post-Unemployment Job Characteristics, All Treatment Groups Combined
Weekly Wage		
$200 or Less	14.2	15.5
$201 to $300	28.7	28.3
$301 to $400	19.7	21.0
$401 to $500	15.2	13.0
$501 to $800	15.8	16.2
$801 or More	6.4	6.2
Hours Per Week		
32 or Less	5.1	4.4
32 to 39	6.7	9.8
40	45.1	50.6
41 to 45	14.6	11.8
46 or More	28.5	23.4
Industry		
Agriculture/Forestry/Fishing/Mining	2.1	1.6
Construction	11.1	10.3
Durable Manufacturing	11.4	11.8
Nondurable Manufacturing	23.0	18.8
Transportation/Public Utilities	7.2	8.0
Wholesale Trade/Retail Trade	18.8	19.1
Finance/Insurance/Real Estate Services	25.4	29.0
Public Administration	1.1	1.5

TABLE VIII.9 (continued)

Characteristic	Pre-Unemployment Job Characteristics, All Treatment Groups Combined	Post-Unemployment Job Characteristics, All Treatment Groups Combined
Occupation		
Managerial/Professional	15.6	14.4
Technical	3.3	4.0
Sales	11.7	11.2
Administrative Support	21.4	21.9
Service	7.0	7.9
Mechanical and Repair	6.7	6.3
Construction and Extractive	5.9	5.5
Precision Production	2.9	3.4
Machine Operators	12.1	11.1
Transportation	6.9	5.7
Handlers	5.0	7.1
Farming, Fishing, and Forestry	1.8	1.0
Sample Size	447	447

NOTE: Observations are weighted to correct for oversampling of bonus recipients.

of post-unemployment jobs) and into financial, insurance, or real estate services (from 25 percent to 29 percent). But the rest of the industrial categories specified in Table VIII.9 retained about the same proportion of bonus-eligible claimants after unemployment spells. The distributions of pre- and post-unemployment jobs according to occupation were also similar. No single occupational category either gained or lost a significant share of bonus-eligible claimants.

We also tested the impact of the bonus offers on post-unemployment job characteristics using the sample of all claimants who reported finding at least one job as of the follow-up interview. We used this sample to estimate the impacts of the treatments on the hourly wage rate in the first post-unemployment job reported in the follow-up interview.[9]

The estimated impacts on the post-unemployment hourly wage rate demonstrate that claimants who received a bonus offer did not sacrifice wages to become reemployed quickly. On the contrary, the regression-adjusted estimates shown in Table VIII.10 demonstrate that claimants who received bonus offers tended to earn higher hourly wages in their first post-unemployment job than did claimants in the control group, although the positive impacts were not statistically significant. The selection-corrected estimates also revealed no significant impacts on hourly wages. Only one of the treatments, treatment 1, had an estimated negative impact on hourly wages in the first post-unemployment job, but this impact was nearly equal to zero and was statistically insignificant.

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[9]Because we could estimate the wage equation only with working claimants, the estimates may have been subject to "selection bias," and they may thus overstate any negative impact of the treatments on the post-unemployment hourly wage. Selection bias occurred if the reemployed claimants in the treatment group were a less "select" group than the reemployed claimants in the control group, who became reemployed even in the absence of a bonus offer. In this case the difference in wages between the two groups would represent a downwardly biased estimate of the impact of the treatments on hourly wages.

To correct for potential selection bias, we used a two-stage estimation method developed by Heckman (1979). We present the full regression estimates for both stages of this procedure in Appendix D.

TABLE VIII.10

ESTIMATED IMPACTS OF THE TREATMENTS ON THE HOURLY WAGE IN THE FIRST POST-UNEMPLOYMENT JOB
(Standard Errors in Parentheses)

Treatment	Regression-Adjusted Estimates	Selection-Corrected Estimates
1 - Low bonus, short qualification period	$.10 (.19)	-$.03 (.19)
2 - Low bonus, long qualification period	.16 (.16)	.19 (.16)
3 - High bonus, short qualification period	.13 (.17)	.08 (.17)
4 - High bonus, long qualification period	.11 (.14)	.14 (.15)
5 - Initially high but declining bonus, long qualification period	.03 (.17)	.02 (.17)
Control Group Mean	$8.31	$8.31

NOTE: The regression-adjusted estimates and selection-corrected estimates are based on a model that includes the following as explanatory variables: treatment indicators, cohort indicators, office indicators, and demographic and economic variables. The sample includes 4,190 reemployed claimants for whom we have data on both demographic characteristics and post-unemployment earnings. The full regression estimates for the models are contained in Table E.11 of Appendix E.

IX. BENEFIT-COST ANALYSIS

In this chapter, we combine estimates of the impacts of the Pennsylvania Reemployment Bonus Demonstration with estimates of the costs of the demonstration to assess whether the benefits of each bonus offer exceeded its costs. We assess benefits and costs from several perspectives--those of the major groups affected by the demonstration (claimants, employers, and the government budget) and society as a whole. The purpose of this exercise is to summarize the information from the evaluation in order to help policymakers determine the relative desirability of using any of the tested reemployment bonuses on an ongoing basis. We address three main issues in the benefit-cost evaluation:

1. The costs of providing each of the bonus offers on an ongoing basis
2. The effects of each bonus offer on benefits and costs from the perspectives of claimants, employers, the government, and society as a whole, and whether benefits outweigh costs or vice versa
3. The sensitivity of the findings to changes in the impact estimates used to calculate the benefits and costs

Our findings demonstrate that four of the five bonus offers (treatments 2 to 5) generated positive net benefits to claimants and to society as a whole. However, the four treatments yielded net losses for the UI trust funds. Despite these net losses to the UI trust funds, two of the four treatments (treatments 2 and 4) yielded positive net benefits to the government as a whole, and a third treatment (treatment 3) generated equal benefits and costs to the government. The government incurred a modest loss for the declining bonus offer, treatment 5.

The least generous bonus offer, treatment 1, yielded net losses for all perspectives except the UI trust fund. However, these results are driven by the unusually large negative impacts on earnings that were estimated for treatment 1, and we thus view the benefit-cost finding for treatment 1 with extreme skepticism.

Our benefit-cost analysis is based on a comprehensive analytical framework. In Section A we discuss how we use this framework to measure benefits and costs. In Section B we estimate the net costs of each of the bonus offers and test the sensitivity of the estimates to alternative assumptions. In Section C, we use these cost estimates, together with our estimates of the impacts of the treatments on UI benefits, employment, and earnings, to conduct the benefit-cost analysis of each of the bonus offers. In Section D, we investigate the sensitivity of the findings to changes in the impact estimates.

A. METHODOLOGY

The analytical framework that we use to compare the benefits and costs of the bonus offers in the Pennsylvania Reemployment Bonus Demonstration includes several steps. The first step is to define the various perspectives from which benefits and costs are measured. First, we consider the benefits and costs to the UI claimants themselves, to determine whether the bonus offers were beneficial to those whom they were designed to serve. Second, we consider the perspective of the employers who hired the claimants, to examine the net effects of their hiring decisions. Third, we consider the perspective of the government, to assess the budgetary impacts of each treatment relative to existing programs. We also break the government perspective down into the UI trust funds versus other government programs, to obtain more specific insight into the distributional implications of these treatments. Together, these perspectives comprise all of society, and the benefits and costs from each perspective can be summed to derive the net benefits and costs to society as a whole.

After the relevant perspectives have been defined, the next step in the analysis is to construct a comprehensive list of the expected benefits and costs from each perspective.[1] From the

[1]Some of the benefits and costs are difficult to value in dollar terms. Although these *intangible* benefits (or costs) are not susceptible to measurement (such as the psychological benefits to claimants from obtaining a job), it is still important that they be assigned to the specific perspective so that policy judgments can be made about their likelihood of affecting the measured benefit-cost

perspective of UI claimants, the key benefits of the demonstration treatments are the bonus payments and any increase in earnings and fringe benefits generated by more rapid reemployment. More rapid reemployment should also be a psychological benefit to claimants, since most persons find unemployment stressful. On the other hand, when claimants become reemployed more rapidly, they lose some of their UI benefits, they pay additional taxes on their increased earnings, they lose leisure time, and they incur any costs associated with working (for example, child care or transportation expenses).

Employers benefit from the increased output produced by claimants who are spurred by the bonus offers to find jobs rapidly, but they also incur costs because they must compensate these employees with salaries and fringe benefits. We make the assumption that the value of the additional output to employers equals the value of the additional compensation by employers, which implies that the demonstration had no net effect on employers. However, this assumption may understate the benefits derived by employers from a labor market that functions more effectively, thereby reducing recruitment and turnover costs. Alternatively, the treatments might impose a cost on some employers if some temporarily laid-off workers were unavailable for rehire because they obtain jobs with new employers.[2]

We assume that the UI trust funds would incur the costs of administering the bonus offers and making bonus payments in an ongoing program. The UI trust funds would benefit from a share of payroll-tax increases paid by claimants and their employers, and from reductions in UI benefits paid to claimants and the costs of providing those benefits. Whether the increase in payroll taxes or the reduction in UI benefits is large enough to offset the net costs of the demonstration is one of the key issues in this analysis.

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comparisons.

[2]Both employers and claimants could also be affected by any changes in taxes due to an increase (or reduction) in government costs from offering the treatments. However, any such changes would occur only in the long run, and their effect would depend on how the treatments were funded. For this reason, they are not included in this analysis.

The other sectors of government inevitably derive net benefits from these treatments (assuming that at least some positive earnings impacts occur) because they receive the portion of claimants' taxes not used to fund UI.[3]

The benefits and costs from all of these perspectives are summed to determine the benefits and costs to society as a whole. On the benefit side, the increased earnings of claimants represent an increase in total output and thus a net benefit to society. A key assumption underlying this approach for valuing output is that the more rapid reemployment of demonstration claimants did not displace the employment of other individuals. In Appendix C, we discuss whether displacement occurred during the Pennsylvania Reemployment Bonus Demonstration. As part of our sensitivity analysis, we consider the effect of displacement on the benefit-cost findings. On the cost side, the net operational costs of offering each treatment represent social resources that could be spent otherwise, and are thus measured as net costs to society.

The general approach for valuing the benefits and costs of the five bonus offers is to measure the market value of the resources consumed, saved, or produced due to the bonus offer. We estimated the market value of these resources for the period in which they were expended or received. We estimated the operational costs of the demonstration for the time period in which it operated, based on the assumption that all costs were incurred during this period.[4] On the other hand, it is possible that the benefits of the demonstration were realized over a longer period of time. However, the impact estimates discussed in Chapters VII and VIII do not indicate that the bonuses generate significant long-term benefits.

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[3]Other sectors of the government could also benefit if the treatments reduced the receipt of such benefits as food stamps or other public assistance. We examined this potential effect, but because we found no impacts we have not included these potential impacts in the benefit-cost framework.

[4]We used actual market prices whenever available to value benefits and costs, on the assumption that these prices are the best measure of the true costs of these resources. When market prices were not available, it was necessary to estimate the dollar value of resources. For example, we estimated the value of fringe benefits, taxes, and the administrative costs of government agencies.

B. THE ESTIMATED COSTS OF AN ONGOING BONUS PROGRAM

Two types of costs are associated with conducting a reemployment bonus program--bonus payments and administrative costs. We discussed the bonus payments of the demonstration in Chapter VI, and in this chapter we assess the costs of the resources used to administer an ongoing program. Our estimates are based on the actual operating costs of the demonstration, adjusted to simulate the costs of an ongoing program. We also test the sensitivity of our estimates to alternative assumptions.

1. Estimation Process

Although actual demonstration expenditures do not accurately predict the costs of an ongoing program, they provide the best starting-point for estimating program costs. We thus based our estimates on state expenditure records for the demonstration. We adjusted these numbers in three ways to reflect assumptions about an ongoing program:

1. We focused on operational expenditures that most closely resembled an ongoing program.
2. We eliminated expenditures for activities that were specific to the demonstration.
3. We distributed the total remaining expenditures across the treatment groups and converted them into per-claimant estimates.

The following discussion examines each of these adjustments in more detail.

a. Start-up Versus Ongoing Operational Costs

As with any demonstration, the Pennsylvania Reemployment Bonus Demonstration encompassed a planning phase and an operational phase. Given that we are interested in estimating the costs of administering an ongoing program, our estimation procedures focused on the expenditures incurred during the operational phase. Moreover, we based our estimates on a time period that excluded the weeks when the demonstration was just starting up, as well as the weeks after intake ended, when

the demonstration was being phased down. Our estimates of local office costs come from the period between January 1989 and September 1989. This period represents a "steady-state" period during which local offices provided bonus offers to a steady stream of claimants.[5] In estimating central office costs, we used the time period between April 1989 and September 1989. This period represents a time in which central office staff processed a full complement of Notices of Hire and Bonus Vouchers.[6]

b. Estimating the Costs of an Ongoing Program

As indicated earlier, the second step in estimating the costs of an ongoing program was to separate expenditures for demonstration-specific tasks from those that would be incurred in an ongoing program. To make this separation, we examined the functions performed by each division at both the central and local office levels and decided whether or not these functions would be included in an ongoing program. We then adjusted the actual expenditures accordingly.

c. Estimating per-Claimant Expenditures by Treatment Group

The final steps in preparing our cost estimates for an ongoing program entailed allocating total costs across the treatment groups and converting these costs into per-claimant expenditures. These steps are necessary in order to compare costs with benefits across the treatment and control groups. In addition, the conversion of costs to per-claimant expenditures allows us to sum the central and local office operational costs, which we estimated over two different time periods.

Most of the central and local office functions required the same level of effort per claimant, regardless of his or her treatment group assignment. Thus, per-claimant expenditures for each

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[5]Once selection began in mid-October 1988, it took several weeks for a steady stream of demonstration claimants to be offered bonuses at the local offices. In addition, the number of bonus offers provided per week declined after demonstration selection ended in mid-October 1989.

[6]We used April 1989 as the starting date for measuring costs so that the early claimants, who entered the demonstration in or before mid-October 1988, had at least 6 weeks to find a bonus-eligible job and 16 more weeks to receive the bonus.

treatment group can be calculated simply as the total cost divided by the number of treatment group members in the analysis sample who were selected during the period on which costs are based.[7] The local office cost estimate for each treatment group is the total cost incurred in January 1989 through September 1989 divided by the number of treatment-group members selected during that period. Similarly, per-claimant costs for the central office were based on the number of treatment-group members selected from April 1989 through September 1989.

The only exception to this procedure pertained to allocating central office UI and check-writing costs across treatment groups. We allocated central office UI costs to each treatment according to the distribution of bonus claims across the treatments. Similarly, we allocated check-writing costs according to the distribution of payments across the treatment groups. These adjustments effectively assumed that greater resources would be required for treatment groups who had a greater share of the bonus claims and payments.

2. Estimating Administrative Costs

Using the procedures identified above, we developed a set of estimated administrative costs for an ongoing program. Operational costs were estimated to be $31 per claimant for all the treatments except for treatment 4. Of this figure, local office costs were $17 per claimant, central office labor costs were $8 per claimant, and nonlabor costs were $5 per claimants. Treatment 4 had slightly higher administrative costs ($34 per claimant), attributable to higher central office labor costs ($11 per claimant) because a greater proportion of bonus claims and payments were processed for this group. Adding these administrative costs to the bonus payments for each treatment, which we presented in Chapter VI, generates a range of per-claimant expenditures of $70 per claimant for treatment 1 to $180 per claimant for treatment 4.

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[7]Because claimants did not express much interest in the demonstration job-search workshop, our calculations of operating costs do not include expenditures related to the workshop.

3. The Sensitivity of the Estimates to Alternative Assumptions

Although these administrative costs represent our best estimate of what the costs of an ongoing program would be, we also investigated how alternative cost assumptions might affect these estimates. Most of our sensitivity tests of the cost estimates had a relatively small effect on our benchmark estimates. The largest effect arose when we used actual demonstration expenditures to provide an upper-bound estimate of the costs of an ongoing bonus program. The estimates based on the actual demonstration costs were 26 percent (or about $8) higher than the benchmark estimates. In other sensitivity tests, we varied the estimates of the central or local office labor costs. The largest impact arose in a test in which we assumed that local office UI staff rather than JS staff would offer the bonus, and that offer time was reduced by 50 percent. These assumptions led to per-claimant estimates that were, at most, 23 percent lower than the benchmark estimates.

C. BENEFITS AND COSTS FROM ALTERNATIVE PERSPECTIVES

In this section, we present baseline estimates of the net benefits and costs of the five bonus offers tested in the Pennsylvania Reemployment Bonus Demonstration. Table IX.1 provides a summary of the findings for each of the five bonus offers, which are presented in greater detail in Tables IX.2 to IX.6. We based the calculations of net benefits on the baseline estimates of administrative costs presented in Section B, the bonus payments presented in Chapter VI, and the estimated impacts of the treatments on UI receipt and earnings presented in Chapters VII and VIII.

Claimants received positive net benefits from most of the bonus offers (Table IX.1). Three of the bonus offers (treatments 2 to 4) yielded net benefits for claimants of well over $100 per claimant. As shown in Tables IX.3 to IX.5, claimants who received these bonus offers incurred net benefits because they received greater average earnings and fringe benefits, and because some claimants received bonus payments for early reemployment. Treatment 2 yielded the greatest net benefits, increasing the income of claimants by an average of $180 in the year after the benefit application

TABLE IX.1

SUMMARY OF THE ESTIMATED NET BENEFITS OF EACH TREATMENT, BY PERSPECTIVE
(Dollars per Claimant)

	Perspective					
			Government			
Treatment	Claimant	Employer	UI Trust Fund	Other Government	Government Total	Society
1 - Low bonus, short qualification period	-312	0	30	-94	-64	-376
2 - Low bonus, long qualification period	180	0	-19	54	35	215
3 - High bonus, short qualification period	123	0	-25	37	12	135
4 - High bonus, long qualification period	171	0	-51	52	0	172
5 - Initially high but declining bonus, long qualification period	57	0	-58	17	-41	17

NOTE: The numbers in this table are taken from Tables IX.2 to IX.5.

TABLE IX.2

BENEFIT-COST ANALYSIS OF TREATMENT 1
(LOW BONUS OFFER, SHORT QUALIFICATION PERIOD)
(Dollars per Claimant)

	Perspective					
			Government			
Benefits and Costs	Claimant	Employer	UI Trust Fund	Other Government	Government Total	Society
Market Output and Wages						
Increased Output	0	-346	0	0	0	-346
Wages and Fringe Benefits	-346	346	0	0	0	0
Tax Payments						
Claimants' Taxes	98	0	-4	-94	-98	0
Income Support Payments						
UI Payments	-103	0	103	0	103	0
Other Payments	0	0	0	0	0	0
Administrative Costs of Income Support Programs						
UI Payment Administration	0	0	1	0	1	1
Administration of Other Programs	0	0	0	0	0	0
Demonstration Costs						
Reemployment Bonuses	39	0	-39	0	-39	0
Local Office Labor Costs	0	0	-18	0	-18	-18
Central Office Labor Costs	0	0	-8	0	-8	-8
Other Costs (Direct and Indirect)	0	0	-5	0	-5	-5
Sum of Measured Benefits and Costs	-312	0	30	-94	-64	-376
Nonmonetary Factors						
Psychological Benefits of More Rapid Reemployment	+					+
Burden of Reporting Requirements, Reduced Leisure Time, and Costs from Working	-					-

NOTE: Row or column sums may not add to totals due to rounding.

TABLE IX.3

BENEFIT-COST ANALYSIS OF TREATMENT 2
(LOW BONUS OFFER, LONG QUALIFICATION PERIOD)
(Dollars per Claimant)

	Perspective					
			Government			
Benefits and Costs	Claimant	Employer	UI Trust Fund	Other Government	Government Total	Society
Market Output and Wages						
Increased Output	0	246	0	0	0	246
Wages and Fringe Benefits	246	-246	0	0	0	0
Tax Payments						
Claimants' Taxes	-57	0	2	54	57	0
Income Support Payments						
UI Payments	-69	0	69	0	69	0
Other Payments	0	0	0	0	0	0
Administrative Costs of Income Support Programs						
UI Payment Administration	0	0	1	0	1	1
Administration of Other Programs	0	0	0	0	0	0
Demonstration Costs						
Reemployment Bonuses	60	0	-60	0	-60	0
Local Office Labor Costs	0	0	-18	0	-18	-18
Central Office Labor Costs	0	0	-9	0	-9	-9
Other Costs (Direct and Indirect)	0	0	-5	0	-5	-5
Sum of Measured Benefits and Costs	180	0	-19	54	35	215
Nonmonetary Factors						
Psychological Benefits of More Rapid Reemployment	+					+
Burden of Reporting Requirements, Reduced Leisure Time, and Costs from Working	-					-

NOTE: Row or column sums may not add to totals due to rounding.

TABLE IX.4

BENEFIT-COST ANALYSIS OF TREATMENT 3
(HIGH BONUS AMOUNT, SHORT QUALIFICATION PERIOD)
(Dollars per Claimant)

	Perspective					
			Government			
Benefits and Costs	Claimant	Employer	UI Trust Fund	Other Government	Government Total	Society
Market Output and Wages						
Increased Output	0	166	0	0	0	166
Wages and Fringe Benefits	166	-166	0	0	0	0
Tax Payments						
Claimants' Taxes	-39	0	2	37	39	0
Income Support Payments						
UI Payments	-99	0	99	0	99	0
Other Payments	0	0	0	0	0	0
Administrative Costs of Income Support Programs						
UI Payment Administration	0	0	1	0	1	1
Administration of Other Programs	0	0	0	0	0	0
Demonstration Costs						
Reemployment Bonuses	95	0	-95	0	-95	0
Local Office Labor Costs	0	0	-18	0	-18	-18
Central Office Labor Costs	0	0	-9	0	-9	-9
Other Costs (Direct and Indirect)	0	0	-5	0	-5	-5
Sum of Measured Benefits and Costs	123	0	-25	37	12	135
Nonmonetary Factors						
Psychological Benefits of More Rapid Reemployment	+					+
Burden of Reporting Requirements, Reduced Leisure Time, and Costs from Working	-					-

NOTE: Row or column sums may not add to totals due to rounding.

TABLE IX.5

BENEFIT-COST ANALYSIS OF TREATMENT 4
(HIGH BONUS OFFER, LONG QUALIFICATION PERIOD)
(Dollars per Claimant)

	Perspective					
			Government			
Benefits and Costs	Claimant	Employer	UI Trust Fund	Other Government	Government Total	Society
Market Output and Wages						
Increased Output	0	204	0	0	0	204
Wages and Fringe Benefits	204	-204	0	0	0	0
Tax Payments						
Claimants' Taxes	-54	0	2	52	54	0
Income Support Payments						
UI Payments	-130	0	130	0	130	0
Other Payments	0	0	0	0	0	0
Administrative Costs of Income Support Programs						
UI Payment Administration	0	0	1	0	1	1
Administration of Other Programs	0	0	0	0	0	0
Demonstration Costs						
Reemployment Bonuses	151	0	-151	0	-151	0
Local Office Labor Costs	0	0	-18	0	-18	-18
Central Office Labor Costs	0	0	-11	0	-11	-11
Other Costs (Direct and indirect)	0	0	-5	0	-5	-5
Sum of Measured Benefits and Costs	171	0	-51	52	0	172
Nonmonetary Factors						
Psychological Benefits of More Rapid Reemployment	+					+
Burden of Reporting Requirements, Reduced Leisure Time, and Costs from Working	-					-

NOTE: Row or column sums may not add to totals due to rounding.

TABLE IX.6

BENEFIT-COST ANALYSIS OF TREATMENT 5
(DECLINING BONUS OFFER, LONG QUALIFICATION PERIOD)
(Dollars per Claimant)

	Perspective					
			Government			
Benefits and Costs	Claimant	Employer	UI Trust Fund	Other Government	Government Total	Society
Market Output and Wages						
Increased Output	0	47	0	0	0	47
Wages and Fringe Benefits	47	-47	0	0	0	0
Tax Payments						
Claimants' Taxes	-18	0	1	17	18	0
Income Support Payments						
UI Payments	-61	0	61	0	61	0
Other Payments	0	0	0	0	0	0
Administrative Costs of Income Support Programs						
UI Payment Administration	0	0	1	0	1	1
Administration of Other Programs	0	0	0	0	0	0
Demonstration Costs						
Reemployment Bonuses	89	0	-89	0	-89	0
Local Office Labor Costs	0	0	-18	0	-18	-18
Central Office Labor Costs	0	0	-9	0	-9	-9
Other Costs (Direct and Indirect)	0	0	-5	0	-5	-5
Sum of Measured Benefits and Costs	57	0	-58	17	-41	17
Nonmonetary Factors						
Psychological Benefits of More Rapid Reemployment	+					+
Burden of Reporting Requirements, Reduced Leisure Time, and Costs from Working	-					-

NOTE: Row or column sums may not add to totals due to rounding.

date. This increase was due primarily to a $246 increase in earnings and fringe benefits in response to the bonus offer, as shown in Table IX.3.

The finding that the bonus offers yielded net benefits for claimants is not surprising because participation in the program was purely voluntary--claimants chose whether to accept a job during the period in which they qualified for a bonus. We would expect that claimants would not respond to the bonus offer in a way that would make themselves worse off than they would be in the absence of the bonus offer.

Only the least generous bonus offer, treatment 1, proved to be a cost to claimants. Treatment 1 generated a net loss because claimants who received the bonus offer also received lower earnings over the year following the benefit application date. The net loss implies that members of treatment group 1 responded to the bonus offer by making decisions that made them worse off than they would have been in the absence of the bonus offer. While such an outcome is possible, it is inconsistent with common sense and with the findings for the other treatments. We thus view the benefit-cost results for treatment 1 with considerable skepticism, and we ignore treatment 1 in much of the discussion in the remainder of this chapter.

The bonus offers generally yielded modest losses from the perspective of the UI trust fund. These losses occurred because the costs associated with administering the bonus program and paying bonuses outweighed the savings in UI compensation that were generated because the bonus offers induced rapid reemployment. Most of the bonus offers cost the UI trust funds between $20 and $60 per claimant, with the more generous bonus offers tending to be more expensive.

From the perspective of the government as a whole, the findings in Table IX.1 suggest that three of the bonus offers (treatments 2 to 4) paid for themselves. The low amount, long duration bonus offer (treatment 2) yielded estimated net benefits to the government of $35 per claimant. For treatments 3 and 4, the per-claimant benefits were either equal to per-claimant costs or exceeded per-

claimant costs by a small amount. Conversely, the declining bonus offer (treatment 5) proved to be a cost to the government.

All of the bonus offers except for treatment 1 yielded net benefits to society. For three of the bonus offers (treatments 2, 3, and 4) net benefits well exceeded $100 per claimant. The declining bonus offer (treatment 5) yielded net benefits of $17 per claimant.

D. THE SENSITIVITY OF THE BENEFIT-COST FINDINGS TO ALTERNATIVE ESTIMATES OF UI AND EARNINGS IMPACTS

The benefit-cost findings presented in the preceding section were based on estimates of the impacts of the treatments on UI receipt and earnings that were subject to statistical uncertainty. In light of the uncertainty of these estimated impacts, we tested the sensitivity of the benefit-cost findings for alternative estimates of UI and earnings impacts. In this section, we summarize the results of these sensitivity tests by presenting net benefit measures based on four alternative scenarios of impact estimates.[8]

In the first scenario, we assumed that the displacement of claimants who did not receive a bonus offer was an important effect of the demonstration. Displacement occurred if, by inducing treatment-group members to become reemployed more quickly, the bonus offers also reduced the number of job vacancies available to other unemployed workers (including control-group members). If the existence of the bonus offers increased the average unemployment duration of the control group, measuring the impact of the program by comparing the average UI receipt and earnings of the treatment and control groups would overstate the true effect of the bonus offer on the UI and earnings outcomes for the treatment groups. In this case, part of the estimated impacts of the

[8]For two reasons, we did not test the sensitivity of the benefit-cost findings to variations in the cost estimates. First, administrative costs were relatively low, and thus even relatively large variations in these costs would have had only small effects on the net benefit calculations. Second, as we discussed in Section B, changes in the assumptic .s that underlie the estimates yielded only small variations in the cost estimates.

treatments represents a shift of unemployment from the treatment groups to the control group. The true net treatment impacts are thus smaller than our estimates.

We adjusted for displacement in our first sensitivity scenario by assuming that the displacement effect was equal to half of the estimated impacts of the treatments on UI benefits and earnings. We refer to this displacement effect as the "50 percent displacement." Although we have no evidence that 50 percent displacement actually occurred in the demonstration, we used this rate because we believe that it represents a reasonable and convenient number for our sensitivity calculations.

The second and third scenarios presented in this section are based on changes in the impacts of the treatments on earnings only. In the second scenario, we assumed that the treatments had no impact on earnings. Although the assumption of no earnings impacts may be unreasonable given the significant impacts on UI receipt, the scenario based on this assumption provides useful insights into the benefits and costs of the bonus offers. In the third scenario, we used earnings estimates that were based on the data from the interviews rather than on data from the UI wage records. Interview-based earnings estimates are larger than the records-based estimates (and are significant in more cases), as we demonstrated in Chapter VIII, and the differences have a substantial effect on the benefit-cost findings.

The fourth scenario is based on an expansion of the bonus payments that were made for each treatment. We tested the effect of expanding the bonus payments on the benefit-cost findings because, as demonstrated in Chapter VI, many claimants who appeared to be eligible for a bonus did not file a bonus claim. The costs of making bonus payments would thus be lower than they would have been had all bonus-eligible claimants filed a bonus claim. In an ongoing program, claimants might be more familiar with the reemployment bonus and thus more likely to claim a bonus if they

are eligible. In our fourth scenario, we expanded the costs of the bonus payments based on the assumption that all claimants who appeared to be eligible received a bonus.[9]

1. Scenario 1: 50 Percent Displacement

If we assume that displacement occurred to the extent that the net impacts of the treatments on UI benefits and earnings were only half the magnitude of our estimates, then the net benefits of the treatments were lower from the perspectives of the UI trust fund and the government as a whole. Table IX.7 shows that the treatments yielded net losses of at least $50 per claimant for the UI trust funds; these losses are substantially larger than the losses found for the baseline estimates in Table IX.1. The net losses incurred by the government as a whole were slightly lower than the losses incurred by the UI trust funds.

Table IX.7 also shows that, relative to the baseline estimates, a 50 percent displacement effect generally reduced the net benefits of each treatment to claimants and to society as a whole. From the perspective of claimants, displacement reduced net benefits because it implied lower earnings and higher UI benefits, and the loss in earnings exceeded the gain in benefits under the scenario used in Table IX.7. However, based on figures in Table IX.7, four of the five treatments still yielded positive net benefits to claimants, even under the assumption of a 50 percent displacement effect. From the perspective of society, displacement tended to reduce net benefits because it reduced the impact of the treatments on earnings, but the treatments still yielded positive net benefits for three of the five treatments.

2. Scenario 2: No Treatment Impacts on Earnings

Although it is probably unrealistic to assume that the bonus offers did not have an impact on earnings given that they reduced UI receipt significantly, the calculation of net benefits based on the

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[9]In our calculations, we assumed that claimants who appeared to be eligible but did not claim a bonus would have received an average bonus amount equal to the bonus amount received by claimants who did claim one.

TABLE IX.7

ESTIMATED NET BENEFITS OF EACH TREATMENT BY PERSPECTIVE,
ASSUMING A 50 PERCENT DISPLACEMENT EFFECT
(Dollars per Claimant)

	Perspective					
			Government			
Treatment	Claimant	Employer	UI Trust Fund	Other Government	Government Total	Society
1 - Low bonus, short qualification period	-141	0	-19	-43	-62	-203
2 - Low bonus, long qualification period	113	0	-55	34	-21	92
3 - High bonus, short qualification period	98	0	-75	29	-45	52
4 - High bonus, long qualification period	143	0	-117	43	-73	70
5 - Initially high but declining bonus, long qualification period	62	0	-88	19	-69	-7

NOTE: The calculation of net benefits assumes that 50 percent of the UI impacts and the earnings impacts represent the displacement of nontreatment claimants. Hence, the net impacts used in this table are equal to half of the impacts used for the baseline estimates presented in Table IX.1.

assumption of no earnings impacts is useful for at least two reasons. First, it allows us to isolate the effects of the UI-related impacts on the costs and benefits from each perspective. Second, it demonstrates that the treatments must have a positive impact on the earnings-related impacts to be cost-effective from the government perspective and the social perspective.

Assuming that the treatments did not have an impact on earnings, the treatments yielded lower net benefits than the baseline findings from all perspectives. As shown in Table IX.8, most of the treatments in this scenario yielded net losses to both the UI trust funds and the government as a whole. The size of these net losses was modest, usually about $50 or less. Both claimants and society also failed to realize net gains from the demonstration. Claimants generally broke even under this scenario, as shown in Table IX.8. This finding suggests that when claimants were offered a reemployment bonus they gave up average UI benefits approximately equal to the average bonus payment that they received. The treatments generated net losses of about $30 per claimant to society, an amount which was approximately equal to the administrative costs of the demonstration.

3. Scenario 3: Earnings Impacts Based on Interview Data

In our baseline benefit-cost estimates presented in Table IX.1, we used estimates based on UI wage records to measure the impact of the bonus offers on earnings. However, as we discussed in Chapter IX, we can use the interview-based data to calculate alternative estimates of the earnings impacts. These alternative estimates changed our benefit-cost findings because the earnings impacts implied by the interviews were substantially greater than the impacts implied by the wage records. Although we prefer to use the records-based estimates to calculate benefit-cost findings, the interview-based estimates provide a useful alternative for testing the sensitivity of the benefit-cost findings to variations in the earnings outcomes.

With the interview-based earnings impacts, net benefits were higher from all perspectives (Table IX.9). Although most of the treatments still yielded a net loss from the perspective of the UI trust

TABLE IX.8

ESTIMATED NET BENEFITS OF EACH TREATMENT BY PERSPECTIVE,
ASSUMING THAT IMPACTS ON EARNINGS ARE ZERO
(Dollars per Claimant)

	Perspective					
			Government			
Treatment	Claimant	Employer	UI Trust Fund	Other Government	Government Total	Society
1 - Low bonus, short qualification period	-49	0	34	-15	19	-30
2 - Low bonus, long qualification period	-7	0	-22	-2	-24	-31
3 - High bonus, short qualification period	-3	0	-27	-1	-28	-31
4 - High bonus, long qualification period	16	0	-53	5	-49	-33
5 - Initially high but declining bonus, long qualification period	21	0	-58	6	-52	-31

TABLE IX.9

ESTIMATED NET BENEFITS OF EACH TREATMENT BY PERSPECTIVE,
BASED ON INTERVIEW DATA ON EARNINGS IMPACTS
(Dollars per Claimant)

	Perspective					
			Government			
Treatment	Claimant	Employer	UI Trust Fund	Other Government	Government Total	Society
1 - Low bonus, short qualification period	347	0	39	105	144	491
2 - Low bonus, long qualification period	487	0	-15	147	132	619
3 - High bonus, short qualification period	217	0	-24	65	42	259
4 - High bonus, long qualification period	454	0	-47	137	90	543
5 - Initially high but declining bonus, long qualification period	351	0	-54	106	52	404

funds, these losses were lower because the higher earnings impacts translated into higher tax revenues. Because the government as a whole realized a substantial gain in tax revenue from the larger earnings impacts, each of the bonus offers generated net benefits to the government under this scenario. Finally, both claimants and society received larger net benefits from the treatments in this scenario than are indicated by the baseline estimates.

4. Scenario 4: All Eligible Claimants Receive a Bonus

Expanding the sample of claimants who receive a bonus raises the costs of the bonus program, and in this scenario the bonus offers generate larger losses for the UI trust funds than were generated in the baseline estimates. The losses to the UI trust funds in this scenario, which are shown in Table IX.10, are about twice the size of the losses in the baseline estimates. The bonus offers also generally yield modest losses for the government as a whole relative to the baseline findings, which showed modest losses from some treatments and modest gains from others.

The claimants receive greater benefits from the treatments in this scenario than in the baseline findings because they receive higher bonus payments. However, the scenario does not change the estimated impacts on the net benefits to society as a whole, because the expanded bonus payments simply represent a redistribution from the UI trust funds to claimants.

5. The Implications of the Sensitivity Tests

These sensitivity tests suggest that the bonus offers generally prompted claimants to reduce their average UI receipt by an amount approximately equal to the average bonus payment that they received. If the bonus offers also enabled claimants to receive higher earnings, they would receive positive net benefits from the bonus program based on the magnitude of the earnings impacts. If the earnings impacts were large, as implied by the interview-based earnings data, then the net benefits to claimants would be substantial, as shown in Table IX.9. Even if the displacement of nontreatment

TABLE IX.10

ESTIMATED NET BENEFITS OF EACH TREATMENT BY PERSPECTIVE,
BASED ON THE ASSUMPTION THAT ALL ELIGIBLE CLAIMANTS RECEIVE A BONUS
(Dollars per Claimant)

	Perspective					
			Government			
Treatment	Claimant	Employer	UI Trust Fund	Other Government	Government Total	Society
1 - Low bonus, short qualification period	-294	0	7	-89	-82	-376
2 - Low bonus, long qualification period	214	0	-63	64	1	215
3 - High bonus, short qualification period	150	0	-60	45	-15	135
4 - High bonus, long qualification period	215	0	-108	65	-43	172
5 - Initially high but declining bonus, long qualification period	107	0	-122	32	-90	17

claimants by treatment-group members were substantial, claimants would receive net benefits if the bonus offers had a moderately positive impact on earnings, as was shown in Table IX.7.

Net benefits to the UI trust funds depended primarily on the size of the treatment impacts on UI receipt and bonus payments. Given the baseline estimates of the treatment impacts on UI receipt, the UI trust funds realized a net loss approximately equal to the administrative costs, on average. That is, claimants responded to the bonus offers by giving up average benefits equal to the average bonus payment that they received, as discussed in the previous paragraph. Thus, the baseline estimates imply that the UI trust funds simply traded bonus payments for benefits. However, if all claimants who appeared to be eligible claimed and received a bonus offer, as in scenario 4, the losses to the UI trust funds generated by the bonus offers would have been about twice as large. If displacement occurred, as in scenario 1, it would have expanded the net loss incurred by the UI trust funds because it would have reduced the impacts of the treatments on UI receipt. In this case, the UI trust funds would pay out more in bonus payments than it saved in UI benefits, because part of the savings in UI benefits to treatment-group members would have been paid out to displaced nontreatment claimants. On the other hand, variations in earnings impacts had only minor effects on the net benefits to the UI trust funds, as shown in Tables IX.8 and IX.9.

Net benefits to the government as a whole were affected by variations in earnings impacts, variations in UI impacts, and variations in the rate of bonus receipt by eligible claimants. The baseline estimates showed that, on average, the government broke even in response to the bonus offers. If displacement occurred, if earnings impacts were equal to zero, or if all bonus-eligible claimants received a bonus, the government would incur net losses of less than $100 per claimant. If, on the other hand, impacts on earnings were substantial, as implied by the interview data, then the government would realize positive net benefits from the bonus offers due to an increase in tax revenue.

Finally, net benefits to society were affected by both variations in earnings impacts and variations in UI impacts, but they were particularly sensitive to the earnings impacts. As shown in Table IX.8, if the bonus offers had no impact on earnings, society would incur a net loss approximately equal to the administrative costs of the bonus offers. The estimated net benefits to society expanded greatly in response to greater earnings impacts, as shown in Table IX.9. Society realized net benefits from greater earnings both because claimants received higher earnings and because the government received greater tax revenue based on the higher earnings.

X. CONCLUSION

The Pennsylvania Reemployment Bonus Demonstration was designed to inform UI policy development by testing the effects of alternative experimental reemployment bonuses on various outcomes. The findings from the Pennsylvania demonstration show that reemployment bonuses can be successfully integrated into the existing UI system and can affect the UI receipt and reemployment behavior of claimants. In this chapter, we discuss the findings from the demonstration and consider the implications of these findings for UI policy. In Section A, we summarize the main findings on both the impacts and operations of the Pennsylvania demonstration. In Section B, we discuss the factors that may affect the replicability of the findings of the Pennsylvania demonstration to an ongoing program. In Section C, we compare the findings of the Pennsylvania demonstration with the findings of other bonus demonstrations in an effort to synthesize the existing evidence on the effects of reemployment bonuses.

A. SUMMARY OF THE FINDINGS

The bonus offers tested in the Pennsylvania demonstration significantly reduced UI receipt during the benefit year. The bonus offers affected a broad group of claimants; the estimated impacts on UI receipt did not vary significantly among most subgroups of claimants. In addition, the estimated impacts on UI receipt were generally larger for the more generous bonus offers. These findings suggest that, as a UI policy intervention, reemployment bonuses can significantly influence the amount of benefits that claimants receive. Thus, to some extent, the availability of a reemployment bonus can compensate for the reemployment disincentives inherent in the UI system, which pays benefits to claimants based on their being unemployed.

Presumably, the bonus offers reduced UI receipt by inducing claimants to become reemployed more quickly. Our estimates do provide some evidence that the bonus offers increased the postapplication employment and earnings of claimants assigned to the treatments. Data from UI

wage records show that the treatments had no impact on employment, but they had a generally positive but statistically insignificant impact on earnings. The interview data provide somewhat stronger evidence that the treatments increased employment and earnings. The interview-based estimates demonstrate that the treatments had a positive but insignificant effect on employment. However, the interview-based estimates show that the treatments increased postapplication earnings significantly, primarily by increasing full-time employment.

Our benefit-cost analysis based on the impact estimates demonstrates that the bonus offers generally yielded net benefits to claimants and to society as a whole. Claimants responded to the bonus offers by giving up benefits that, on average, were equal to the bonus payment that they received. Consequently, because the bonus offers tended to increase the employment and earnings of claimants, they received net benefits from the bonus demonstration. Society also received net benefits from the demonstration, because the administrative costs of the demonstration were relatively low.

Although the bonus offers significantly reduced UI receipt, they were not cost-effective from the perspective of the UI system. The costs of administering and paying reemployment bonuses in the Pennsylvania demonstration generally exceeded the bonus-induced reduction in benefits. The bonus offers thus generated net losses for the UI trust fund. Despite the net losses, some of the treatments generated positive net benefits for the government as a whole, and the government either broke even or incurred a modest loss for the other treatments.

The experience in implementing the demonstration shows clearly that reemployment bonuses can be implemented successfully as part of the existing UI system. In the demonstration, UI staff were able to screen claimants, gather the required information, make bonus offers to eligible claimants, process bonus claims, and pay bonuses.

The only component of the demonstration that was problematic was the voluntary job-search workshop. Participation in the job-search component of the demonstration was extremely low,

rendering it ineffective in contributing to the work search of demonstration participants. However, the low rate of participation was due to a lack of interest among claimants, rather than to operational problems. The low unemployment rate in the state and the fact that the bonus was offered early in a claimant's unemployment spell may have contributed to the lack of interest in the job-search workshop.

B. THE REPLICABILITY OF THE PENNSYLVANIA DEMONSTRATION FINDINGS

A number of factors in the demonstration probably contributed to its impacts. Some of these factors, such as local economic conditions, are outside the control of program administrators and might not exist in an ongoing bonus program. Other factors involve operational elements that could be controlled by administrators in an ongoing program. We discuss both types of factors in this section.

First, the economy in Pennsylvania during the demonstration was strong and dynamic. The unemployment rate for the state was a low 4.5 percent, and employment in the demonstration sites was rising during the demonstration. In most sites, the overall employment growth reflected substantial employment growth in the service and trade sectors that outweighed modest employment declines in manufacturing. Bonus offers could have different impacts in economic environments that differed from the economic environment of the demonstration.

Second, a greater percentage of eligible claimants may receive a bonus offer in an ongoing program. In the Pennsylvania demonstration, approximately 6 percent of the claimants who were assigned to a treatment group never received a bonus offer. Most of these claimants exited the UI system quickly and thus might have been eligible to receive the bonus had they been offered one. In an ongoing bonus program, we would expect that all eligible claimants would be offered a bonus offer, including the types of individuals who were missed in the Pennsylvania demonstration. While making offers to these claimants would probably not have affected the estimated UI impacts, since

these claimants did not collect a substantial amount of benefits, it might increase the rate of bonus receipt and thus the costs of the bonus program.

Third, some of the claimants who received a bonus offer and stopped collecting UI within the bonus qualification period did not claim a bonus. Presumably, a high proportion of these claimants would claim a bonus in an ongoing program, in which the bonus offers would be part of the regular UI system rather than an experimental component. If many of these claimants did collect a bonus in an ongoing program, then the costs of an ongoing program would be higher than the costs of the Pennsylvania demonstration. The findings presented in Chapter VI suggest that bonus payments might have been as much as 50 percent higher if all claimants who appeared to eligible for bonuses received bonuses.[1]

Fourth, displacement might prevent any positive impacts on net benefits from occurring in an ongoing program. As described in Chapter IX, displacement might have occurred in the demonstration if the bonus-induced reduction in UI receipt was offset by an increase in unemployment and UI receipt for claimants who did not receive a bonus offer. However, in an ongoing program, a greater proportion of UI claimants would receive a bonus offer. These claimants may compete for a limited number of job vacancies, precluding any reduction in UI receipt. Another possibility in an ongoing program is that claimants would reduce UI receipt by displacing nonclaimants from job vacancies. Any of these possibilities would effect the benefit-cost impacts of an ongoing program.

Finally, our evaluation of the bonus-offer process demonstrates that the bonus offers were understood and taken seriously by claimants. Several tools used in the demonstration (including a script for the verbal bonus offers and additional written materials to reiterate the details of the bonus offer) were instrumental in ensuring that claimants understood the bonus offer. Similar tools would be required in an ongoing program to ensure that claimants understand the bonus offer.

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[1]This estimate includes all individuals who appeared eligible for a bonus regardless of whether or not they received a bonus offer.

C. COMPARISON WITH OTHER REEMPLOYMENT BONUS DEMONSTRATIONS

In addition to the Pennsylvania Reemployment Bonus Demonstration, three other reemployment bonus demonstrations were conducted in recent years. These three bonus demonstrations, conducted in Illinois, New Jersey, and Washington, provide an opportunity to place the findings of the Pennsylvania demonstration in a more general policy context.

Our comparison of the findings of the bonus demonstrations focuses on two major issues--the design characteristics of the demonstrations, and the impacts of each demonstration. Table X.1 summarizes the factors that we use to compare the demonstrations. The findings of the other three bonus demonstrations support our conclusion from the Pennsylvania demonstration that a reemployment bonus can be implemented successfully as part of the existing UI system, and that the bonus offer can significantly affect UI receipt. These findings also generally demonstrate that the bonus offers that were tested in the demonstrations yielded net benefits to claimants and to society as a whole, but were not cost-effective from the perspective of the UI system.

1. The Characteristics of the Bonus Demonstrations

The four bonus demonstrations differed primarily along two dimensions. First, the demonstrations served different populations of UI claimants, due both to different eligibility restrictions and to differences in the timing of the offers. Second, the demonstrations tested different bonus amounts and qualification periods, and some of the demonstrations tied the bonus offer to job-search assistance.

a. The Population Who Received a Bonus Offer

Two factors affected the population who received a bonus offer in each of the bonus demonstrations--the eligibility criteria and the timing of the bonus offer. In terms of eligibility criteria, the Pennsylvania and Illinois demonstrations targeted regular UI claimants who did not have a strong attachment to their pre-unemployment employer. These two demonstrations attempted to

TABLE X.1

COMPARISON OF REEMPLOYMENT BONUS DEMONSTRATIONS

	Location of Bonus Demonstration (Years of Operation)			
	Illinois (1984)	New Jersey (1986-87)	Washington (1988)	Pennsylvania (1988-89)
Characteristics				
Eligibility Criteria	• Initial claims only • Eligible for 26 weeks of UI benefits • At least 20 years old but younger than 55 • Registered with Job Service (claimant was to register with the Job Service prior to the final processing of benefit application, unless the claimant had a definite recall date or claimant was a union member who finds jobs through a union hiring hall)	• Initial claims only • Received a first payment under the regular state UI program • No more than 4 weeks between initial claim and the first payment • Not receiving partial payments because of earnings • At least 25 years old • Had worked with pre-UI employer for at least 3 years before applying for UI, and worked full-time only for one employer during that 3-year period • Did not have a definite recall date • Did not use a union hiring hall to secure employment	• Initial claims only • Eligible to receive benefits from the state UI trust fund • Monetarily valid claims at the time of filing	• Initial claims only • Regular UI claimants • Initially satisfied monetary eligibility conditions • Did not accept employment exclusively through a union • Did not have a specific recall date within 60 days of benefit application • Was not separated from job due to a labor dispute • Signed for a waiting week or first payment within 6 weeks of benefit application date
Bonus Structure and Qualification Period	$500 paid to a claimant who found a job within 11 weeks of the initial offer. The bonus amount represented approximately 4 times the average weekly benefit amount.	Initial bonus offer equal to half of a claimant's remaining UI entitlement (average initial offer = $1,644). The bonus amount then declined by 10 percent of the initial offer each week, falling to zero by the end of the 11th full week of the bonus offer.	Six constant-amount bonus offers tested: 1. amount = 2 x WBA; qual. period = .2 x maximum benefit duration + 1 week 2. amount = 4 x WBA; qual. period = .2 x maximum benefit duration + 1 week 3. amount = 6 x WBA; qual. period = .2 x maximum benefit duration + 1 week 4. amount = 2 x WBA; qual. period = .4 x maximum benefit duration + 1 week 5. amount = 4 x WBA; qual. period = .4 x maximum benefit duration + 1 week 6. amount = 6 x WBA; qual. period = .4 x maximum benefit duration + 1 week	Five constant-amount and one declining-amount bonus offers tested: 1. amount = 3 x WBA; qual. period = 6 weeks; 2. amount = 3 x WBA; qual. period = 12 weeks 3. amount = 6 x WBA; qual. period = 6 weeks 4. amount = 6 x WBA; qual. period = 12 weeks 5. initial amount = 6 x WBA, and the amount declined by 10 percent each week after the first two weeks of the offer. 6. same as 4.

TABLE A.1 (continued)

	Location of Bonus Demonstration (Years of Operation)			
	Illinois (1984)	New Jersey (1986-87)	Washington (1988)	Pennsylvania (1988-89)
Additional Services Offered	None	Mandatory job-search assistance	None	Voluntary job-search workshop
Delivery of Bonus Offer	Offer made at the time claimants registered with the Job Service.	Offer made at the assessment interview, which was part of the job-search assistance package and typically occurred at seven or eight weeks after claimants filed their initial claim.	Offer made at the time claimants filed for benefits.	Offer made to claimants when they signed for a waiting week or first payment.
Findings				
Bonus Receipt Rates	13.6 percent of full sample of claimants who received a bonus offer	18.5 percent of those who were assessed and received a bonus offer	As a proportion of claimants assigned to each treatment group: Treatment: 1. 8.7 percent 2. 12.4 percent 3. 15.0 percent 4. 13.9 percent 5. 17.8 percent 6. 22.0 percent Total: 14.6 percent	As a proportion of claimants assigned to each treatment group: Treatment: 1. 6.9 percent 2. 10.7 percent 3. 8.3 percent 4. 13.2 percent 5. 10.0 percent 6. 13.9 percent Total: 10.5 percent
Impact on UI Receipt	-1.15*** weeks of insured unemployment in the benefit year -$158*** regular UI benefits paid in the benefit year	Combination of bonus offer and mandatory job-search assistance: -.97*** weeks of benefits paid in the benefit year; -$170*** paid in the benefit year. Marginal impact of the bonus offer for claimants who were offered the bonus: -.69*** weeks paid in the benefit year; -$101** paid in the benefit year.	Impacts on UI receipt in the benefit year: Treatment: 1. -.04 weeks, $19 compensation 2. -.27 weeks, -$41 comp. 3. -.70** weeks, -$107** comp. 4. -.62** weeks, -$117** comp. 5. -.26 weeks, -$40 comp. 6. -.75** weeks, -$141** comp.	Impacts on UI receipt in the benefit year: Treatment: 1. -.65## weeks, -$103## compensation 2. -.36# weeks, -$69# comp. 3. -.44# weeks, -$99## comp. 4. -.82### weeks, -$130### comp. 5. -.33 weeks, -$61 comp.

TABLE X.1 (continued)

	Location of Bonus Demonstration (Years of Operation)			
	Illinois (1984)	New Jersey (1986-87)	Washington (1988)	Pennsylvania (1988-89)
Impact on Employment and Earnings	NA	Estimates based on interview data suggest that the combination of bonus offer and mandatory job-search assistance increased employment and earnings significantly in the first two quarters after benefit application: Impacts per Quarter[b]: Earnings / Proportion of Time Employed Q1: $160*** / 2.8%*** Q2: $278*** / 5.0%*** Q3: $131 / 2.3% Q4: $22 / 0.6% Estimates based on the interview data suggest that the bonus offer had only a small marginal impact on employment and earnings for claimants who received the offer: Impacts per Quarter: Earnings / Proportion of Time Employed Q1: $58* / 0.7% Q2: $90 / 1.4% Q3: -$22 / -1.8% Q4: -$58 / -2.8%	NA	Overall: Generally the treatments did not have a significant impact on post-application employment or earnings when wage records were used to measure the outcomes. When outcomes were measured with the interview responses, the treatments had a positive but insignificant impact on employment, but the impacts on earnings were substantial and significant in some cases.

TABLE X.1 (continued)

	Location of Bonus Demonstration (Years of Operation)			
	Illinois (1984)	New Jersey (1986-87)	Washington (1988)	Pennsylvania (1988-89)
Benefit-Cost Findings	Net benefits per claimant = $90 (calculated as the treatment response minus the bonus cost per claimant)	For the combination of the bonus offer and mandatory job-search assistance: Net benefits from: • The perspective of society = $565 • The pers. of the Labor Dept. programs = -$99 • The perspective of the govt. as a whole = $55 These estimates were similar to those found for the treatment that included only the job-search assistance. However, adding the bonus offer generated slightly higher costs from the Labor Department and total government perspectives.	Net benefits from society perspective: Treatment: 1. -$6 per claimant 2. $90 3. $266 4. $308 5. $88 6. $349 Net benefits from UI system perspective: Treatment: 1. -$51 per claimant 2. -$42 3. -$38 4. $68 5. -$77 6. -$77 Net benefits from total government perspective: Treatment: 1. -$51 2. -$28 3. $2 4. $115 5. -$63 6. -$24	Net benefits from society perspective: Treatment: 1.-$376 per claimant 2. $215 3. $135 4. $172 5. $17 Net benefits from UI system perspective: Treatment: 1. $30 per claimant 2. -$19 3. -$25 4. -$51 5. -$58 Net benefits from total government perspective: Treatment: 1. -$64 per claimant 2. $35 3. $12 4. $0 5. -$41

SOURCE: Illinois: Woodbury and Spiegelman (1987); Spiegelman and Woodbury (1987).
New Jersey: Corson et al. (1989).
Washington: Spiegelman, O'Leary, and Kline (1991).

[a]Estimates based on the combined treatments 4 and 6.

[b]Quarters are defined relative to benefit application date.

NA = not available.

TABLE X.1 (continued)

*Statistically significant at the 90 percent confidence level in a two-tail test.
**Statistically significant at the 95 percent confidence level in a two-tail test.
***Statistically significant at the 99 percent confidence level in a two-tail test.

#Significantly less than zero at the 90 percent confidence level in a one-tail test.
##Significantly less than zero at the 95 percent confidence level in a one-tail test.
###Significantly less than zero at the 99 percent confidence level in a one-tail test.

achieve this objective by excluding claimants who had a specific recall date and claimants who accepted employment exclusively through a union. The Pennsylvania demonstration imposed these criteria directly, while the Illinois demonstration imposed similar criteria by offering the bonus only to claimants who were required to report to the Job Service. Since claimants in Illinois who had a definite recall date or used a union hiring hall were not required to report to the Job Service, they were effectively excluded from the demonstration.

The New Jersey demonstration was more exclusive than the Pennsylvania and Illinois demonstrations, since it targeted services to dislocated workers. To achieve this targeting objective, the demonstration imposed an additional eligibility requirement that participants have at least three years of tenure with their pre-unemployment employer. The addition of this eligibility requirement to other criteria that were similar to those applied in Pennsylvania and Illinois made the New Jersey demonstration the most exclusive of the four bonus demonstrations.

The Washington demonstration was less exclusive than the Pennsylvania and Illinois demonstrations because it offered reemployment bonuses to as broad a group of UI recipients as possible. Thus, the recall and union-hiring eligibility restrictions that were used in the Pennsylvania and Illinois demonstrations were not used in the Washington demonstration.

The timing of the bonus delivery also varied across the bonus demonstrations. Both the Pennsylvania and the Illinois demonstrations attempted to deliver the bonus offers shortly after a claimant's initial contact with the UI system. In the Pennsylvania demonstration, claimants were offered the bonus when they filed for the waiting week or first payment; in Illinois, claimants were offered the bonus when they reported to the Job Service, shortly after they applied for benefits.

In contrast, claimants in the New Jersey demonstration received the bonus offer at the assessment interview, which typically occurred seven or eight weeks after their initial claim. Hence, many claimants who were eligible for the demonstration never received a bonus offer in New Jersey because they returned to work before their assessment interview. The Pennsylvania and Illinois

demonstrations served a broader group of claimants than the New Jersey demonstration because the Pennsylvania and Illinois offers occurred earlier in the UI spell, before most claimants exited the UI system.

The Washington demonstration made the bonus offers when claimants applied for benefits, rather than waiting until claimants signed for a waiting week or reported to Job Service, as in the Pennsylvania and Illinois demonstrations. Hence, some of the claimants who received bonus offers in Washington would not have received offers according to the Pennsylvania and Illinois designs.

b. Bonus Design

The Pennsylvania demonstration was similar to the Washington demonstration in that it tested a variety of alternative bonus offers. In both demonstrations, the alternative bonus offers that were tested differed primarily by the amount of the bonus offer and the duration of the bonus qualification period. As shown in Table X.1, the Washington demonstration tested six different bonus offers based on three alternative bonus amounts that were tied to the claimant's weekly benefit amount, and two alternative qualification periods that were tied to the claimant's maximum benefit duration. The Pennsylvania demonstration tested similar variations in bonus amounts and qualification periods, as shown in Table X.1.[2] Some of the individual treatments in the two demonstrations were almost identical--for example, treatments 3 and 6 in the Washington demonstration were nearly identical to treatments 3 and 4 in the Pennsylvania demonstration, respectively. The Pennsylvania demonstration also tested the impact of two additional design elements. First, it tested the impact of making the amount of a bonus offer declining rather than constant over the qualification period, in an attempt

[2]The qualification periods for the Pennsylvania treatments were constant, rather than tied to a maximum benefit duration, as in Washington. However, over 98 percent of claimants in Pennsylvania had the same maximum benefit duration of 26 weeks, and the constant qualification periods thus represented a constant proportion of maximum benefit durations for the majority of claimants.

to hasten the reemployment of relatively short-term claimants. Second, it attempted to test the impact of linking a voluntary job-search workshop to the bonus offer.[3]

Each of the bonus demonstrations in Illinois and New Jersey tested only a single bonus offer. The structure of the bonus offers in these two demonstrations differed substantially. In Illinois, the bonus offer consisted of a constant bonus amount, $500 (about four times the average weekly benefit amount), which was paid to claimants who became reemployed within 11 weeks after the initial offer. This bonus offer was similar to treatment 2 in the Pennsylvania demonstration, in which the average bonus amount was equal to about $500 and lasted for 12 weeks. No other employment services were tied to the bonus offer in the Illinois demonstration. In contrast, the bonus offer in New Jersey declined over time (similar to treatment 5 in the Pennsylvania demonstration) and was tied to a *mandatory* job-search assistance package. The amount of the initial offer in New Jersey was equal to half of the remaining UI entitlement at the time of the offer, and the amount of the offer then declined over an 11-week qualification period.

2. Findings

The findings of the Pennsylvania demonstration support the findings of the other three bonus demonstrations. For example, the bonus receipt rates for the Pennsylvania treatments were similar to those found in the other three demonstrations. The proportion of treatment-group members who received a bonus in the Pennsylvania demonstration ranged from about 7 percent for the least generous bonus offer (treatment 1) to nearly 14 percent for the most generous bonus offers (treatments 4 and 6). Bonus receipt rates in the Washington demonstration were similar on average and were also higher for the most generous offers. Overall, 10.5 percent of claimants in the Pennsylvania demonstration received a bonus, compared with 14.6 percent of the claimants in the Washington demonstration. The receipt rate for the single bonus offer in the Illinois demonstration

[3]As discussed previously, the lack of participation in the job-search workshop precluded estimating the impacts of the workshop.

(13.6 percent) also falls in this range. The receipt rate for the New Jersey demonstration was slightly higher, at 18.5 percent.

The estimated impacts of the bonus offers on UI receipt in the Pennsylvania demonstration support the findings of the other three demonstrations that bonus offers reduced UI receipt significantly, and that the magnitude of the reduction depended on the parameters of the offers. As in the Pennsylvania demonstration, the bonus offers in the Washington demonstration generally reduced UI receipt, and the reductions were statistically significant for some of the individual treatments. In addition, the more generous offers generally caused the greatest reductions in UI receipt, as occurred in the Pennsylvania demonstration.

The bonus offers in New Jersey and Illinois also significantly reduced UI receipt. However, the impact of the Illinois bonus was somewhat higher than might be expected given the estimates in the other three demonstrations. The Illinois bonus amount of $500 was fairly modest, yet the bonus offer had the greatest impact of any of the offers tested in the four bonus demonstrations, reducing UI receipt by more than a full week during the benefit year. None of the bonus offers in the Pennsylvania or Washington demonstration, even the most generous offers, was able to replicate an impact of this magnitude. This inability to replicate the Illinois finding makes us skeptical about using the Illinois finding to formulate expectations about its impact for an ongoing bonus program.

The estimates based on the Pennsylvania demonstration show that the bonus offers probably increased postapplication employment and earnings for claimants, but that the impacts were small. Only one of the other three bonus demonstrations, the New Jersey demonstration, tested the impact of the bonus offers on employment and earnings among the full sample of claimants.[4] In the New Jersey demonstration, the bonus offer tied to mandatory job-search assistance had a significant impact on both employment and earnings in the first two quarters following benefit application, as shown

[4]The evaluations of the Illinois and Washington demonstrations tested whether the bonus offers had any impact on earnings among claimants who found reemployment, but they did not test the impacts on the employment and earnings of all claimants.

in Table X.1. However, the impacts of the bonus offer on employment and earnings beyond the impacts of the mandatory job-search assistance were relatively modest and generally insignificant. This finding is consistent with the small estimated impacts on employment and earnings found in the Pennsylvania demonstration.

Based on the impact estimates and the costs of the demonstration, four of the five bonus offers in the Pennsylvania demonstration generated net losses for the UI trust fund. This finding is relatively consistent with the findings from the other bonus demonstrations. Two of the other demonstrations, those in Washington and New Jersey, included bonus offers that also generated net losses for the UI trust fund. Only the bonus offer in Illinois generated positive net benefits for the UI trust fund. However, this finding depends on the large reduction in UI receipt found in the Illinois demonstration, which was relatively inconsistent with the UI reductions in the other demonstrations.[5]

Despite the net losses incurred by the UI trust fund for the Pennsylvania bonus offers, some of the bonus offers generated modest but positive net benefits for the government as a whole, as shown in Table X.1. This finding is consistent with the results for the Washington demonstration, in which some of the bonus offers that were tested also yielded positive net benefits for the government as a whole. The findings from the Pennsylvania and Washington demonstrations do not provide a clear indication about the type of bonus offers that generated net benefits for the government. However, the high-amount short-duration offers in each of the demonstrations (treatment 3) yielded positive net benefits to the government. In the New Jersey demonstration, the effect of the single bonus offer increased the costs from the perspective of the government as a whole.

Finally, the Pennsylvania demonstration estimates support findings from the other demonstrations that the bonus offers generally yielded net benefits to society. Three of the five Pennsylvania bonus

[5]The net benefit calculation for the UI trust fund in Illinois did not include the costs of administering the bonus program, but the estimated impact of the Illinois bonus offer on UI receipt was so large that the bonus offer would still be cost-effective for the UI trust fund even after administrative costs were accounted for.

offers yielded net benefits of greater than $100 per claimant. In the Washington demonstration, five of the six bonus offers that were tested generated net benefits of greater than $100 per claimant for society. In the New Jersey demonstration, the bonus offer had approximately zero effect on the net benefits to society. These findings suggest that society breaks even or receives net benefits from a reemployment bonus program.

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APPENDIX A

SURVEY RESULTS AND IMPLICATIONS OF NONRESPONSE

The evaluation of the Pennsylvania Reemployment Bonus Demonstration was based on administrative records for the full analysis sample and survey data collected from a subset of this sample that oversampled treatment group members who applied for a bonus. Overall, 5,201 out of 14,086 analysis sample members were interviewed, including 1,000 treatment group members who applied for a bonus, 2,578 treatment group members who did not apply for a bonus, and 1,623 control group members. The interview was administered over a one-year period such that each individual was interviewed 12 to 13 months after they had initially applied for UI. The interview took approximately 20 minutes and was conducted by telephone.

The remainder of this appendix summarizes the results of the survey, examines the characteristics of the respondent and nonrespondent, and examines the potential implications of nonresponse.

A. SURVEY RESULTS

The results of the survey are reported in Table A.1. Overall, we attempted to interview 6,532 members of the analysis sample and completed interviews with 5,201, an 80 percent response rate. As would be expected, this response rate was highest (90 percent) among individuals who applied for a bonus. The response rate was virtually the same for the other two groups in the sample--78 percent for non-bonus treatment group members and 77 percent for control group members.[1]

The main reason for nonresponse (61 percent) was that individuals could not be located. Refusals accounted for another 26 percent of nonresponses, and cases that were retired from the sample after multiple unsuccessful attempts accounted for most of the remaining nonresponses.

[1]Within these three subgroups, survey targets were established to ensure that the distribution of the survey sample by month of intake and by treatment group was the same as the distribution of the entire sample.

TABLE A.1

RESULTS OF SURVEY, BY TREATMENT STATUS AND BONUS RECEIPT
(Analysis Sample Members Only)

	Treatments							
	Bonus		Non-bonus		Controls		Total	
Interview Outcome	Number	Percent	Number	Percent	Number	Percent	Number	Percent
Interview Completed	1,000	89.8	2,578	77.6	1,623	77.4	5,201	79.6
Partial Interview	0	0.0	2	0.0	0	0.0	2	0.0
Sample Member Not Located	62	5.6	462	13.9	291	13.9	815	12.5
Respondent Refused Interview	38	3.4	182	5.5	125	6.0	345	5.3
Sample Member Screened from Sample	0	0.0	1	0.0	1	0.0	2	0
Language Barrier/Physical Impairment	0	0.0	1	0.0	1	0.0	2	0
Sample Member Cannot Be Interviewed	0	0.0	5	0.2	2	0.1	7	0.1
Sample Member Deceased	2	0.2	14	0.4	7	0.3	23	0.4
Case Retired After Multiple Unsuccessful Attempts	9	0.8	60	1.8	36	1.7	105	1.6
Other	2	0.2	17	0.5	11	0.5	30	0.5
Total Nonrespondents	113	10.2	744	22.4	474	22.6	1,331	20.4
Total Sample Release	1,113	100.0	3,322	100.0	2,097	100.0	6,532	100.0

B. THE CHARACTERISTICS OF NONRESPONDENTS

The characteristics of survey respondents and nonrespondents are reported in Table A.2.[2] Respondents were more likely to be female, more likely to be white, and less likely to be younger than age 35. Respondents also had higher weekly benefit amount than did nonrespondents. These differences, which were found for both treatment and control group members were statistically significant.[3] None of these differences is particularly surprising, since the group least likely to be interviewed--young black males--is thought to be highly mobile and thus more difficult to locate and interview.

C. IMPLICATIONS OF NONRESPONSE

Nonresponse may have three impacts. First, it could affect estimates of the characteristics of the treatment or control populations. However, despite the differences between respondents and nonrespondents, nonresponse had little effect on these estimates, since the overall response rate was quite high (80 percent). For example, the proportion of female treatment group members is 43 percent based on data for respondents only and 41 percent based on data for the entire sample (respondents and nonrespondents). Other variables for which we have data on both respondents and nonrespondents are also quite similar.

Second, nonresponse could affect estimates of the *differences* between treatments and controls, but this does not seem to be a problem. The characteristics of treatment and control respondents are quite similar for the variables that we can measure (Table A.2).

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[2]The nonrespondents were weighted so that their distribution by sample category (bonus applicants, other treatment group members, and control group members) was the same as the distribution of respondents, in order to ensure that differences in response rates by category would not affect our comparisons between respondents and nonrespondents.

[3]We used a two-tail test at the 95 percent confidence level to determine statistical significance.

TABLE A.2

CHARACTERISTICS OF THE INTERVIEW SAMPLE BY TREATMENT AND SURVEY RESPONSE STATUS
(Analysis Sample Members Only)

	Treatments		Controls	
	Respondents	Nonrespondents	Respondents	Nonrespondents
Demographic Variables				
Gender				
Percent female	42.9 *	33.9	42.8 *	28.4
Race/Ethnicity				
Percent white	87.0 *	69.9	88.1 *	65.7
Percent black	9.9 *	20.5	9.5 *	22.8
Percent Hispanic	2.5 *	8.6	1.8 *	11.0
Percent other race	0.6	1.0	0.6	0.5
Age				
Percent younger than 35	53.3 *	61.7	52.1 *	59.5
Percent 35 to 54	35.5 *	29.7	35.8	32.8
Percent older than 54	11.2 *	8.6	12.1 *	7.7
Mean Base Period Earnings (in thousands of dollars)	14.5	13.7	14.6 *	12.3
UI Entitlement				
Mean weekly benefit amount	167 *	161	166 *	154
Percent 16-week potential duration	0.7 *	2.0	1.1	2.1
Sample Size	3,578	857	1,623	474

*Difference between respondents and nonrespondents significant at the 95 percent confidence level in a two-tail test.

Finally, nonresponse might affect our estimates of the *impacts* of the demonstration (Table A.3). Again, respondents and nonrespondents differ; respondents generally had longer periods of UI collection than did nonrespondents. As above, the differences between respondents and nonrespondents are similar for treatment and control group members. However, the estimated impacts on the treatment group are larger if we use respondents rather than the full sample. For example, the impact of the bonus offer was to reduce weeks of UI payments by one week among interview respondents, (15.9 for treatments and 14.9 weeks for controls) while there was no difference in weeks collected among treatment and control nonrespondents.[4] Because the interview response rate was high, these differences in the estimated impacts do not have a major effect on the overall impact estimates (in this case, the estimated reduction in UI weeks is .8 weeks over the entire sample). However, the differences suggest that individuals who did not respond to the interview probably did not respond to the bonus offer either. Thus, any estimates based solely on interview data probably overstate program impacts to some degree.

In summary, this analysis suggests that nonresponse to the survey does not pose a problem for estimates of the characteristics of treatment or control group members, but that estimates of treatment impacts that rely on interview data may be overstated. For this reason, we emphasize the results obtained from records data whenever possible.

[4]The treatment-control differences for respondents were statistically significant except for the difference in the exhaustion rate. None of the differences observed for controls was statistically significant.

TABLE A.3

UNEMPLOYMENT INSURANCE OUTCOMES BY TREATMENT AND SURVEY RESPONSE STATUS

	Treatments		Controls	
	Respondents	Nonrespondents	Respondents	Nonrespondents
Mean Number of Payments Collected	14.9 *	12.7	15.9 *	12.7
Initial UI Duration (Weeks)	12.4 *	11.4	13.1 *	11.3
Mean Dollars of UI Collected	2,418 *	2,050	2,533 *	2,027
Percent Exhaustees	28.5	26.0	28.8	27.2
Sample Size	3,578	857	1,623	474

*Difference between respondents and nonrespondents significant at the 95 percent confidence level in a two-tail test.

APPENDIX B

IMPACTS ON THE INITIAL SPELL OF UNEMPLOYMENT

The objective of the reemployment bonus was to encourage claimants to become reemployed more quickly. In Chapter VIII we used quarterly employment outcomes to investigate the impacts of the bonus offers on the employment of UI claimants. A more direct way to test whether the bonus offers hastened reemployment is to measure their impact on the duration of claimants' initial unemployment spells. In this appendix, we estimate these impacts on unemployment duration.

The findings presented in this appendix demonstrate that, as expected, the treatments reduced the duration of the initial unemployment spell, although the estimated reductions were not statistically significant for most treatments.[1] In addition, the treatments increased the rate of reemployment during the bonus qualification periods, and these increases were statistically significant. Our findings also demonstrate that the bonus offers reduced the duration of unemployment among short-term unemployed claimants but not among longer-term unemployed claimants.

A. IMPACTS ON THE DURATION OF INITIAL UNEMPLOYMENT SPELLS

We define the duration of the initial unemployment spell as the number of weeks from the benefit application date to the reemployment date provided in the interview.[2] A potential problem with estimating the impacts of the bonus offers on the duration of unemployment spells is that the end of the spell cannot be observed for some claimants because they were still unemployed at the time of the interview. This *censoring* of the unemployment spell may bias the estimated impacts of

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[1]Because most claimants return to work and stop receiving benefits at the same time, we expected that the impacts on the initial unemployment spell were similar to the impacts on the initial spell of UI receipt, which were presented in Chapter VII. However, estimates of unemployment spells are likely to be less precise than estimates of UI spells. The maximum initial UI spell was 26 weeks, and all claimants who exhausted their benefits would have had initial UI spells of 26 weeks, even though their spell of unemployment was often longer than 26 weeks. On the other hand, our interview data allow us to measure unemployment spells that were substantially longer than 26 weeks. Because the unemployment spells were not bounded at 26 weeks, the variance of the unemployment spells was likely to be greater than the variance of the UI spells, making significant findings less likely to occur.

[2]We used the benefit application date as the starting point of the unemployment spell, rather than the date on which the pre-unemployment job ended, because the end date of the pre-unemployment job was subject to greater measurement error. However, we also used this alternative unemployment starting-point measure to derive estimates, and found that they were similar to the findings presented in this appendix.

the treatments on lengths of spells, particularly if the censoring is more severe for the control group than for the treatment groups. In estimating the impacts on the initial unemployment spell, we attempted to minimize the impact of censoring by using the natural logarithm of the observed unemployment spell as the dependent variable. Using the logarithmic form of the dependent variable minimized the effect of censoring on the estimates, because censoring occurred primarily with relatively long spells.[3]

Our estimates provide relatively weak evidence that the treatments reduced the duration of the initial unemployment spell. Table B.1 shows that all treatments had a negative impact on the duration of unemployment, but only one of the impacts, the impact for treatment 1, was significantly less than zero at the 90 percent confidence level. The estimate for treatment 1 implies that the bonus offer reduced the initial unemployment spell by nearly 9 percent relative to the initial unemployment spell experienced by control group members. This impact represents about 1.25 weeks of the initial unemployment spell, based on the control-group mean of 13.9 weeks of unemployment. Treatments 2, 3, and 5 reduced unemployment duration by between 4 and 6 percent of the unemployment duration of control group members, but none of these estimates was significantly less than zero at the 90 percent confidence level.[4] Finally, treatment 4 had no impact on unemployment

[3]In our sample, respondents whose spells were censored had experienced at least 47 weeks of unemployment, and they had been unemployed for an average of 55 weeks at the time of the interview. In terms of the logarithmic measure, treating these censored spells as if they had ended at the time of the interview probably created only a relatively small error. For example, suppose that a respondent whose spell was incomplete at 55 weeks had returned to work in 75 weeks. In logarithmic terms, treating the 55-week incomplete spell as if it were complete would understate the true spell only by 7 percent, compared with 36 percent if the spell were not measured in logarithmic terms.

[4]The insignificance of these findings is not surprising, given the relatively high variance of the length of the initial unemployment spell. As we explained in footnote 1, we expected that the variance of the initial unemployment spell would be greater than the variance of the initial UI spell. We compared the variances of the two measures by using the coefficient of variation (the variance divided by the mean) for each measure, which was roughly twice as large for the unemployment spell as for the UI spell. A higher variance for the dependent variables implies that the standard errors for the impact estimates were also larger, meaning that we were less likely to find a significant impact on the unemployment spell than on the UI spell, even though the impact estimates were similar in magnitude.

TABLE B.1

ESTIMATED IMPACTS OF THE TREATMENTS ON INITIAL UNEMPLOYMENT DURATION
(Standard Errors in Parentheses)

Treatment	Dependent Variable: Log of Weeks from Benefit Application Date to Reemployment
1 - Low bonus, short qualification period	-.088 *
	(.056)
2 - Low bonus, long qualification period	-.046
	(.045)
3 - High bonus, short qualification period	-.051
	(.050)
4 - High bonus, long qualification period	-.001
	(.042)
5 - Initially high but declining bonus, long qualification period	-.051
	(.050)
All Treatments Combined	-.040
	(.032)

NOTE: The sample includes 5,061 claimants for whom we have data on both demographic characteristics and unemployment duration. The explanatory variables used in the model include treatment indicators, cohort indicators, office indicators, and demographic and economic variables.

*Significantly less than zero at the 90 percent confidence level in a one-tail test.
**Significantly less than zero at the 95 percent confidence level in a one-tail test.
***Significantly less than zero at the 99 percent confidence level in a one-tail test.

duration. This finding is surprising, because treatment 4 had the largest impact on weeks of UI receipt, as shown in Chapter VII. But the finding is consistent with the estimated impact of treatment 4 on weeks of employment presented in Table VIII.2, which showed that treatment 4 had no impact on weeks of employment over the postapplication period.

B. IMPACTS ON REEMPLOYMENT RATES

In addition to investigating how the treatments affected the duration of the initial unemployment spell, we also investigated how the treatments affected rates of reemployment at various points in time. To evaluate reemployment rates, we estimated the impacts of the treatments on *conditional* reemployment rates for eight separate five-week periods following benefit application. The conditional rate is the proportion of claimants who were not employed prior to the five-week period that became reemployed during the five-week period. The five-week periods, which are specified in Table B.2, were chosen for convenience. The simple differences in reemployment rates between the treatment groups and the control group can be interpreted as the effect of the treatments on the probability of reemployment among claimants who were still unemployed at the beginning of the period.

The bonus offers generally increased the conditional reemployment rate during the periods in which treatment-group members were eligible for a reemployment bonus. As shown in Table B.2, eight of the ten estimated impacts on reemployment rates in the first two periods were greater than zero, although none of the impacts was statistically significant. Most claimants in the treatment groups were eligible for a reemployment bonus during at least part of these two periods, which together included the first 10 weeks following the benefit application date. For two of the bonus offers, treatments 2 and 4, claimants were still eligible to receive a bonus in the 11- to 15-week period. Table B.2 shows that both of these treatments increased the reemployment rate for the 11- to 15-week period by more than 5 percentage points, and the impacts were statistically significant at

TABLE B.2

ESTIMATED IMPACTS OF THE TREATMENTS ON CONDITIONAL REEMPLOYMENT RATES
(Standard Errors in Parentheses)

Length of Initial Unemployment Spell, in Weeks	Proportion of Control-Group Members Reemployed (Percent)	Treatment Impacts on Reemployment Rate (Percent)					
		1 Low Bonus, Short Qualification Period	2 Low Bonus, Long Qualification Period	3 High Bonus, Short Qualification Period	4 High Bonus, Long Qualification Period	5 Declining Bonus, Long Qualification Period	All Treatments Combined
0-15	24.4	-0.4	3.0	1.4	-0.4	0.3	0.8
		(2.3)	(1.9)	(2.0)	(1.7)	(2.1)	(1.3)
6-10	19.5	2.9	0.0	2.7	1.1	2.9	1.7
		(2.5)	(2.1)	(2.3)	(1.9)	(2.3)	(1.4)
11-15	18.1	2.8	5.3**	-0.3	6.3***	0.4	3.4**
		(2.9)	(2.4)	(2.6)	(2.2)	(2.6)	(1.7)
16-20	18.0	6.8**	-3.1	-2.6	-1.8	-1.5	-1.0
		(3.2)	(2.6)	(2.8)	(2.4)	(2.8)	(1.8)
21-25	17.3	-1.3	-2.6	1.8	1.1	-1.8	-0.4
		(3.6)	(2.9)	(3.1)	(2.7)	(3.1)	(2.0)
26-30	22.3	-0.7	-5.8*	-3.4	-6.1*	-1.0	-4.0*
		(4.2)	(3.3)	(3.6)	(3.1)	(3.6)	(1.3)
31-35	15.5	-3.1	-1.4	-1.8	-2.4	-1.4	-2.0
		(4.3)	(3.3)	(3.7)	(3.1)	(3.7)	(2.3)
36-40	14.2	6.0	-0.1	-0.7	1.9	-1.0	0.9
		(4.7)	(3.7)	(4.0)	(3.5)	(4.1)	(2.6)

NOTE: The estimates are based on the difference in conditional reemployment rates between the treatment groups and the control group in each period. The conditional reemployment rate for a period is the number of claimants who became reemployed during the period as a percentage of claimants who were still unemployed at the beginning of the period. Unemployment spells that were censored prior to a period were also excluded from the calculation of the reemployment rate for that period. The initial sample includes 5,126 claimants for whom we have employment data from the follow-up interview.

*Statistically significant at the 90 percent confidence level in a two-tail test.
**Statistically significant at the 95 percent confidence level in a two-tail test.
***Statistically significant at the 99 percent confidence level in a two-tail test.

the 95 percent confidence level. The estimated impacts of the other treatments were insignificant during the 11- to 15-week period.

Beyond week 15, the treatments generally had a negative impact on the conditional reemployment rate, although most of the estimates were not statistically significant. The largest negative impacts occurred for treatments 2 and 4, which reduced the conditional reemployment rate during the 26- to 30-week period by more than 5 percentage points. The other treatments had smaller and statistically insignificant negative impacts on the reemployment rate during the 26- to 30-week period. In other periods, the estimates tended to be negative but statistically insignificant for all of the treatments.

The negative estimates for the treatment impacts in the periods following week 15 are consistent with the "catching up" phenomenon described in our discussion of UI exit rates in Chapter VII. The negative estimates suggest that the *cumulative* reemployment rate of the control group "caught up" to the cumulative reemployment rate of the treatment groups following the bonus qualification periods.

We examined this "catching up" phenomenon directly by estimating the impacts of the treatments on the cumulative reemployment rate at the end of each five-week interval. The estimates, presented in Table B.3, show that the impacts of the treatments on the cumulative reemployment rate reached a maximum at week 15 for the treatments with long qualification periods (treatments 2, 4, and 5). This finding is consistent with the design of the long-duration bonus offers, because for most of the claimants who received a bonus offer the qualification period ended at some point shortly before week 15. Hence, the maximum impact occurred near the end of the bonus qualification period. A similar result occurred for treatment 3, a short-duration bonus offer, which achieved its maximum impact on the cumulative reemployment rate at week 10. Only the treatment 1 impacts were inconsistent with the bonus design. This short-duration bonus offer had its greatest impact on

TABLE B.3

ESTIMATED IMPACTS OF THE TREATMENTS ON CUMULATIVE REEMPLOYMENT RATES
(Standard Errors in Parentheses)

Weeks	Proportion of Control Group Members Reemployed (Percent)	Treatment Impacts on Reemployment Rate (Percent)					
		1 Low Bonus, Short Qualification Period	2 Low Bonus, Long Qualification Period	3 High Bonus, Short Qualification Period	4 High Bonus, Long Qualification Period	5 Declining Bonus, Long Qualification Period	All Treatments Combined
5	24.9	-0.7	2.4*	1.3	-0.6	0.4	0.6
		(2.3)	(1.9)	(2.0)	(1.7)	(2.1)	(1.3)
10	39.9	2.3	1.7	3.0*	0.1	2.8	1.7
		(2.6)	(2.1)	(2.3)	(2.0)	(2.3)	(1.5)
15	51.1	4.0*	4.3**	2.1	2.6*	3.3*	3.2**
		(2.6)	(2.1)	(2.3)	(2.0)	(2.4)	(1.5)
20	60.3	6.9***	2.1	0.4	0.4	2.3	2.0*
		(2.6)	(2.1)	(2.3)	(1.9)	(2.3)	(1.4)
25	67.5	5.6**	0.8	1.0	0.7	1.7	1.6
		(2.4)	(2.0)	(2.1)	(1.8)	(2.2)	(1.4)
30	75.1	4.1**	-1.3	-0.4	-1.2	1.3	0.1
		(2.3)	(1.8)	(2.0)	(1.7)	(2.0)	(1.3)
35	79.3	2.6	-1.5	-0.8	-1.5	0.8	-0.5
		(2.1)	(1.7)	(1.9)	(1.6)	(1.9)	(1.2)
40	82.5	3.4**	-1.3	-1.1	-1.0	0.4	-0.3
		(2.0)	(1.6)	(1.8)	(1.5)	(1.8)	(1.1)

NOTE: The estimates are based on linear probability models of the cumulative reemployment rate. The explanatory variables used in the model include treatment indicators, cohort indicators, office indicators, and demographic variables. The sample includes 5,061 claimants for whom we have both interview data on employment and records data on the explanatory variables.

*Statistically significant at the 90 percent confidence level in a one-tail test.
**Statistically significant at the 95 percent confidence level in a one-tail test.
***Statistically significant at the 99 percent confidence level in a one-tail test.

cumulative reemployment at week 20, a point in time long after the end of the bonus qualification period.

After week 15, the impacts of the treatments on the cumulative reemployment rate tended to decay, as shown in Table B.3. The impact of the combined treatments on the reemployment rate decayed to zero by week 30. Similar patterns occurred for the impacts of individual treatments, except for the treatment 1 impact, which did not decay as fully.

The decay of the impacts on the cumulative reemployment rate does not imply that the treatments had no impact on reemployment. Rather, it supports the conclusion presented in Chapter VII that the effect of the treatments was concentrated among claimants who faced relatively short potential unemployment spells. The treatments did not appear to have had much of an impact on the unemployment spells of long-term unemployed claimants (those whose initial potential unemployment spells were longer than 15 weeks).

APPENDIX C

POTENTIAL DISPLACEMENT OF CLAIMANTS WHO DID NOT RECEIVE A BONUS OFFER

In the final report, we derived impacts of the bonus offers in the Pennsylvania Reemployment Bonus Demonstration by comparing the outcomes of treatment group members with the outcomes of control group members. This estimation methodology implicitly assumes that the displacement of claimants who did not receive a bonus offer was not an important factor in the demonstration. However, displacement would occur if, by inducing treatment-group members to search for work and become reemployed more quickly, the bonus offers also reduced the number of job vacancies available to other unemployed workers (including control group members), thus constraining the rapidity of job-finding among these workers. If the existence of bonus offers increased the duration of unemployment among the control group, then a part of the estimated treatment impact would represent a shift of UI receipt from the treatment group to the control group. Thus, if displacement occurred, the true net treatment impacts on UI receipt would be smaller than the estimates presented in Chapter VII of this report.

In this appendix we examine evidence of displacement in the PRB demonstration. We first construct a conceptual framework for measuring displacement in situations like the PRB demonstration, where many UI offices did not participate in the demonstration and thus would serve as control sites. Despite the convenience of the PRB demonstration design for calculating a displacement effect, the statistical power for detecting a significant displacement effect can be shown to be extremely limited. As expected, the estimate of the displacement effect that we derived was highly imprecise and thus not statistically significant.

A. ESTIMATING DISPLACEMENT EFFECTS

A natural measure of the effect of the reemployment bonus on weeks of UI receipt is the difference between average weeks of UI receipt of the treatment group and the control group. However, this gross treatment effect fails to account for displacement, and hence cannot distinguish adequately between net treatment effects and displacement effects. Because the bonus program may affect the UI receipt of control group members, it is natural to measure displacement by comparing

UI receipt for the control group to UI receipt for a comparable group of claimants in an external site where the bonus program is not operating. The difference between average weeks of UI receipt for the control group and average weeks of UI receipt for the external group is a measure of the amount by which the bonus program has, on average, increased (that is, displaced) UI receipt for control group members.

To use this information correctly for calculating the net treatment effect, we must convert the measure of displacement which is expressed in terms of an increase in average UI receipt for control group members to a measure of displacement which is expressed in terms of a reduction in average UI receipt for treatment group members. This conversion can be done by multiplying the difference in average UI receipt between the control group and the external group by the ratio of the total internal population to the treatment group. This conversion takes account of the fact that the treatment and control groups differ in size. For example, if the treatment group includes a small proportion of the local UI claimant population (say 10 percent), a relatively small difference in UI receipt (say 0.1 week) between the control group and the external group would translate into a relatively large displacement effect as measured per treatment group member (in this case one week of UI receipt).[1]

To derive an estimate of displacement for the Pennsylvania Reemployment Bonus Demonstration, we defined three groups of claimants--the treatment group, the *internal* control group, and the *external* control group. The internal control group includes *all* UI claimants in the demonstration sites who did not receive a bonus offer, not just those who were explicitly assigned to the demonstration control group. Identifying an internal control group is necessary because displacement probably occurred among all non-treatment group members, not just those who were designated as demonstration control group members. The external control group includes all UI

[1]This displacement effect is calculated as the difference in UI receipt between the control group and the external group (0.1) multiplied by the ratio of the total population to the size of the treatment group (10).

claimants in nondemonstration sites. The treatment group includes all claimants assigned to one of the demonstration treatments.

Based on our discussion of the displacement effect above, the net treatment impact (E) of a bonus offer can be expressed in the following equation:

$$(C.1) \qquad E = (w^t - w^c) - (w^o - w^c)[N/N^t]$$

where w^i is the average weeks of UI receipt among members of the i-th group, t denotes treatment group members, c denotes internal control group members, o denotes external control group members, N^t is the number of treatment group members in the demonstration, and N is the number of persons who are potentially affected by the bonus program. The first term on the right-hand side of equation (C.1) is the simple treatment-control estimate of the treatment impact, and the second term is the *displacement wedge*, which we estimate in this appendix. If no displacement occurred ($w^o = w^c$), our estimator of the net treatment effect is reduced to the simple treatment-control difference estimator.

Specific features of the experimental design of the PRB demonstration lend themselves to the task of measuring displacement. The twelve demonstration sites were selected randomly, one from each of 12 strata defined according to average UI duration and employment security region. According to the formula presented above, displacement in the demonstration can be measured by comparing the UI receipt of non-treatment claimants at sites in which the program operated (the internal control group) with the UI receipt of claimants at sites in which the program did not operate (the external control group).

Despite the convenience of the demonstration design for calculating an estimate of the displacement wedge, the statistical power for detecting a significant displacement wedge is extremely limited. Dynarksi (1990) and Metcalf and Kerachsky (1988) have demonstrated that a sample design based on the demonstration was unlikely to detect even a substantial displacement wedge. For

example, Dynarksi (1990) calculated a minimum detectable displacement wedge of about 6.5 weeks. An estimate of this magnitude would represent an enormous displacement wedge, exceeding 40 percent of average UI receipt in Pennsylvania during the demonstration period. Hence, even large displacement estimates would not likely be statistically significant.

B. THE OVERALL DISPLACEMENT EFFECT

To estimate the displacement effect in the PRB demonstration, we extracted UI payment data for claimants at nearly all UI offices in Pennsylvania during the period of the demonstration.[2] The offices from which we drew these data are shown in Table C.1. In choosing sample members for this displacement file, we attempted to apply the same eligibility criteria that were applied in the demonstration. Our sampling of claimants implicitly assumed that displacement occurred only among UI claimants who met the demonstration eligibility criteria. The demonstration may have also displaced other types of UI claimants, but we ignore their displacement because we expect that these effects play a relatively minor role in the overall displacement measures.[3]

Based on our analysis of data from the displacement file, we estimate that the overall displacement wedge for the PRB demonstration is small and has a sign opposite from expected. We present the calculation of the estimated displacement wedge in Table C.2. The point estimate presented in Table C.2 implies that taking displacement into account would *add* .15 weeks to the magnitude of the estimated impact of the demonstration on weeks of UI receipt. Given that the treatment-control estimate of the average treatment impact in the demonstration was -.55 weeks, the

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[2]Our sample excludes a few small UI offices that were not incorporated into the demonstration sample design.

[3]We also ignored the potential displacement of nonclaimants because we wanted to focus on displacement as measured in terms of UI receipt. The displacement of nonclaimants would not affect UI receipt. However, the displacement of nonclaimants would increase unemployment among nonclaimants. Such an effect could have important implications for estimating the net effect of the demonstration on average employment and earnings.

TABLE C.1

UI OFFICES INCLUDED IN DISPLACEMENT CALCULATIONS, BY SAMPLING STRATUM

Stratum	Demonstration Office	Nondemonstration Offices
1	Phil. - Uptown	Phil. - Downtown Phil.- Frankford Phil. - West
2	Phil. - North	Chester Upper Darby Phil. - Germantown
3	Coatesville	Levittown Norristown Hatboro
4	Pittston	Shamokin State College Hazelton
5	Scranton	Allentown Bethlehem Williamsport
6	Reading	Easton Jim Thorpe
7	Lewistown	Chambersburg Lebanon Berwick Sunbury
8	Lancaster	Carlisle York
9	McKeesport	Ambridge Beaver Falls New Kensington Washington Pitt. - E. Lib.
10	Butler	Greensburg Indiana Kittanning Pitt. - North New Castle

TABLE C.1 (continued)

Stratum	Demonstration Office	Nondemonstration Offices
11	Connellsville	Johnstown Somerset Uniontown Bradford Meadville Oil City
12	Erie	Altoona Bedford Clearfield Huntington Waynesburg Clarion

NOTE: Some small UI offices were excluded from the sample.

TABLE C.2

CALCULATION OF THE DISPLACEMENT WEDGE

Claimant Group	Sample Size	Average Weeks of UI Benefits in Benefit Year[a]
Treatment Group	9,859	14.08
Internal Control Group	40,271	13.39
External Control Group	160,927	13.42

Displacement Wedge = $(w^o - w^c)[N/N_t]$

where w^o = average weeks of UI benefits for external control group members.

w^c = average weeks of UI benefits for internal control group members.

N = the number of claimants affected by the demonstration.

N_t = the number of claimants in the treatment group.

After substituting,

Displacement Wedge = (13.42 - 13.39) [9,859 + 40,271/ 9,859]

= .15 weeks of benefits

NOTE: Analysis sample includes claimants from the displacement file with a benefit application date between 10/15/88 and 10/15/89. Because the displacement sample includes many claimants who were ineligible for the demonstration, we cannot use UI receipt for the treatment and internal control groups to calculate the gross treatment impact. See Section D of this appendix for further discussion of the sampling of ineligible claimants.

[a]To calculate the mean weeks of UI benefits for the internal and external control groups, we weighted their observations so that their distribution over time and across sites reflects the distribution of treatment group members.

magnitude of the displacement wedge represents about one-quarter of the magnitude of the treatment impact.

Despite the counterintuitive sign of the point estimate of the displacement wedge, the estimate was imprecise and thus not statistically significant. Our estimate of the standard error of the displacement wedge estimate is equal to about one week.[4] The size of this standard error implies that our point estimate provides extremely limited information about the actual size of the displacement wedge.

To illustrate this point, consider the 95 percent confidence interval for the estimated displacement wedge, which is between 2.11 and -1.81 weeks. An estimated displacement wedge at either end of this confidence interval represents an enormous effect, whose magnitude is roughly 4 times the size of the treatment impact. Thus, the confidence interval probably includes all reasonable values for the displacement wedge. Hence, we have little confidence that actual displacement was close to our estimate.

C. STRATUM-SPECIFIC DISPLACEMENT

The estimated displacement wedge varies widely across strata of sites, as shown in Table C.3. The estimates range from -5.33 weeks for stratum 9, in which McKeesport is the demonstration site, to 10.20 weeks for stratum 1, in which Philadelphia-Uptown is the demonstration site. This wide variation is not surprising given the small number of sites being compared in each stratum and the associated design effect.

The mean of the stratum-specific wedges is .43 weeks. This number provides an alternative estimate of the overall displacement wedge for the demonstration--an estimate that is about three times larger than the estimate presented in Table C.2. The two estimates differ because $[N/N^t]$ is positively correlated with the displacement effect--the displacement effects in strata that contain

[4]The estimate of the standard error is based on 1986 Pennsylvania UI data. See Dynarski (1990) for discussion of minimum detectable displacement effects based on the same data.

TABLE C.3

STRATUM-SPECIFIC DISPLACEMENT WEDGES

Stratum	Demonstration Office	Number of Internal Control Group Members	Number of Treatment Group Members	Estimated Displacement Effect	Estimated Displacement Wedge
1	Phil.-Uptown	3,733	919	2.01	10.20
2	Phil.-North	4,202	813	-0.67	-4.16
3	Coatesville	3,038	853	1.08	4.91
4	Pittston	1,640	624	-0.44	-1.59
5	Scranton	3,571	838	-0.89	-4.69
6	Reading	4,679	900	0.42	2.59
7	Lewistown	2,500	614	1.38	7.00
8	Lancaster	4,123	880	-0.93	-5.28
9	McKeesport	3,032	896	-1.22	-5.33
10	Butler	2,795	858	0.20	0.84
11	Connellsville	1,169	773	-0.94	-2.36
12	Erie	5,789	891	0.39	2.95

proportionately fewer treatments are likely to be higher. Of course, the large variance associated with the two estimates implies that the difference between the two estimates is not statistically significant.

D. ELIGIBILITY CHARACTERISTICS OF CLAIMANTS IN THE DISPLACEMENT FILE

In the final step of our analysis of displacement, we used a separate comprehensive file containing all UI claimants from the demonstration sites to investigate the selection criteria used for the displacement file. As discussed in Section B, the displacement file was intended to contain all UI claimants in the demonstration or nondemonstration sites who fulfilled the eligibility conditions for the demonstration. However, for the purpose of creating the displacement file, the characteristics determining eligibility were measured at the time the file was created rather than at the time claimants applied for benefits. Because the relevant characteristics for some claimants changed after the benefit application date, some claimants who would have been ineligible at the time of application were treated as eligible when the displacement sample was drawn, and consequently these claimants were included in the displacement file. For example, the expected date of recall was revised for some claimants when they were recalled, making some claimants who were ineligible for the demonstration appear to be eligible. Because we have eligibility data on all UI claimants in the demonstration sites on a separate comprehensive file, we can check some of the claimants from the displacement file to see how many were actually ineligible for the demonstration at the time they applied for benefits.

A large proportion of the individuals included in the displacement file were selected despite being ineligible for the demonstration at the time that they applied for benefits. As shown in Table C.4, the proportion of claimants who were not in the demonstration who were also ineligible was 34 percent. Table C.4 also shows that the proportion of claimants in the displacement file who were ineligible for the demonstration varied widely across strata. The highest proportion of ineligibles was in Pittston, where over 90 percent of the claimants contained in the displacement file were actually

TABLE C.4

PROPORTION OF CLAIMANTS IN THE DISPLACEMENT FILE WHO WERE INELIGIBLE FOR THE DEMONSTRATION

Demonstration Office		Number of Claimants Not Participating in Demonstration	Percentage of Nondemonstration Claimants Found To Be Ineligible
1.	Phil.-Uptown	3,405	3.6
2.	Phil.-North	3,928	2.8
3.	Coatesville	2,784	12.8
4.	Pittston	1,479	90.9
5.	Scranton	3,296	48.3
6.	Reading	4,400	36.8
7.	Lewistown	2,321	94.7
8.	Lancaster	3,873	41.1
9.	McKeesport	2,748	36.4
10.	Butler	2,532	33.3
11.	Connellsville	928	65.0
12.	Erie	5,531	23.5
Total		37,225	34.0

ineligible for the demonstration. For the two Philadelphia sites, less than 5 percent of the claimants contained in the displacement file were ineligible for the demonstration.

Further investigation revealed that most of these claimants who were ineligible for the demonstration originally had a recall date within sixty days of benefit application. Based on discussion with state staff we believe that these claimants were included in the displacement file because they returned to their pre-UI employer *after* 60 days and their recall date was revised accordingly, thereby making them appear eligible for the demonstration. They were then included in the displacement file based on this revised recall date when the file was created.

These findings show that our displacement file includes many ineligible claimants that we originally intended to exclude from the file. The ineligible claimants probably differed systematically from the eligible claimants in terms of UI receipt because the ineligible claimants expected to be recalled quickly to their pre-UI employer. Theoretically, this selection issue should not bias the estimate of the displacement effect because the same type of selection occurred for the internal and external control groups, the two groups which are compared in measuring displacement. However, individual sites varied widely in terms of how many ineligible claimants were selected for the displacement file. Consequently, the stratum-specific estimates, which were based on a small number of sites, may be substantially affected by the variation across sites in eligibility determination. In addition, the internal control group differed systematically from the treatment group, and therefore the difference in UI receipt between the treatment and internal control groups is not a valid estimate of the gross treatment effects on UI receipt.

APPENDIX D

COMPARISON OF INTERVIEW AND WAGE RECORDS DATA ON EARNINGS

We used both UI wage records and interview data on employment and earnings to derive the employment and earnings impacts presented in Chapter VIII. Because UI wage records were available for the full analysis sample, we used them as our primary source of earnings data, and we based our main estimates of employment and earnings impacts on these data. However, we also presented estimates of the impacts of the treatments based on the interview data. As reported in Chapter VIII, the estimates based on the interview data provide slightly stronger evidence that the treatments increased the employment of claimants.

Both data sources have potential shortcomings for measuring earnings. Wage records overlook the earnings of any claimant who became reemployed either outside the state or outside the UI-covered sector (for example, those who became self-employed). Wage records may also misrepresent earnings because earnings are reported when they are received rather than when they are earned. For instance, some individuals in the sample appear to have received sizeable lump-sum amounts of severance pay from their pre-UI employers after their benefit application date. These payments could be misinterpreted as representing earnings from a post-UI job, thus overstating the earnings received by the claimants following their benefit application date.

Interviews also represent an imperfect source of earnings information. Interview data are subject to measurement error due to the faulty recall of employment dates or wage rates by respondents. In addition, nonresponse is a potential problem with interview data, although our analysis of nonresponse in Appendix A suggests that it was not a serious problem for this study.

Previous research revealed significant differences between earnings data from wage records and earnings data from interviews for a sample of claimants in the New Jersey UI Reemployment Demonstration (Decker, 1989b). However, the same research showed that a portion of these differences were explained by three types of compensation that were treated differently in the wage-records and interview data--earnings from out-of-state employment, earnings from self-employment, and severance pay. In addition, Decker (1989b) concluded that the comparison of the earnings data

did not indicate the better data source for estimating the impacts of the treatments on earnings in the New Jersey demonstration.

In this appendix we repeat this investigation by examining the consistency between earnings reported in the wage records and those reported in the interviews, and by attempting to relate the empirical differences to the structural differences between the two data sources. This investigation enables us to determine whether the structural differences between the data sources, including those discussed above, significantly affect measured earnings in a systematic way.

A. COMPARISON OF THE INCIDENCE OF EARNINGS IN THE TWO DATA SOURCES

Our initial step in investigating the consistency of earnings data from the two data sources was to compare the incidence of earnings receipt in each data source for the post-application quarters. Table D.1 shows for each quarter the proportion of the sample for whom either both sources reported positive earnings, both sources reported zero earnings, or one source reported positive earnings but the other reported zero earnings. These calculations are based on the sample of claimants for whom we have both wage-records data and interview data on earnings.

Data on the incidence of earnings in the two sources were consistent in approximately 80 percent of the cases. In the first post-application quarter, the two data sources provided comparable information on the incidence of earnings for 78 percent of the sample--49 percent of the sample had positive earnings in both data sources, and 29 percent had zero earnings in both data sources. For 22 percent of the cases, the incidence of earnings was not consistent in the data sources. For nearly 16 percent of the sample, positive earnings were reported in the wage records but zero earnings were reported in the interview. While some of these cases probably arose because individuals failed to recall employment dates accurately or neglected to report employment in their interview, some may also have arisen because some individuals received severance pay during the first post-application quarter, which would have been counted as earnings in the wage records but not reported as earnings in the interview. Other sample members (6 percent of the sample) reported earnings in the interview

TABLE D.1

INCIDENCE OF EARNINGS RECEIPT BY DATA SOURCE

QUARTER 1[a]

		Interview: Zero Earnings	Interview: Positive Earnings
Wage Records: Zero Earnings		29.2% 1,520 Claimants	6.0% 309 Claimants
Wage Records: Positive Earnings		15.6% 813 Claimants	49.2% 2,556 Claimants

QUARTER 2[a]

		Interview: Zero Earnings	Interview: Positive Earnings
Wage Records: Zero Earnings		20.3% 1,053 Claimants	6. 7% 351 Claimants
Wage Records: Positive Earnings		10.0% 522 Claimants	63.0% 3,272 Claimants

TABLE D.1 (continued)

QUARTER 3[a]

		Interview: Zero Earnings	Interview: Positive Earnings
Wage Records	Zero Earnings	16.3% 832 Claimants	7. 6% 388 Claimants
	Positive Earnings	8.9% 456 Claimants	67.1% 3,425 Claimants

[a]Quarters 1, 2, and 3 are the first, second, and third full calendar quarters following each claimant's benefit application.

but have zero earnings in the wage records for the first quarter. These cases can be explained partly by self-employment or other non-UI-covered employment, or by out-of-state employment, which are not covered in Pennsylvania wage records.

In the second and third quarters the rate of consistency between the two data sources was slightly higher than in the first quarter--approximately 83 percent of the cases. Both data sources reported earnings for nearly two-thirds of all cases. Both sources reported zero earnings for 20 percent of the cases in quarter 2, while both sources reported zero earnings for 16 percent of the cases in quarter 3. The rate at which wage records reported earnings but the interview reported zero earnings fell from 16 percent in quarter 1 to 10 percent in quarter 2, and to 9 percent in quarter 3. One possible explanation for this reduction may be that the incidence of severance pay, which was counted as earnings only in the wage records, declined in the later quarters. The rate at which the interview reported earnings but the wage records reported zero earnings increased slightly in the later quarters, from 6 percent in quarter 1 to about 7 or 8 percent in the later quarters. One possible explanation for this slight increase may be that, over time, more claimants took jobs out of state, where they received earnings that were not reported in the Pennsylvania wage records.

B. COMPARISON OF THE MEAN EARNINGS FROM WAGE RECORDS AND INTERVIEW DATA

To conduct further analysis of the differences in the earnings data from our two data sources, we examined mean earnings for the first three full calendar quarters following each claimant's benefit application date. Table D.2 reports the mean earnings from the two data sets for the three calendar quarters. The statistics presented in Table D.2 show that, on average, the interview reported $51 less in earnings per claimant than was reported in the wage records in the first quarter after benefit application. This difference did not differ significantly from zero at the 90 percent confidence level. For subsequent quarters, the interview earnings exceeded the wage record earnings, with the

TABLE D.2

SAMPLE MEANS OF EARNINGS BY QUARTER
(Standard Deviations in Parentheses)

	Observations	Interview Data Earnings	Wage-Record Data Earnings	Interview Earnings Minus Wage-Records Earnings	Absolute Value of Interview Earning Minus Wage-Record Earning
			Quarter 1[a]		
Full sample	5,199	$1,781 (2,435)	$1,832 (2,917)	-$51 (2,664)	$1,038 ** (2,453)
No Severance Pay	4,568	1,767 (2,324)	1,724 (2,383)	44 (2,013)	907 ** (1,798)
Not Self-Employed	5,072	1,775 (2,398)	1,855 (2,925)	-79 ** (2,616)	1,020 ** (2,410)
PA Area Codes Only	5,061	1,775 (2,407)	1,845 (2,932)	-69 * (2,645)	1,033 ** (2,435)
PA Area Codes, Not Self-Employed, and No Severance Pay	4,354	1,767 (2,407)	1,755 (2,932)	13 (1,966)	897 ** (1,750)
			Quarter 2[a]		
Full Sample	5,199	$2,658 (2,881)	$2,494 (2,676)	$165 *** (2,386)	$1,153 ** (2,095)
No Severance Pay	4,568	2,592 (2,607)	2,470 (2,553)	123 *** (2,060)	1,082 ** (1,757)
Not Self-Employed	5,072	2,633 (2,731)	2,527 (2,671)	107 *** (2,156)	1,109 ** (1,852)
PA Area Codes Only	5,061	2,656 (2,866)	2,506 (2,678)	151 *** (2,342)	1,139 ** (2,052)
PA Area Codes, Not Self-Empoloyed, and No Severance Pay	4,354	2,588 (2,561)	2,508 (2,542)	81 ** (1,966)	1,054 ** (1,661)

TABLE D.2 (continued)

	Observations	Interview Data Earnings	Wage-Record Data Earnings	Interview Earnings Minus Wage-Records Earnings	Absolute Value of Interview Earnings Minus Wage-Record Earnings
			Quarter 3[a]		
Full Sample	5,103	$2,994	$2,766	$218 ***	$1,234 ***
		(2,963)	(2,741)	(2,577)	(2,273)
No Severance Pay	4,482	2,911	2,725	179 ***	1,153 ***
		(2,700)	(2,556)	(2,226)	(1,912)
Not Self-Employed	4,977	2,961	2,801	149 ***	1,181 ***
		(2,786)	(2,702)	(2,329)	(2,013)
PA Area Codes Only	4,965	2,995	2,782	203 ***	1,222 ***
		(2,950)	(2,743)	(2,541)	(2,237)
PA Area Codes, Not Self-Employed, and No Severance Pay	4,268	2,908	2,772	126 ***	1,116 ***
		(2,618)	(2,539)	(2,078)	(1,758)

[a]Quarters 1, 2, and 3 are the first, second, and third full calendar quarters following each claimant's benefit application date.

*Statistically significant at the 90 percent confidence level in a two-tail test.
**Statistically significant at the 95 percent confidence level in a two-tail test.
***Statistically significant at the 99 percent confidence level in a two-tail test.

difference growing from an average of $165 in quarter 2 to $218 in quarter 3. Both of these mean differences were significantly greater than zero at the 90 percent confidence level.

The means of the absolute differences, which are shown in the final column of Table D.2, were greater than $1,000 in all three quarters. These differences imply that an individual's interview-reported earnings, were more than $1,000 less or greater on average than the earnings reported in the wage records. These large absolute differences, together with the much smaller real differences, demonstrate that the two earnings reports exhibit considerable differences that, to a large extent, balance out on average.

To determine the effects of the structural differences between the two data sets on the earnings differences, we investigated the impact of three types of claimants on the earnings differences reported in Table D.2: those who became reemployed outside Pennsylvania after their benefit application date, those who became self-employed after their benefit application date, and those who reported that they received severance pay from their pre-UI employer after their benefit application date. Because the earnings of out-of-state and self-employed workers would not be detected by Pennsylvania wage records, the inclusion of these workers in the sample should generate a positive difference between interview earnings and wage-record earnings. On the other hand, because severance pay is treated as earnings in the wage records when it is received, we expect that the inclusion of workers in the sample who received severance pay after their benefit application date would generate a negative difference between interview earnings and wage-record earnings.

The increase in the mean difference over time, as shown in Table D.2, is consistent with the scenario described above. One would expect that severance pay would more likely be received in the first quarter than in the succeeding two quarters. At the same time, it is likely that fewer claimants found employment outside Pennsylvania or became self-employed in the first quarter as compared to the second or third quarters. Given these two assertions, wage-records earnings should exceed interview earnings in the first quarter, in which severance pay was a relatively important factor. As

time passed, however, interview earnings should have dominated, as severance pay became scarce and more claimants found employment outside Pennsylvania or became self-employed. The actual mean differences shown in Table D.2 fit this pattern, increasing over time, from -$51 in quarter 1 to $218 in quarter 3.

The next step in analyzing the differences between the two data sets was to identify claimants in the sample who found reemployment outside Pennsylvania, who became self-employed, or who received severance pay after their benefit application date. We addressed out-of-state employment by using the respondents' telephone numbers to determine which respondents lived outside Pennsylvania. About 3 percent of the full sample had a non-Pennsylvania area code. This approach constitutes a crude method for isolating out-of-state workers because some of the claimants who lived outside Pennsylvania may have found reemployment within the state, and other claimants' who lived inside Pennsylvania may have become reemployed outside the state. However, the incidence of claimants' crossing the state border to find employment should be small for our sample, because most of the demonstration sites were not near the state border.[1]

Self-employment and severance pay can be dealt with in a more straightforward manner. We treated claimants as self-employed if they reported in the interview that at least one of their post-application jobs entailed self-employment. Similarly, we also used the interview to determine whether claimants received severance pay from their pre-UI employer. Over 12 percent of the sample reported receiving severance pay, while only two percent reported self-employment.

As we expected, the exclusion of either out-of-state claimants or self-employed claimants generally reduced the mean difference between the interview earnings and the wage-records earnings, and brought about greater consistency between the individual earnings reports. The reduction in the earnings difference occurred in all three quarters following the benefit application date, as shown in

[1]This situation contrasts with the situation in the New Jersey UI Reemployment Demonstration, in which, as pointed out by Decker (1989b), the demonstration sites were located in areas in which claimants were likely to cross the state border to find employment.

Table D.2. Excluding out-of-state or self-employed workers also reduced the mean absolute differences and the standard deviations of the real differences, although the reductions were not large. Excluding the self-employed workers appears to have had a slightly larger impact than excluding the out-of-state workers. Overall, these findings reveal that the exclusion of self-employed and out-of-state claimants from the comparison led to greater consistency between the two sources of earnings data, but the impacts were of limited magnitude, since these exclusions affected relatively few cases.

Removing claimants who reported receiving severance pay had the expected impact of increasing the real difference between interview earnings and wage-records earnings in quarter 1, and of reducing the absolute difference over the sample. In later quarters, the effect of removing severance-pay recipients had a negative impact on the real difference between interview earnings and wage-records earnings, contrary to our expectations. Nevertheless, the exclusion of these claimants from the sample brought about greater consistency between the two earnings measures, as demonstrated by the reduction in the absolute differences. As occurred with the other sample exclusions, the magnitude of the impacts of the severance-pay exclusion was, however, relatively small.

Finally, we measured the impact of excluding all three types of claimants from the sample. As shown in Table D.2, these exclusions reduced both the mean real differences and the mean absolute differences between the earnings measures in all quarters. However, the mean real differences for both quarters 2 and 3 were still significantly greater than zero, indicating that systematic differences between the two measures remained even after the exclusions. In addition, after the exclusions, the mean absolute differences were only about 10 percent smaller on average than the mean differences for the full sample. Hence, the exclusions did not substantially increase the consistency of the earnings measures, and systematic differences remained despite the exclusions.[2]

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[2]This finding contrasts somewhat with Decker's (1989b) findings from the evaluation of the New Jersey UI Reemployment Demonstration. Decker showed that after sample exclusions for severance pay, self-employment, and out-of-state employment the mean of the differences between the earnings reports no longer differed significantly from zero in three of the four calendar quarters that were

C. CONCLUSION

The findings presented in this appendix demonstrate that wage-record earnings differed considerably from interview earnings. In terms of the incidence of earnings, nearly 20 percent of the claimants had positive earnings in one data source but zero earnings in the other data source for the first three quarters after the benefit application date. In addition, data on mean earnings receipt demonstrate that some systematic differences exist between the two earnings measures. By controlling for some of the structural differences between the data sources, we were able to generate somewhat greater consistency between the two measures. But none of the sample exclusions that we tested reduced the differences in the earnings measures substantially, and systematic differences between the measures remained even after the exclusions. Thus, we conclude that the empirical discrepancies between the two data sources provide no clear guidance about the preferred source of earnings data. Consequently, we used both data sources to analyze the earnings impacts in Chapter VIII, but focused primarily on the impacts based on the wage records, since these data were available for the full sample of claimants.

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examined. However, absolute differences in the New Jersey demonstration were nearly $1,000 or more, even after the sample exclusions.

APPENDIX E

SUPPLEMENTAL TABLES

TABLE E.1

REGRESSION RESULTS FOR THE
UI OUTCOMES USING TREATMENT INDICATORS
(Standard Errors in Parentheses)

Explanatory Variable	Dependent Variable: Weeks of Benefits Received in Benefit Year	Dollars of Benefits Received in Benefit Year	Rate of Benefit Exhaustion (Percent)	Weeks of Benefits Received in Initial UI Spell
Intercept	14.37	112	30.3	12.94
	(0.64)	(111)	(2.7)	(0.63)
Treatment 1	-0.65	-103	0.0	-0.21
	(0.34)	(58)	(1.4)	(0.33)
Treatment 2	-0.36	-69	0.1	-0.55
	(0.28)	(48)	(1.2)	(0.27)
Treatment 3	-0.44	-99	0.0	-0.37
	(0.30)	(52)	(1.3)	(0.29)
Treatment 4	-0.82	-130	-1.4	-0.68
	(0.26)	(46)	(1.1)	(0.26)
Treatment 5	-0.33	-61	1.3	-0.08
	(0.30)	(53)	(1.3)	(0.30)
Cohort 1988.3	-2.45	-446	-13.4	-3.42
	(0.90)	(155)	(3.7)	(0.87)
Cohort 1988.4	-1.53	-314	-10.0	-1.09
	(0.38)	(66)	(1.6)	(0.37)
Cohort 1989.1	-2.05	-427	-9.5	-1.98
	(0.37)	(64)	(1.6)	(0.36)
Cohort 1989.2	-1.93	-370	-7.9	-1.89
	(0.37)	(65)	(1.6)	(0.36)
Cohort 1989.3	-1.67	-305	-6.1	-1.96
	(0.37)	(64)	(1.6)	(0.36)
Weekly Benefit Amount (Dollars)	.020	17.2	.074	.013
	(.002)	(0.4)	(.009)	(.002)
Base Period Earnings (Thousands of Dollars)	-.065	-10.0	-0.21	-.006
	(.014)	(2.4)	(0.06)	(.013)
Potential UI Duration = 16 Weeks	-1.71	29	15.8	-1.35
	(0.87)	(150)	(3.6)	(0.85)
Female	1.04	145	5.9	1.01
	(0.20)	(34)	(0.8)	(0.19)
Age Less than 35 Years	-1.54	-215	-5.0	-1.27
	(0.20)	(34)	(0.8)	(0.19)
Age Greater than 54 Years	3.19	299	7.8	2.58
	(0.31)	(54)	(1.3)	(0.30)
Black	-1.99	-285	-1.2	-1.57
	(0.36)	(63)	(1.5)	(0.35)
Hispanic	-1.72	-277	-6.1	-1.56
	(0.51)	(88)	(2.1)	(0.50)

TABLE E.1 (continued)

Explanatory Variable	Dependent Variable: Weeks of Benefits Received in Benefit Year	Dollars of Benefits Received in Benefit Year	Rate of Benefit Exhaustion (Percent)	Weeks of Benefits Received in Initial UI Spell
Other Non-White	-1.62	-196	-2.3	-2.03
	(1.19)	(205)	(5.0)	(1.15)
Expect Recall	0.64	96	-13.6	0.90
	(0.30)	(51)	(1.2)	(0.29)
Coatesville	-0.94	-171	-6.7	-0.54
	(0.48)	(83)	(2.0)	(0.47)
Phil. - North	1.28	179	3.1	0.77
	(0.46)	(80)	(1.9)	(0.45)
Reading	-1.85	-323	-7.5	-2.23
	(0.48)	(82)	(2.0)	(0.46)
Lewistown	0.19	-95	-6.3	-1.98
	(0.54)	(94)	(2.3)	(0.53)
Butler	0.93	109	-2.7	-0.14
	(0.50)	(87)	(2.1)	(0.49)
Connellsville	1.64	150	0.8	-0.62
	(0.52)	(89)	(2.2)	(0.50)
McKeesport	1.27	130	-0.3	0.16
	(0.49)	(84)	(2.0)	(0.47)
Erie	0.49	-3	-4.3	-0.85
	(0.49)	(84)	(2.0)	(0.47)
Pittson	2.15	281	2.9	0.87
	(0.54)	(93)	(2.3)	(0.53)
Scranton	-0.04	-40	-5.1	-1.29
	(0.50)	(86)	(2.1)	(0.48)
Lancaster	-2.20	-417	-10.8	-2.64
	(0.49)	(84)	(2.0)	(0.47)
Sample Size	13,913	13,913	13,913	13,913

NOTE: All explanatory variables are binary indicators except where noted otherwise.

TABLE E.2

REGRESSION RESULTS FOR WEEKS OF UI RECEIPT
USING BONUS PARAMETERS
(Standard Errors in Parentheses)

	Model	
Explanatory Variable	(1)	(2)
Intercept	14.34 (0.64)	14.10 (0.65)
Bonus Parameters		
Received a Bonus Offer	-0.71 (0.25)	
Amount of Offer Was Limited	0.09 (0.23)	
Duration of Offer Was Limited	0.21 (0.23)	
Amount of Offer Declined Over Time	0.38 (0.30)	0.38 (0.28)
Amount of Bonus Offer (Thousands of Dollars)		0.33 (0.29)
Duration of Bonus Offer (Weeks)		-.029 (.026)
Cohort 1988.3	-2.43 (0.90)	-2.42 (0.89)
Cohort 1988.4	-1.51 (0.38)	-1.50 (0.38)
Cohort 1989.1	-2.03 (0.37)	-2.02 (0.37)
Cohort 1989.2	-1.91 (0.37)	-1.90 (0.37)
Cohort 1989.3	-1.63 (0.37)	-1.62 (0.37)
Weekly Benefit Amount (Dollars)	.020 (.002)	.021 (.002)
Base Period Earnings (Thousands of Dollars)	-.065 (.014)	-.065 (.014)
Potential UI Duration = 16 Weeks	-1.72 (0.87)	-1.71 (0.87)
Female	1.04 (0.20)	1.04 (0.20)
Age Less than 35 Years	-1.53 (0.20)	-1.53 (0.20)
Age Greater than 54 Years	3.19 (0.31)	3.18 (0.31)
Black	-1.98 (0.36)	-1.98 (0.36)

TABLE E.2 (continued)

Explanatory Variable	Model (1)	Model (2)
Hispanic	-1.72 (0.51)	-1.72 (0.51)
Other Non-White	-1.63 (1.19)	-1.64 (1.19)
Expect Recall	0.64 (0.30)	0.63 (0.30)
Coatesville	-0.94 (0.48)	-0.93 (0.48)
Phil. - North	1.27 (0.46)	1.28 (0.46)
Reading	-1.85 (0.48)	-1.85 (0.48)
Lewistown	0.20 (0.54)	0.20 (0.54)
Butler	0.93 (0.50)	0.93 (0.50)
Connellsville	1.64 (0.52)	1.64 (0.52)
McKeesport	1.27 (0.49)	1.27 (0.49)
Erie	0.49 (0.49)	0.48 (0.49)
Pittston	2.15 (0.54)	2.15 (0.54)
Scranton	-0.04 (0.50)	-0.04 (0.50)
Lancaster	-2.19 (0.49)	-2.19 (0.49)
Sample Size	13,913	13,913

NOTE: All explanatory variables are binary indicators except where noted otherwise.

TABLE E.3

REGRESSION RESULTS FOR THE CUMULATIVE
UI EXIT RATES
(Standard Errors in Parentheses)

	Dependent Variable: Cumulative Exit Rate (Percent) at			
Explanatory Variable	Week 8	Week 14	Week 20	Week 26
Intercept	46.7	58.9	64.6	73.4
	(3.0)	(3.0)	(2.7)	(2.4)
Treatment 1	2.2	1.2	0.2	0.1
	(1.6)	(1.5)	(1.4)	(1.3)
Treatment 2	2.6	2.7	2.1	0.8
	(1.3)	(1.3)	(1.2)	(1.1)
Treatment 3	3.7	2.2	0.9	0.6
	(1.4)	(1.4)	(1.3)	(1.1)
Treatment 4	3.6	3.7	2.2	1.4
	(1.2)	(1.2)	(1.1)	(1.0)
Treatment 5	1.2	1.0	-0.2	0.7
	(1.4)	(1.4)	(1.3)	(1.2)
Cohort 1988.3	15.4	12.2	10.0	11.1
	(0.4)	(4.1)	(3.8)	(3.4)
Cohort 1988.4	0.8	3.3	7.8	8.0
	(1.8)	(1.7)	(1.6)	(1.4)
Cohort 1989.1	6.7	10.8	10.1	7.1
	(1.7)	(1.7)	(1.6)	(1.4)
Cohort 1989.2	4.3	10.3	10.1	7.7
	(1.8)	(1.7)	(1.6)	(1.4)
Cohort 1989.3	8.0	10.2	7.5	5.7
	(1.7)	(1.7)	(1.6)	(1.4)
Weekly Benefit Amount (Dollars)	-.049	-.065	-.051	-.045
	(.010)	(.010)	(.009)	(.008)
Base Period Earnings (Thousands of Dollars)	-.030	.080	.078	.045
	(.064)	(.063)	(.058)	(.052)
Potential UI Duration = 16 Weeks	-3.1	-0.4	18.8	12.3
	(4.1)	(4.0)	(3.7)	(3.3)
Female	-5.4	-3.9	-4.6	-4.4
	(1.0)	(0.9)	(0.8)	(0.7)
Age Less than 35 Years	5.1	4.5	5.2	5.2
	(0.9)	(0.9)	(0.8)	(0.8)
Age Greater than 54 Years	-5.4	-6.8	-8.4	-8.8
	(1.5)	(1.4)	(1.3)	(1.2)
Black	8.7	4.1	2.1	-0.6
	(1.7)	(1.7)	(1.5)	(1.4)
Hispanic	4.8	6.9	6.2	4.6
	(2.4)	(2.3)	(2.2)	(1.9)
Other Non-White	9.8	6.6	6.2	2.0
	(5.5)	(5.4)	(5.0)	(4.5)

TABLE E.3 (continued)

Explanatory Variable	Dependent Variable: Cumulative Exit Rate (Percent) at			
	Week 8	Week 14	Week 20	Week 26
Expect Recall	-20.8	0.7	11.3	12.2
	(1.4)	(1.3)	(1.2)	(1.1)
Coatesville	0.8	3.0	4.6	4.9
	(2.3)	(2.2)	(2.0)	(1.8)
Phil.-North	-1.5	-3.4	-1.9	-0.7
	(2.2)	(2.1)	(2.0)	(1.8)
Reading	11.0	7.9	7.9	6.3
	(2.2)	(2.2)	(2.0)	(1.8)
Lewistown	7.7	6.2	8.7	8.6
	(2.5)	(2.5)	(2.3)	(2.1)
Butler	2.0	-0.7	1.0	2.9
	(2.4)	(2.3)	(2.1)	(1.9)
Connellsville	5.1	1.5	1.8	2.2
	(2.4)	(2.4)	(2.2)	(2.0)
McKeesport	-0.5	-1.1	0.8	1.6
	(2.3)	(2.2)	(2.0)	(1.8)
Erie	4.4	2.8	3.8	5.3
	(2.3)	(2.2)	(2.1)	(1.8)
Pittston	-2.5	-3.5	-2.1	0.0
	(2.5)	(2.5)	(2.3)	(2.1)
Scranton	5.3	5.2	5.8	5.1
	(2.3)	(2.3)	(2.1)	(1.9)
Lancaster	11.5	10.1	10.3	10.9
	(2.3)	(2.2)	(2.1)	(1.8)
Sample Size	13,913	13,913	13,913	13,913

NOTE: All explanatory variables are binary indicators except where noted otherwise.

TABLE E.4

REGRESSION RESULTS FOR THE PROBABILITY OF EMPLOYMENT
(Standard Errors in Parentheses)

	Dependent Variable: Whether Employed in			
Explanatory Variable	Quarter of Benefit Application	Quarter 1	Quarter 2	Quarter 3
Intercept	63.3 %	44.5 %	53.1 %	52.8 %
	(2.2)	(3.0)	(2.8)	(2.8)
Treatment 1	-0.8	-1.6	-0.6	-2.8
	(1.2)	(1.6)	(1.5)	(1.4)
Treatment 2	-0.1	1.1	-0.5	-2.2
	(1.0)	(1.3)	(1.2)	(1.2)
Treatment 3	0.3	0.4	-0.4	0.4
	(1.0)	(1.4)	(1.3)	(1.3)
Treatment 4	-0.8	0.5	-0.1	-0.9
	(0.9)	(1.2)	(1.2)	(1.1)
Treatment 5	0.4	-1.3	0.1	-1.9
	(1.1)	(1.4)	(1.3)	(1.3)
Cohort 1988.3	14.4	14.1	4.8	13.4
	(3.1)	(4.2)	(3.9)	(3.9)
Cohort 1988.4	13.2	-0.2	3.9	4.3
	(1.3)	(1.8)	(1.7)	(1.6)
Cohort 1989.1	7.7	7.3	1.3	1.2
	(1.3)	(1.7)	(1.6)	(1.6)
Cohort 1989.2	9.0	4.9	0.8	-0.6
	(1.3)	(1.7)	(1.6)	(1.6)
Cohort 1989.3	10.0	4.1	-2.0	-0.8
	(1.3)	(1.7)	(1.6)	(1.6)
Weekly Benefit Amount (Dollars)	.008	.027	.047	.068
	(.007)	(.010)	(.009)	(.009)
Base Period Earnings (Thousands of Dollars)	.234	-.085	-.328	-.381
	(.048)	(.064)	(.060)	(.059)
Potential UI Duration = 16 Weeks	-3.7	-1.2	-0.5	-.051
	(3.0)	(4.1)	(3.8)	(.038)
Female	2.8	-1.4	-7.3	1.0
	(0.7)	(0.9)	(8.7)	(8.5)
Age Less than 35 Years	2.4	2.4	1.7	2.0
	(0.7)	(0.9)	(0.9)	(0.9)
Age Greater than 54 Years	-1.3	-6.7	-8.2	-12.8
	(1.1)	(1.5)	(1.4)	(1.3)
Black	-3.1	-5.2	-6.3	-3.0
	(1.3)	(1.7)	(1.6)	(1.6)
Hispanic	0.6	2.3	0.7	-1.1
	(1.8)	(2.4)	(2.3)	(2.2)
Other Non-White	-4.9	0.9	-3.3	-9.2
	(4.1)	(5.5)	(5.2)	(5.1)

TABLE E.4 (continued)

Explanatory Variable	Dependent Variable: Whether Employed in Quarter of Benefit Application	Quarter 1	Quarter 2	Quarter 3
Expect Recall	5.2	9.2	17.0	14.1
	(1.0)	(1.4)	(1.3)	(1.3)
Coatesville	5.6	6.3	7.8	8.9
	(1.7)	(2.2)	(2.1)	(2.1)
Phil.-North	2.0	1.2	3.5	5.7
	(1.6)	(2.2)	(2.0)	(2.0)
Reading	7.9	12.1	15.3	15.2
	(1.7)	(2.2)	(2.1)	(2.1)
Lewistown	4.0	11.3	13.1	12.3
	(1.9)	(2.5)	(2.4)	(2.3)
Butler	7.6	7.9	10.4	12.4
	(1.7)	(2.3)	(2.2)	(2.2)
Connellsville	2.5	7.9	9.5	9.1
	(1.8)	(2.4)	(2.3)	(2.2)
McKeesport	2.5	6.3	9.7	10.8
	(1.7)	(2.3)	(2.1)	(2.1)
Erie	5.2	8.7	12.6	11.7
	(1.7)	(2.3)	(2.1)	(2.1)
Pittston	3.5	4.4	9.8	11.8
	(1.9)	(2.5)	(2.4)	(2.3)
Scranton	7.4	10.2	12.7	12.8
	(1.7)	(2.3)	(2.2)	(2.1)
Lancaster	6.2	9.6	12.4	11.1
	(1.7)	(2.3)	(2.1)	(2.1)
Sample Size	13,907	13,901	13,897	13,902

NOTE: Quarters 1, 2, and 3 are the first, second, and third full calendar quarters after benefit application. All explanatory variables are binary indicators except where noted otherwise.

TABLE E.5

REGRESSION RESULTS FOR WEEKS OF EMPLOYMENT
(Standard Errors in Parentheses)

	Dependent Variable: Weeks Employed in			
Explanatory Variable	Quarter of Benefit Application	Quarter 1	Quarter 2	Quarter 3
Intercept	2.64	3.33	4.95	5.20
	(0.28)	(0.33)	(0.34)	(0.34)
Treatment 1	-0.25	-0.22	-0.19	-0.46
	(0.15)	(0.17)	(0.18)	(0.18)
Treatment 2	-0.04	0.09	-0.07	-0.23
	(0.12)	(0.14)	(0.15)	(0.15)
Treatment 3	-0.10	0.13	-0.08	0.00
	(0.13)	(0.15)	(0.16)	(0.16)
Treatment 4	-0.14	0.14	0.09	-0.12
	(0.11)	(0.13)	(0.14)	(0.14)
Treatment 5	0.02	-0.10	0.02	-0.19
	(0.13)	(0.16)	(0.16)	(0.16)
Cohort 1988.3	2.77	1.01	0.66	0.95
	(0.39)	(0.46)	(0.48)	(0.48)
Cohort 1988.4	2.89	-0.15	0.55	0.68
	(0.17)	(0.20)	(0.20)	(0.20)
Cohort 1989.1	1.93	0.91	0.23	0.23
	(0.16)	(0.19)	(0.20)	(0.20)
Cohort 1989.2	2.08	0.06	0.31	-0.06
	(0.16)	(0.19)	(0.20)	(0.20)
Cohort 1989.3	2.07	0.52	-0.25	-0.02
	(0.16)	(0.19)	(0.20)	(0.20)
Weekly Benefit Amount (Dollars)	.0039	.0047	.0068	.0100
	(.0009)	(.0011)	(.0011)	(.0011)
Base Period Earnings (Thousands of Dollars)	.044	-.001	-.028	-.033
	(.006)	(.007)	(.007)	(.007)
Potential UI Duration = 16 Weeks	-1.26	-0.68	-1.48	-1.54
	(0.38)	(0.45)	(0.47)	(0.47)
Female	0.55	-0.05	0.14	0.40
	(0.09)	(0.10)	(0.11)	(0.11)
Age Less than 35 Years	0.17	0.11	0.11	0.12
	(0.09)	(0.10)	(0.11)	(0.11)
Age Greater than 54 Years	-0.10	-0.81	-1.05	-1.49
	(0.14)	(0.16)	(0.17)	(0.17)
Black	-0.27	-0.53	-0.99	-0.98
	(0.16)	(0.19)	(0.19)	(0.19)
Hispanic	0.21	0.23	0.17	-0.17
	(0.22)	(0.26)	(0.27)	(0.27)
Other Non-White	-0.02	-0.49	-0.47	-0.69
	(0.51)	(0.61)	(0.63)	(0.64)

TABLE E.5 (continued)

Explanatory Variable	Dependent Variable: Weeks Employed in Quarter of Benefit Application	Quarter 1	Quarter 2	Quarter 3
Expect Recall	0.80	-0.11	2.55	2.01
	(0.13)	(0.15)	(0.16)	(0.16)
Coatesville	0.92	0.61	0.84	0.85
	(0.21)	(0.25)	(0.26)	(0.26)
Phil.-North	0.54	0.29	0.30	0.37
	(0.20)	(0.24)	(0.25)	(0.25)
Reading	1.12	1.45	1.59	1.51
	(0.21)	(0.24)	(0.26)	(0.26)
Lewistown	0.55	1.13	0.87	0.92
	(0.24)	(0.28)	(0.29)	(0.29)
Butler	1.28	0.91	1.13	1.28
	(0.22)	(0.26)	(0.27)	(0.27)
Connellsville	0.99	1.16	0.98	0.67
	(0.22)	(0.26)	(0.28)	(0.28)
McKeesport	0.73	0.76	0.92	1.05
	(0.21)	(0.25)	(0.26)	(0.26)
Erie	1.21	0.98	1.38	1.20
	(0.21)	(0.25)	(0.26)	(0.26)
Pittston	0.50	0.53	0.89	1.28
	(0.23)	(0.28)	(0.29)	(0.29)
Scranton	1.25	1.38	1.30	1.27
	(0.22)	(0.25)	(0.27)	(0.27)
Lancaster	0.98	1.11	1.32	1.02
	(0.21)	(0.25)	(0.26)	(0.26)
Sample Size	13,907	13,901	13,897	13,902

NOTE: Quarters 1, 2, and 3 are the first, second, and third full calendar quarters after benefit application. All explanatory variables are binary indicators except where noted otherwise.

TABLE E.6

REGRESSION RESULTS FOR EARNINGS
(Standard Errors in Parentheses)

Explanatory Variable	Dependent Variable: Earnings in Quarter of Benefit Application	Quarter 1	Quarter 2	Quarter 3
Intercept	$-839	$139	$226	$215
	(190)	(159)	(159)	(156)
Treatment 1	-22	12	-85	-191
	(99)	(83)	(83)	(82)
Treatment 2	96	83	28	-4
	(82)	(69)	(69)	(68)
Treatment 3	-20	116	-14	55
	(89)	(74)	(74)	(73)
Treatment 4	-36	74	81	50
	(78)	(65)	(65)	(64)
Treatment 5	65	-62	2	34
	(90)	(75)	(75)	(74)
Cohort 1988.3	1,270	239	276	269
	(264)	(221)	(221)	(218)
Cohort 1988.4	1,336	-49	204	352
	(113)	(94)	(94)	(93)
Cohort 1989.1	801	322	193	207
	(110)	(92)	(92)	(91)
Cohort 1989.2	910	112	122	0
	(110)	(92)	(92)	(91)
Cohort 1989.3	958	149	-138	45
	(110)	(92)	(92)	(90)
Weekly Benefit Amount (Dollars)	-5.1	0.9	7.6	7.9
	(0.6)	(0.5)	(0.5)	(0.5)
Base Period Earnings (Thousands of Dollars)	231	89	52	59
	(4)	(3)	(3)	(3)
Potential UI Duration = 16 Weeks	471	241	134	35
	(257)	(215)	(215)	(212)
Female	59	-231	-281	-262
	(58)	(49)	(49)	(48)
Age Less than 35 Years	-37	8	52	37
	(59)	(49)	(49)	(48)
Age Greater than 54 Years	128	-205	-483	-677
	(92)	(77)	(77)	(76)
Black	-255	-270	-530	-503
	(107)	(89)	(89)	(88)
Hispanic	63	56	-48	-149
	(151)	(126)	(126)	(125)
Other Non-White	-135	-338	-318	-130
	(350)	(293)	(293)	(289)

TABLE E.6 (continued)

Explanatory Variable	Dependent Variable: Earnings in Quarter of Benefit Application	Quarter 1	Quarter 2	Quarter 3
Expect Recall	197	59	969	863
	(87)	(73)	(73)	(72)
Coatesville	421	222	271	424
	(142)	(119)	(119)	(117)
Phil.-North	-12	186	168	252
	(136)	(114)	(114)	(112)
Reading	67	271	349	387
	(141)	(118)	(118)	(116)
Lewistown	-60	220	160	290
	(160)	(134)	(134)	(132)
Butler	198	91	140	258
	(149)	(125)	(124)	(123)
Connellsville	8	172	117	17
	(152)	(128)	(127)	(126)
McKeesport	-2	96	144	306
	(143)	(120)	(120)	(118)
Erie	307	276	368	450
	(144)	(120)	(120)	(119)
Pittston	-124	54	46	271
	(160)	(134)	(134)	(132)
Scranton	183	171	117	170
	(147)	(123)	(123)	(121)
Lancaster	383	123	174	152
	(144)	(120)	(120)	(118)
Sample Size	13,913	13,913	13,913	13,913

NOTE: Quarters 1, 2, and 3 are the first, second, and third full calendar quarters after benefit application. All explanatory variables are binary indicators except where noted otherwise.

TABLE E.7

REGRESSION RESULTS FOR THE PROPORTION OF TIME EMPLOYED, BASED ON INTERVIEW DATA
(Standard Errors in Parentheses)

Explanatory Variable	Dependent Variable: Proportion of Time Employed in			
	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Intercept	13.8 %	38.5 %	52.7 %	58.2 %
	(3.6)	(4.7)	(4.4)	(4.9)
Treatment 1	1.7	4.9	3.1	2.8
	(1.8)	(2.3)	(2.2)	(2.5)
Treatment 2	2.4	1.2	-2.6	-2.7
	(1.4)	(1.9)	(1.8)	(2.1)
Treatment 3	1.7	0.2	-1.2	2.4
	(1.6)	(2.1)	(1.9)	(2.2)
Treatment 4	0.2	0.2	-0.6	-0.2
	(1.4)	(1.8)	(1.7)	(1.9)
Treatment 5	1.6	2.0	3.0	1.3
	(1.6)	(2.1)	(2.0)	(2.3)
Cohort 1988.3	17.7	12.2	-1.1	4.3
	(4.9)	(6.4)	(6.0)	(6.0)
Cohort 1988.4	3.7	11.8	10.8	7.6
	(2.1)	(2.7)	(2.5)	(2.6)
Cohort 1989.1	7.9	12.7	4.3	-0.1
	(2.0)	(2.6)	(2.4)	(2.6)
Cohort 1989.2	5.2	12.8	4.2	-0.6
	(2.0)	(2.6)	(2.5)	(2.7)
Cohort 1989.3	7.2	6.1	1.9	1.1
	(2.0)	(2.6)	(2.5)	(2.7)
Weekly Benefit Amount (Dollars)	.014	.004	.037	.041
	(.011)	(.015)	(.014)	(.016)
Base Period Earnings (Thousands of Dollars)	-.068	-.010	-.004	.146
	(.071)	(.093)	(.087)	(.100)
Potential UI Duration = 16 Weeks	-13.9	-14.5	-11.6	-4.9
	(5.2)	(6.8)	(6.3)	(7.5)
Female	-1.1	-3.4	-1.0	0.5
	(1.1)	(1.4)	(1.3)	(1.5)
Age Less than 35 Years	1.8	2.5	1.6	2.0
	(1.1)	(1.4)	(1.3)	(1.5)
Age Greater than 54 Years	-2.6	-8.4	-12.1	-13.2
	(1.6)	(2.1)	(2.0)	(2.4)
Black	-3.2	-9.6	-11.2	-9.1
	(2.1)	(2.7)	(2.6)	(2.9)
Hispanic	1.5	7.5	3.0	2.2
	(3.3)	(4.3)	(4.0)	(4.6)
Other Non-White	-9.0	1.0	-6.7	-5.7
	(6.3)	(8.2)	(7.7)	(9.0)

TABLE E.7 (continued)

Explanatory Variable	Dependent Variable: Proportion of Time Employed in			
	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Expect Recall	-6.9	13.3	14.3	3.4
	(1.5)	(2.0)	(1.8)	(2.1)
Coatesville	4.9	7.3	6.7	4.1
	(2.7)	(3.5)	(3.3)	(3.8)
Phil. - North	-1.0	-3.3	-2.8	-2.6
	(2.7)	(3.5)	(3.3)	(3.7)
Reading	9.6	12.0	12.7	10.6
	(2.7)	(3.6)	(3.4)	(3.8)
Lewistown	5.1	5.9	7.5	6.8
	(3.1)	(4.0)	(3.8)	(4.3)
Butler	6.4	9.1	7.6	8.2
	(2.8)	(3.7)	(3.4)	(3.9)
Connellsville	5.9	1.4	1.4	-3.3
	(2.9)	(3.7)	(3.5)	(4.0)
McKeesport	1.9	2.8	1.8	2.1
	(2.8)	(3.6)	(3.4)	(3.8)
Erie	5.8	5.2	5.7	5.1
	(2.8)	(3.6)	(3.4)	(3.9)
Pittston	1.5	0.6	6.7	3.4
	(3.0)	(3.9)	(3.7)	(4.2)
Scranton	8.7	9.4	10.2	8.2
	(2.8)	(3.6)	(3.4)	(3.9)
Lancaster	11.7	13.7	9.3	5.2
	(2.8)	(3.6)	(3.4)	(3.9)
Sample Size	5,134	5,133	5,133	3,556

NOTE: Quarters are defined relative to the benefit application date. All explanatory variables are binary indicators except where noted otherwise. Observations are weighted to correct for the oversampling of bonus receipients.

TABLE E.8

REGRESSION RESULTS FOR EARNINGS,
BASED ON INTERVIEW DATA
(Standard Errors in Parentheses)

Explanatory Variable	Dependent Variable: Earnings in Quarter 1	Quarter 2	Quarter 3	Quarter 4
Intercept	$41	$433	$980	$1,188
	(179)	(266)	(281)	(339)
Treatment 1	134	287	99	-90
	(90)	(134)	(142)	(174)
Treatment 2	152	135	122	129
	(73)	(108)	(114)	(144)
Treatment 3	111	14	2	112
	(80)	(118)	(125)	(154)
Treatment 4	73	130	142	131
	(68)	(101)	(107)	(133)
Treatment 5	48	70	146	95
	(81)	(120)	(127)	(156)
Cohort 1988.3	563	516	151	270
	(245)	(364)	(384)	(412)
Cohort 1988.4	134	550	480	298
	(104)	(154)	(163)	(179)
Cohort 1989.1	347	636	254	52
	(100)	(149)	(157)	(179)
Cohort 1989.2	257	594	126	-43
	(101)	(150)	(158)	(186)
Cohort 1989.3	273	226	-57	-240
	(101)	(149)	(158)	(187)
Weekly Benefit Amount (Dollars)	3.2	4.9	5.0	5.6
	(0.6)	(0.8)	(0.9)	(1.1)
Base Period Earnings (Thousands of Dollars)	12.1	43.3	67.9	79.9
	(3.6)	(5.3)	(5.6)	(6.9)
Potential UI Duration = 16 Weeks	-293	33	321	745
	(260)	(386)	(408)	(513)
Female	-258	-550	-528	-497
	(53)	(79)	(84)	(103)
Age Less than 35 Years	70	144	170	290
	(53)	(79)	(84)	(103)
Age Greater than 54 Years	-213	-644	-840	-971
	(81)	(120)	(127)	(163)
Black	-272	-637	-782	-795
	(105)	(156)	(165)	(199)
Hispanic	-59	207	88	43
	(166)	(246)	(260)	(316)
Other Non-White	-456	-123	-440	-732
	(316)	(469)	(496)	(617)

TABLE E.8 (continued)

Explanatory Variable	Dependent Variable: Earnings in Quarter 1	Quarter 2	Quarter 3	Quarter 4
Expect Recall	-184	593	673	183
	(76)	(112)	(119)	(142)
Coatesville	203	486	521	293
	(136)	(202)	(214)	(262)
Phil. - North	19	-123	-102	-164
	(134)	(198)	(210)	(254)
Reading	399	457	437	375
	(138)	(205)	(217)	(263)
Lewistown	26	67	64	49
	(155)	(230)	(243)	(296)
Butler	96	91	-100	-154
	(141)	(209)	(221)	(271)
Connellsville	89	-134	-290	-577
	(143)	(213)	(225)	(274)
McKeesport	11	-91	-136	-242
	(138)	(206)	(217)	(263)
Erie	192	102	98	140
	(139)	(207)	(219)	(267)
Pittston	-13	-136	-160	-361
	(151)	(224)	(237)	(288)
Scranton	186	60	-8	-90
	(140)	(208)	(220)	(269)
Lancaster	376	567	402	142
	(139)	(206)	(218)	(267)
Sample Size	5,134	5,133	5,133	3,556

NOTE: Quarters are defined relative to the benefit application date. All explanatory variables are binary indicators except where noted otherwise. Observations are weighted to correct for the oversampling of bonus recipients.

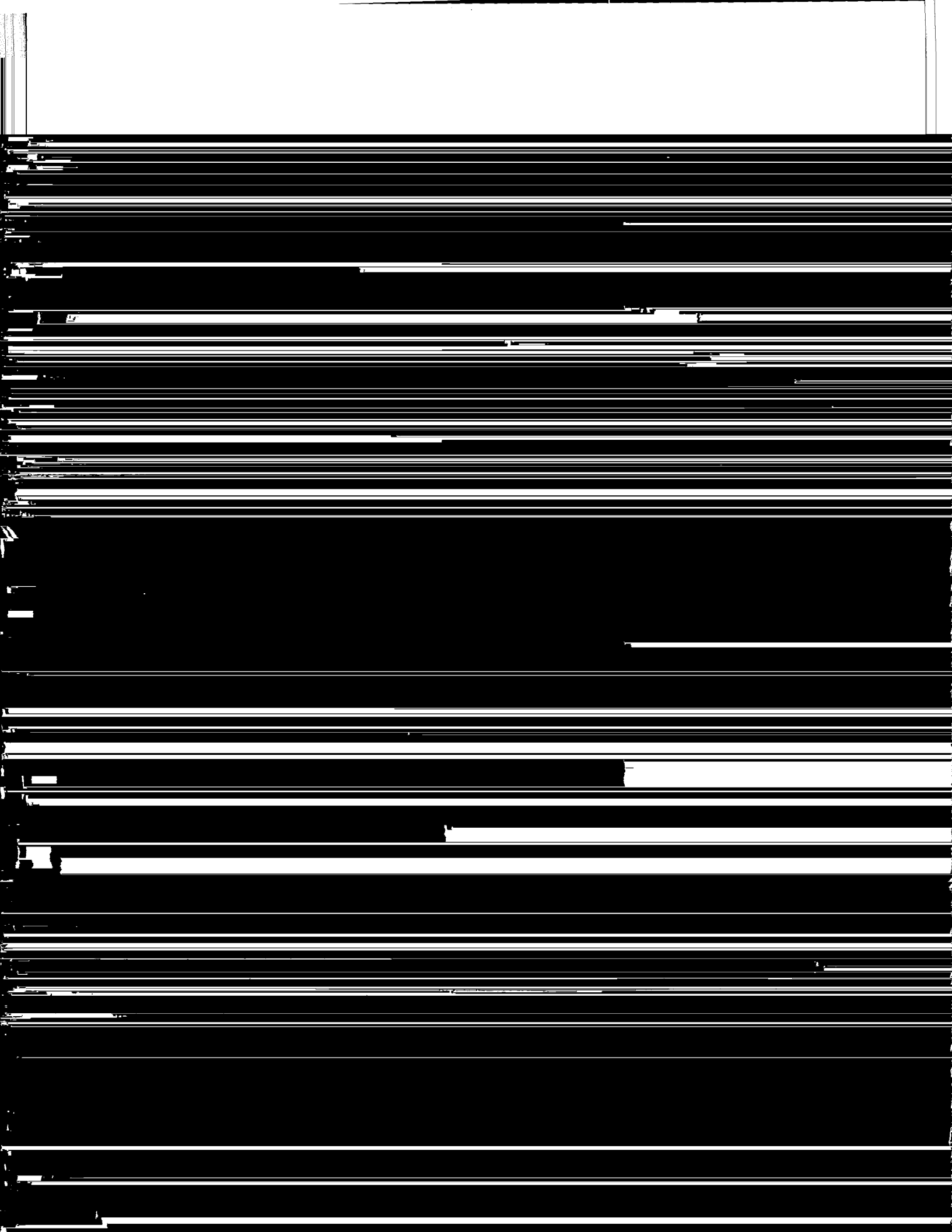

TABLE E.9 (continued)

Explanatory Variable	Dependent Variable: Whether Employed in			
	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Expect Recall	5.2	14.2	11.4	6.2
	(2.2)	(2.0)	(1.8)	(2.0)
Coatesville	7.5	7.4	5.7	7.2
	(4.0)	(3.7)	(3.3)	(3.7)
Phil. - North	-2.0	-3.7	0.1	-0.1
	(3.9)	(3.6)	(3.2)	(3.6)
Reading	11.2	13.8	10.3	12.1
	(4.0)	(3.7)	(3.3)	(3.7)
Lewistown	7.3	7.2	7.3	7.4
	(4.5)	(4.2)	(3.7)	(4.2)
Butler	7.5	10.9	5.4	10.0
	(4.1)	(3.8)	(3.4)	(3.8)
Connellsville	3.4	3.8	-1.1	-3.2
	(4.2)	(3.9)	(3.5)	(3.9)
McKeesport	3.0	3.9	1.8	2.5
	(4.0)	(3.7)	(3.3)	(3.7)
Erie	7.0	6.4	4.3	5.6
	(4.1)	(3.8)	(3.4)	(3.8)
Pittston	-0.6	5.6	4.3	2.1
	(4.4)	(4.1)	(3.6)	(4.1)
Scranton	10.0	10.6	8.3	8.8
	(4.1)	(3.8)	(3.4)	(3.8)
Lancaster	15.9	15.5	7.5	8.2
	(4.1)	(3.8)	(3.4)	(3.8)
Sample Size	5,134	5,133	5,133	3,556

NOTE: Quarters are defined relative to the benefit application date. All explanatory variables are binary indicators except where noted otherwise. Observations are weighted to correct for the oversampling of bonus recipients.

TABLE E.10

REGRESSION RESULTS FOR THE PROBABILITY OF FULL-TIME EMPLOYMENT, BASED ON INTERVIEW DATA
(Standard Errors in Parentheses)

	Dependent Variable: Whether Employed Full-Time in			
Explanatory Variable	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Intercept	18.0 % (5.1)	34.8 % (5.1)	43.8 % (4.8)	42.4 % (5.5)
Treatment 1	5.5 (2.6)	6.2 (2.6)	3.6 (2.4)	1.4 (2.8)
Treatment 2	3.4 (2.1)	0.9 (2.1)	0.0 (2.0)	0.3 (2.3)
Treatment 3	3.9 (2.3)	3.2 (2.3)	1.9 (2.2)	4.9 (2.5)
Treatment 4	2.9 (1.9)	1.3 (1.9)	0.7 (1.8)	0.2 (2.1)
Treatment 5	3.4 (2.3)	3.1 (2.3)	2.8 (2.2)	2.8 (2.5)
Cohort 1988.3	22.0 (6.9)	4.1 (6.9)	0.7 (6.6)	8.3 (6.6)
Cohort 1988.4	7.3 (2.9)	15.6 (2.9)	7.8 (2.8)	6.8 (2.9)
Cohort 1989.1	12.4 (2.8)	9.6 (2.8)	0.4 (2.7)	0.3 (2.9)
Cohort 1989.2	9.6 (2.9)	7.0 (2.9)	-1.8 (2.7)	-4.2 (3.0)
Cohort 1989.3	8.9 (2.8)	1.2 (2.8)	-3.5 (2.7)	-4.3 (3.0)
Weekly Benefit Amount (Dollars)	.050 (.016)	.078 (.016)	.119 (.015)	.129 (.017)
Base Period Earnings (Thousands of Dollars)	-.085 (.101)	-.083 (.101)	-.111 (.096)	-.089 (.111)
Potential UI Duration = 16 Weeks	-14.0 (7.4)	-0.3 (7.4)	-3.0 (7.0)	-6.1 (8.3)
Female	-8.3 (1.5)	-10.5 (1.5)	-10.1 (1.4)	-8.8 (1.7)
Age Less than 35 Years	3.0 (1.5)	5.1 (1.5)	5.6 (1.4)	5.3 (1.7)
Age Greater than 54 Years	-5.6 (2.3)	-12.4 (2.3)	-14.3 (2.2)	-17.6 (2.6)
Black	-5.7 (3.0)	-8.6 (3.0)	-6.3 (2.8)	0.5 (3.2)
Hispanic	6.9 (4.7)	10.0 (4.7)	1.8 (4.5)	3.5 (5.1)
Other Non-White	-1.3 (8.9)	-1.5 (8.9)	-13.6 (8.5)	-7.8 (9.9)

TABLE E.10 (continued)

Explanatory Variable	Dependent Variable: Whether Employed Full-Time in Quarter 1	Quarter 2	Quarter 3	Quarter 4
Expect Recall	-2.8	5.2	3.0	0.0
	(2.1)	(2.1)	(2.0)	(2.3)
Coatesville	6.3	4.6	4.7	9.9
	(3.9)	(3.9)	(3.7)	(4.2)
Phil. - North	-1.5	-6.4	1.1	0.2
	(3.8)	(3.8)	(3.6)	(4.1)
Reading	10.0	11.5	12.9	16.1
	(3.9)	(3.9)	(3.7)	(4.2)
Lewistown	7.3	3.9	6.2	7.7
	(4.4)	(4.4)	(4.2)	(4.8)
Butler	4.2	6.8	5.6	14.6
	(4.0)	(4.0)	(3.8)	(4.4)
Connellsville	1.1	-0.7	-2.5	-3.6
	(4.1)	(4.1)	(3.9)	(4.4)
McKeesport	0.4	-3.3	-1.4	2.5
	(3.9)	(3.9)	(3.7)	(4.2)
Erie	6.2	4.6	4.6	7.9
	(3.9)	(3.9)	(3.8)	(4.3)
Pittston	0.8	4.0	5.9	10.4
	(4.3)	(4.3)	(4.1)	(4.6)
Scranton	9.1	7.9	10.3	11.3
	(4.0)	(4.0)	(3.8)	(4.3)
Lancaster	14.0	11.8	9.3	11.9
	(3.9)	(3.9)	(3.8)	(4.3)
Sample Size	5,134	5,133	5,133	3,556

NOTE: Quarters are defined relative to the benefit application date. All explanatory variables are binary indicators except where noted otherwise. Observations are weighted to correct for the oversampling of bonus receipients.

TABLE E.11

REGRESSION RESULTS FOR THE HOURLY WAGE IN THE FIRST POST-UNEMPLOYMENT JOB
(Standard Errors in Parentheses)

Explanatory Variable	Least-Squares Estimates	Selection-Corrected Estimates
Intercept	$4.88 (0.40)	$6.01 (0.55)
Treatment 1	0.10 (0.19)	-0.03 (0.19)
Treatment 2	0.16 (0.16)	0.19 (0.16)
Treatment 3	0.13 (0.17)	0.08 (0.17)
Treatment 4	0.11 (0.14)	0.14 (0.15)
Treatment 5	0.03 (0.17)	0.02 (0.17)
Cohort 1988.3	-0.17 (0.51)	-0.55 (0.53)
Cohort 1988.4	0.05 (0.22)	-0.15 (0.23)
Cohort 1989.1	0.26 (0.21)	0.25 (0.21)
Cohort 1989.2	0.20 (0.22)	0.16 (0.22)
Cohort 1989.3	0.12 (0.21)	0.12 (0.21)
Weekly Benefit Amount (Dollars)	.007 (.001)	.006 (.001)
Based Period Earnings (Thousands of Dollars)	.175 (.008)	.178 (.008)
Potential UI Duration = 16 Weeks	2.05 (.057)	2.17 (0.57)
Female	-0.54 (0.13)	-0.37 (0.14)
Age Less than 35 Years	-0.12 (0.11)	-0.16 (0.11)
Age Greater than 54 Years	-0.16 (0.19)	0.32 (0.25)
Black	-0.72 (0.23)	-0.38 (0.26)
Hispanic	-0.36 (0.35)	-0.29 (0.35)
Other Non-White	-0.94 (0.72)	-0.28 (0.75)

TABLE E.11 (continued)

Explanatory Variable	Least-Squares Estimates	Selection-Corrected Estimates
Expect Recall	0.40 (0.16)	0.11 (0.18)
Nondurable Manufacturing	0.23 (0.16)	0.08 (0.17)
Durable Manufacturing	0.33 (0.14)	0.24 (0.15)
Seasonal Industry	1.49 (0.15)	1.24 (0.17)
White Collar	0.41 (0.16)	0.46 (0.16)
Administrative Support	-0.47 (0.15)	-0.46 (0.15)
High School Dropout	-0.40 (0.15)	-0.10 (0.18)
College Graduate	1.34 (0.19)	1.30 (0.19)
Coatesville	-0.05 (0.30)	-0.32 (0.31)
Phil. - North	-0.01 (0.30)	-0.14 (0.30)
Reading	-1.07 (0.30)	-1.60 (0.35)
Lewistown	-1.45 (0.33)	-1.72 (0.34)
Butler	-1.29 (0.31)	-1.59 (0.32)
Connellsville	-1.42 (0.31)	-1.48 (0.31)
McKeesport	-0.98 (0.30)	-1.17 (0.31)
Erie	-1.04 (0.31)	-1.22 (0.31)
Pittston	-1.29 (0.33)	-1.33 (0.33)
Scranton	-1.07 (0.30)	-1.46 (0.33)
Lancaster	-1.07 (0.30)	-1.53 (0.34)
Selection-Correction Term		-3.12 (1.04)
Sample Size	4,190	4,190

NOTE: Explanatory variables are binary indicators except where noted otherwise. Observations are weighted to correct for the oversampling of bonus recipients.

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