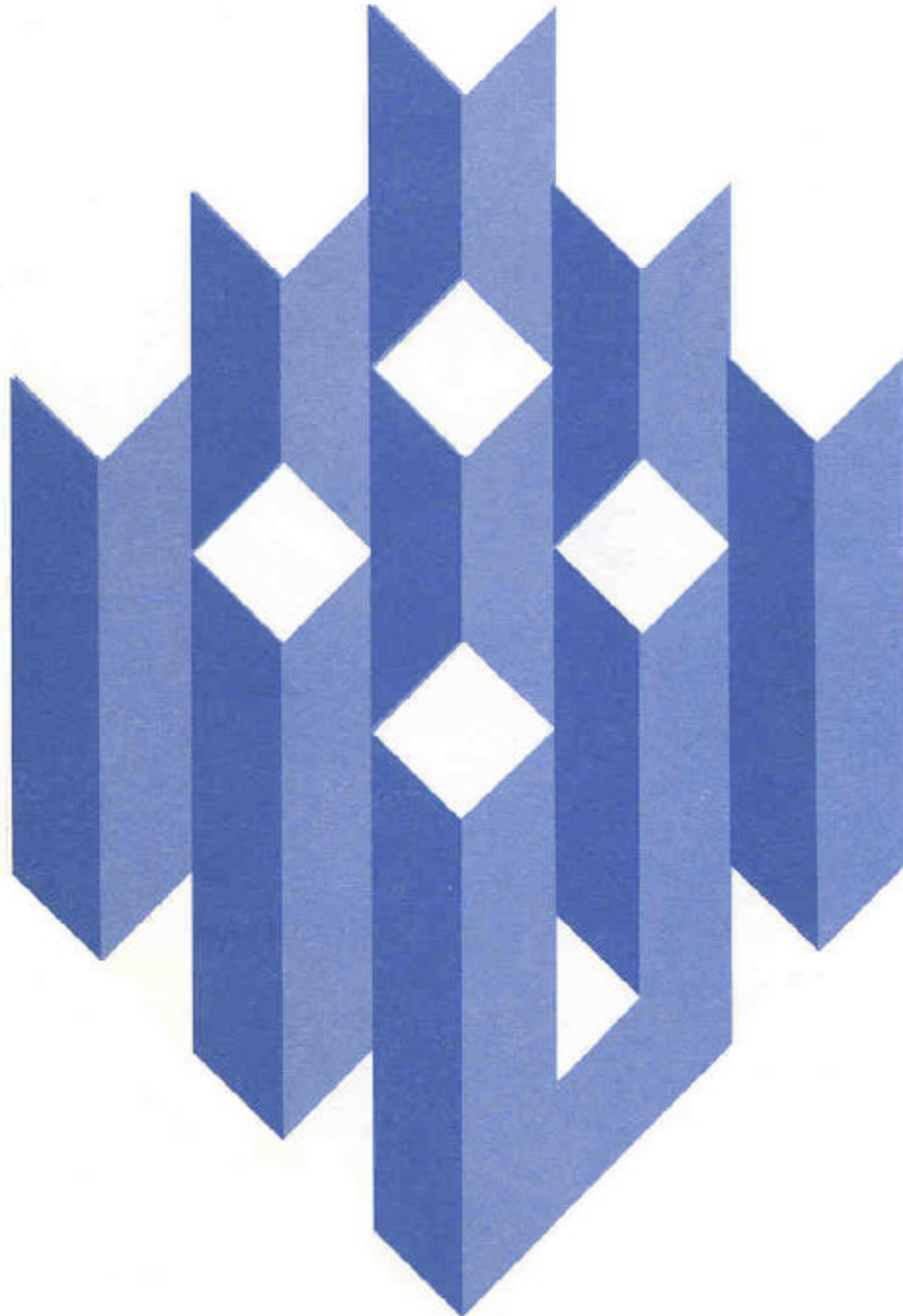


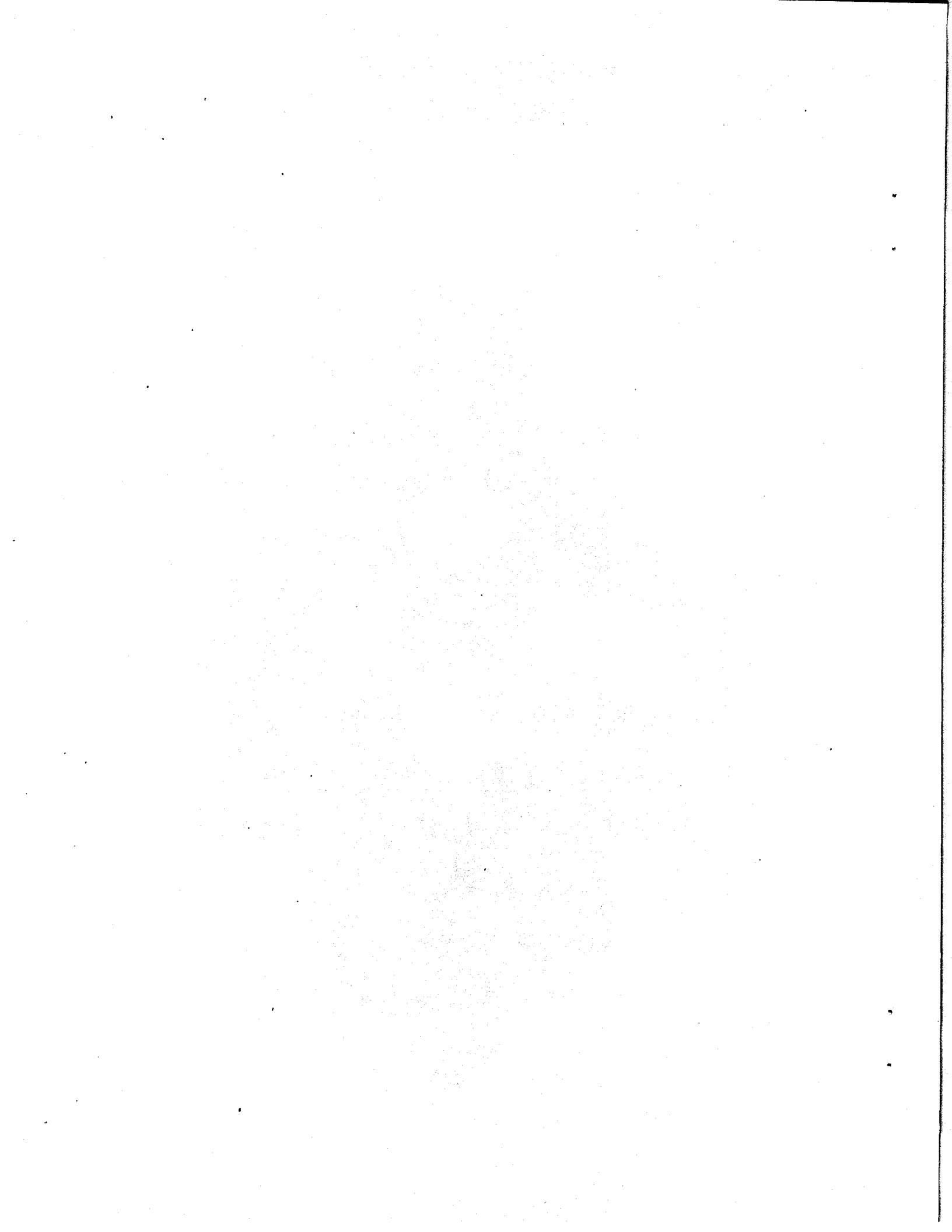


Evaluation of the charleston Claimant Placement and Work Test Demonstration

Unemployment Insurance
Occasional Paper 85-2

Department of Labor
Employment and Training Administration







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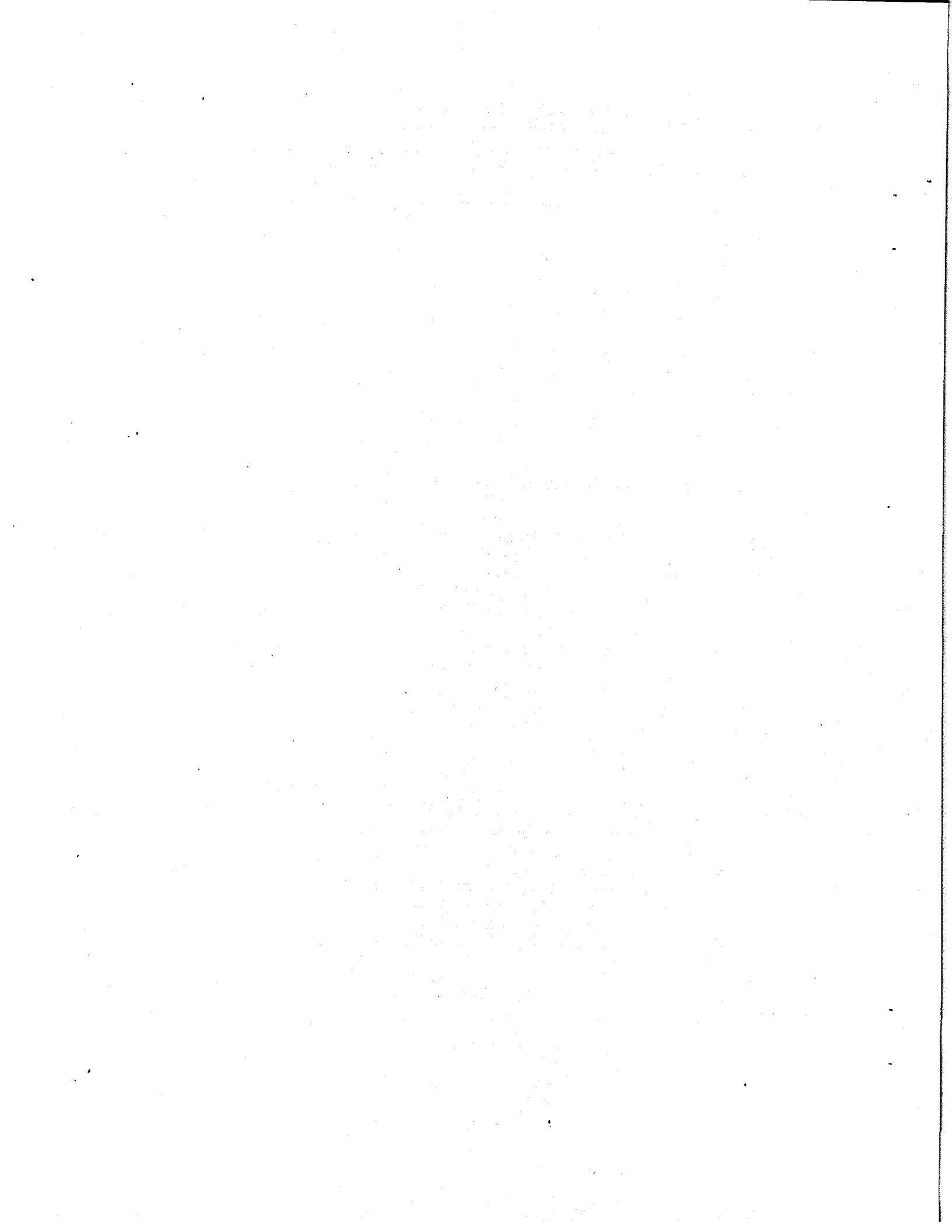
U.S. Department of Labor
William E. Brock, Secretary

Employment and Training Administration
Frank C. Casillas
Assistant Secretary of Labor

Unemployment Insurance Service
1985

This report was prepared by Walter Corson, Senior Economist, and David Long, Researcher, Mathematica Policy Research, and by Walter Nicholson, Professor of Economics, Amherst College, and Senior Fellow, Mathematica Policy Research. The research was sponsored by the Office of Strategic Planning and Policy Development of the Employment and Training Administration, U.S. Department of Labor, under Contract No. 20-34-82-07. Because researchers are encouraged to express their own viewpoints, the opinions offered in this document do not necessarily represent the official position or policy of the Department of Labor.

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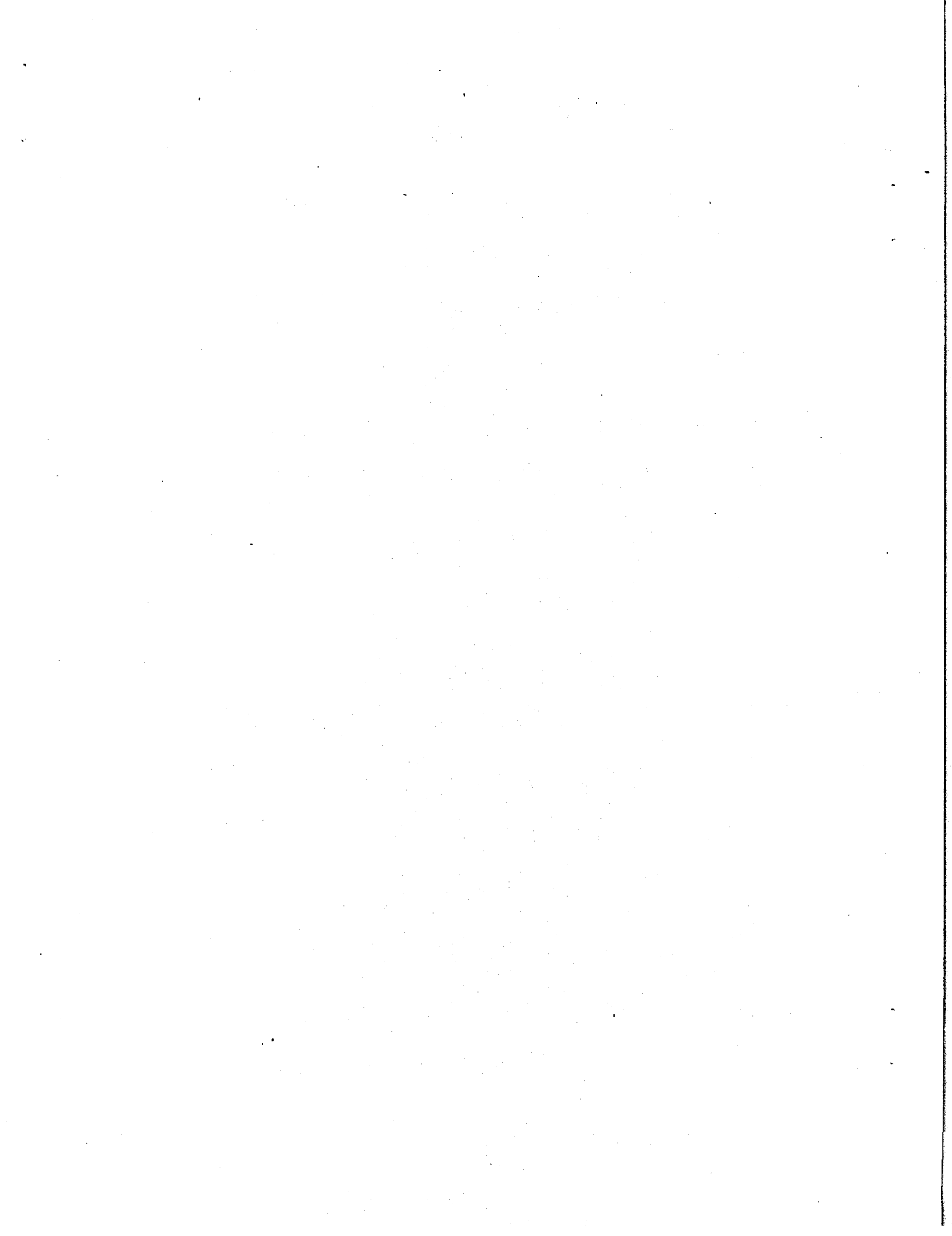
Demonstration projects such as the Charleston Claimant Placement and Work Test Demonstration require the input and cooperation of many individuals, and this demonstration was not an exception. Numerous federal, state, and contractor staff played important roles in designing and implementing this demonstration. A somewhat smaller but still sizeable number of individuals provided assistance to us in developing and implementing the impact evaluation, and we wish to thank those individuals here for their help.

Our project officers, Norm Harvey, who initiated the project and guided it through most of its life, and Bill Showler, who assisted Norm and took over for him towards the end of the project, provided helpful comments throughout the project, and their active interest in the research and our findings was greatly appreciated. At DOL, we also received useful comments on the evaluation design and early findings from Irene Lynn and Burt Barnow. Helen Manheimer's careful reading of our draft final report was also appreciated. Susan Bloom of Bloom Associates, who was coordinator of the Job Service Demonstration Offices Program, played an active role in designing the evaluation. Terry Johnson of SRI International, the implementation contractor, also played an active role in designing the evaluation, and his insights into the operation of the demonstration were particularly useful to our analysis. His extensive comments on our draft final report were also very helpful.

At the state level, we were helped immeasurably by the superb job performed by the data processing staff, particularly Bob Shipley and Dale Spires, in providing us with data. Unlike most evaluations, the time between the occurrence of events and the availability of data on those events for evaluation purposes was extremely short, and thus their assistance enabled us to monitor demonstration outcomes on an ongoing basis. Gary Crossley, Jimmy Jones, and Sharon Postel were also very responsive in helping us obtain data. Finally, the insights of local Job Service and UI staff, particularly Betty Jo Renau, Iris Roberts, and Roy Ferguson, were quite useful to our interpretation of the data.

At MPR, David Long made an important contribution to the evaluation by overseeing the collection of administrative cost data and writing the cost chapter of this report. Myrna Thrower carefully collected data in Charleston on administrative costs and nonmonetary determinations. Meg Beardslee cheerfully performed all the necessary programming, and Annette Kasprack provided her usual expert assistance in the production of this report. Finally, Thomas Good carefully edited the report and oversaw its final production.

Walter Corson
Walter Nicholson -
Principal Investigators



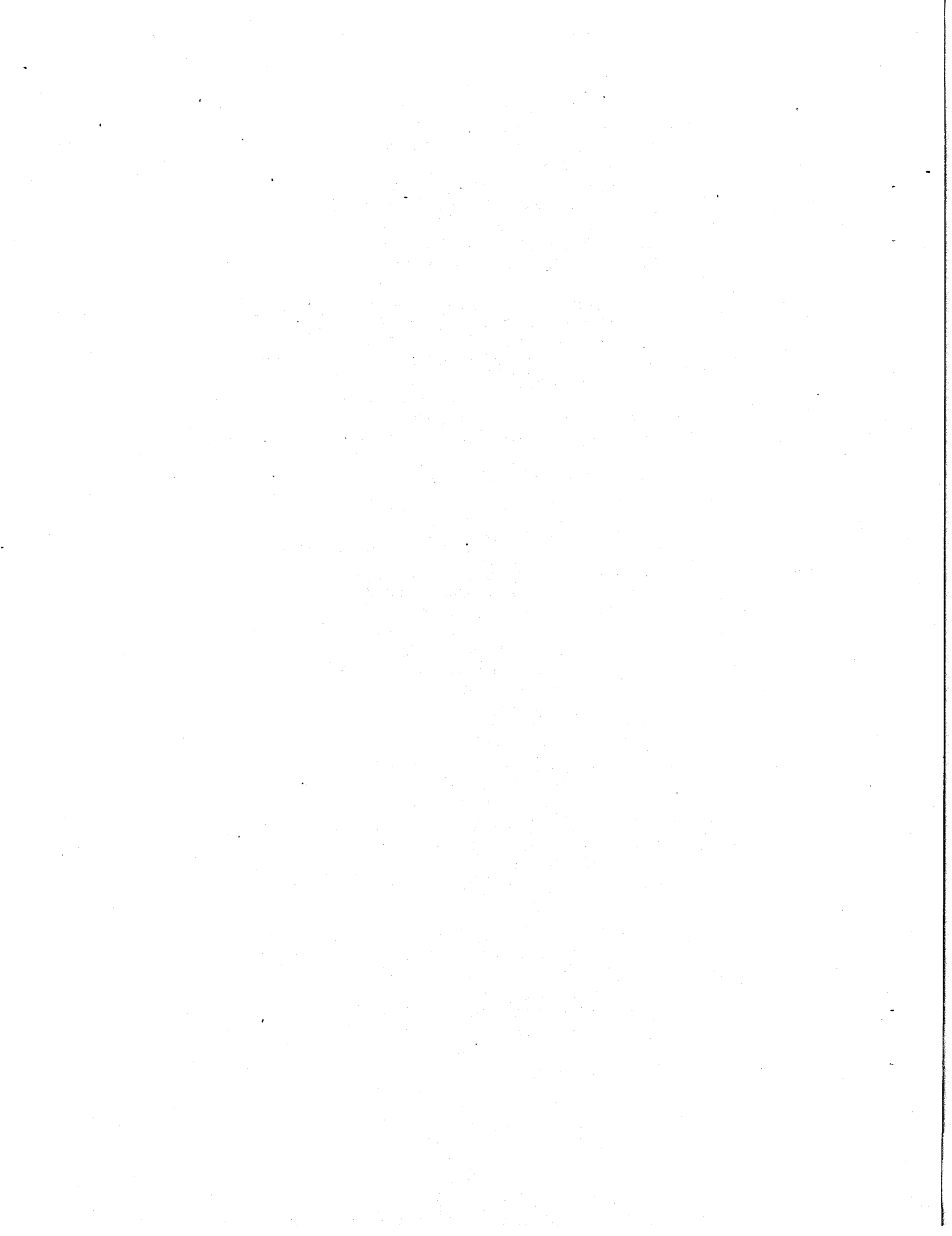
ABSTRACT

The objectives of this project were to improve joint Employment Service/Unemployment Insurance (ES/UI) procedures for monitoring the work test and enhancing placement services for UI claimants in order to reduce weeks claimed and benefits paid. The demonstration consisted of dividing all claimants in the Charleston, South Carolina area into three demonstration groups and one control group on a randomly assigned basis during the period February through December, 1983. Over 5,000 claimants participated in the experiment.

The Treatment Groups (Groups 1 through 3) received enhanced monitoring of the work test by being called in to the local ES office by a computer-generated letter sent at the time their first benefit check was mailed and by improved ES/UI coordination. The call-in letter informed these claimants that no further checks would be issued until they reported to their nearest ES office. Treatment Group 1 received an enhanced placement services interview which provided either a job referral or a job development attempt. Each claimant in this treatment group who was still receiving benefits was sent a call-in letter again four weeks later and given an opportunity to participate in a three-hour job search assistance session. This group was sent a final call-in letter for a second enhanced placement interview after receiving their ninth UI benefit. Treatment Group 2 received the two call-ins for enhanced placement interviews, but not the call-in for the job search assistance session. Group 3 received only one call-in shortly after receiving their first benefit check and received the same services as all other ES applicants. Group 4, the control group, was told about the services available at the ES offices but not required to report. All groups received the standard Eligibility Review Program services.

Results showed a statistically significant average reduction of about 0.5 weeks claimed and net savings in UI payments of \$56 per claimant for Treatment Group 1, and a \$46-\$48 per claimant reduction for the other two treatment groups after deducting estimated administrative costs. Much higher average reductions were obtained for males and especially for those from the construction industry. (Reductions of about 2 weeks in benefits claimed were obtained for construction industry claimants.)

Savings seem to have been mostly due to an increase in non-monetary determinations and denials and to persons failing to continue to apply for benefits in the early weeks of their claims as a result of the improved computer assisted monitoring of failure to report to the ES offices rather than to the improved placement services provided.



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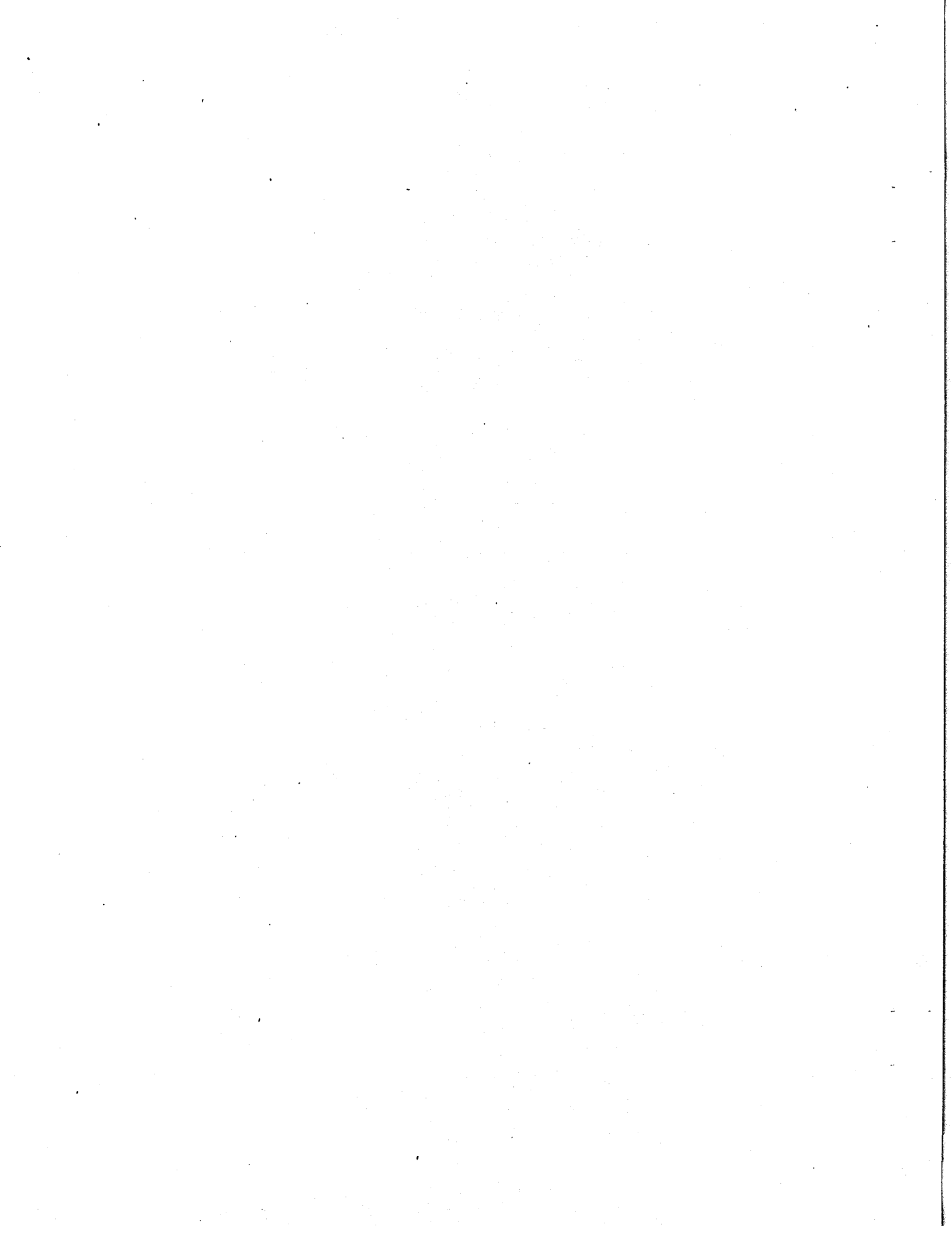
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EXECUTIVE SUMMARY

This report summarizes the results of the Claimant Placement and Work Test Demonstration conducted in Charleston, South Carolina from February to December 1983. The purpose of the demonstration, which involved nearly 6,000 unemployment insurance (UI) claimants, was to develop and implement new operating procedures for the Job Service and UI that would both improve UI "work test" monitoring activities and provide enhanced employment services to UI claimants. It was hoped that these procedures would be cost-effective from the overall perspective of the UI system--that is, it was hoped that the reduction in UI benefits effected by the new demonstration procedures would exceed the increased administrative costs involved. If that proved to be the case, implementing these procedures elsewhere (suitably modified to meet local circumstances) might be considered.

The Charleston demonstration focused on the interaction between the UI system and the Job Service--a relationship that has been characterized by recurrent difficulties since the 1930s. Most of these problems have been caused by the sometimes conflicting roles assigned to the Job Service relative to UI claimants. On the one hand, the Job Service is charged with helping claimants find jobs. In that respect, the Job Service acts in the same role as labor-market intermediary that it does for any other worker who uses its services. On the other hand, the Job Service is also charged with monitoring the continuing eligibility of UI claimants for benefits, by gathering information on their availability for work. In practice, this function has meant that many states have required large numbers of UI

claimants to register with the Job Service, even in cases (as for job-attached workers) where the services being offered might be of little benefit to the individuals involved. These conflicting roles have led to sometimes spotty Job Service monitoring of the UI work test and has compromised the ability of the Job Service to provide services to those UI claimants who would truly benefit from them.

The new procedures implemented in Charleston were intended to mitigate these problems. By delaying required Job Service registration until the first weekly UI benefit payment was received, large numbers of ineligible and job-attached claimants were exempted from the requirement, thereby focusing the attention of the Job Service on those who might benefit most from its services.¹ Concurrent with delayed registration, procedures for monitoring and reporting the failure of the claimants to register were tightened and regularized (by computer checking). Failure to register led to the cessation of UI benefits, at least until claimants could verify that they were indeed available for work. Other communications between the Job Service and UI about availability for work issues were also improved. For claimants who did register at the Job Service, two types of special services were offered in Charleston. First, claimants were offered enhanced placement services through more intensive interviewing and greater job-referral and job-development activities. Second, those claimants who remained unemployed for four weeks were also offered participation in a three-hour workshop whose goal was to teach more

¹ Claimants in Charleston who are on mass layoff and whose employer indicates that they will eventually be recalled were also exempt from registration--a practice that had been followed for some time in Charleston and, indeed, is followed in many other UI jurisdictions.

effective job-search skills. Since both of these services involved making special call-ins to the Job Service,¹ and since, of necessity, claimants participating in the services were more closely observed by the Job Service, the availability of these services also increased the ability of the Job Service to provide information to enforce the UI work test.

In order to evaluate these new procedures, participants in the demonstration were randomly assigned to one of three treatment groups or to a fourth control group that followed a well-defined set of procedures that were similar to the predemonstration procedures used in South Carolina. All individuals in the three treatment groups were subject to the regularized initial Job Service reporting requirement and to improved communications between the Job Service and the UI service about possible violations of availability for work; that is, all were subject to a "strengthened" work test. In addition, claimants in treatment group 2 were offered enhanced placement services, whereas those in treatment group 1 were offered both those services and the opportunity to participate in the job-search workshops. Hence, the design facilitated an appraisal of the effectiveness of the new procedures individually, by drawing comparisons among the treatment groups or by comparing one treatment category with the control group. Sample sizes available in the demonstration were sufficiently large to be able to detect fairly small differences in such outcomes as weeks of UI benefits received or claimants' reemployment rates with a high level of statistical power.

¹ Specifically, claimants eligible for the job-search workshop were called in after receiving their fourth week of UI benefits, whereas those who received enhanced placement services were called in a second time when they received their ninth weekly UI benefit.

The principal results from implementing the changed Job Service procedures in Charleston were as follows:

- o The demonstration treatments, as expected, led to increased use of the Job Service among UI claimants. This increased use was reflected primarily in an increase in the percentage of claimants receiving job referrals or job-development attempts, and the impact was more pronounced for claimants who were assigned to the enhanced placement treatment groups than for those who were assigned to the regular placement treatment.
- o About 25 percent of the claimants who were subject to the initial call-in to the Job Service and 20 percent of those who were subject to additional notices failed to report within the required time periods. These individuals were sent notices to report to UI for fact-finding purposes, and about 40 percent ultimately did report. Most of these claimants then reported to the Job Service and received the appropriate services. Although inactive claimants were told not to respond to the notices, the percentage who did not report was higher than can be explained by this reason. It is clear that some individuals who were still filing claims, or who otherwise would have filed claims, did not report as instructed.
- o As expected, the demonstration treatments increased the percentage of claimants who had a nonmonetary determination. Although the rate at which these demonstration-generated determinations led to a denial of benefits was lower than the rate for non-demonstration-generated determinations, the overall denial rate increased substantially. The denial rate for claimants in the experimental treatments was double the rate for the control group, with the difference being slightly more pronounced for the two treatments who received the most special services. This increase in denials did not appreciably raise the number of appeals filed by claimants.
- o Claimants in the enhanced placement treatment groups obtained significantly (3 to 4 percent) more Job Service placements than did those who were not selected for such services. Most of these additional placements were obtained by males. For females, there were few significant differences in the levels of placements among the treatment categories.

- o Information from UI wage records showed few significant or consistent employment impacts of the demonstration treatments. Both the percentage of claimants who were reemployed one or two quarters after they started to receive UI and the quarterly wage received by these claimants did not seem to be affected by either the strengthened work test or the special employment services. In particular, the effects of the additional placements received did not appear in the overall employment statistics.
- o Each of three experimental treatments had a significant negative impact on weeks of UI benefits collected. The reduction in weeks collected ranged from about one-half a week for the group that received the strengthened work test to only three-quarters of a week for the group that received additional call-ins for the job-search workshop and a second placement interview. Data on the distribution of weeks collected showed that significantly higher fractions of claimants collected only one or two weeks of benefits in the treatment groups relative to the control group.
- o An examination of the characteristics of claimants affected by the demonstration showed that the impact of the treatments was significant for men but not women. Among men, there was some evidence that the impact was smaller for those who expected to be recalled by their former employers than for those without such expectations. There was also evidence that the treatment impact was concentrated among claimants in the construction industry, and this industry phenomenon appeared to be a more important explanatory factor than recall expectation.
- o Estimates of the per-claimant additional administrative costs associated with the treatments showed that these costs were modest, ranging from \$4.00 to \$16.00 for the various treatments. The major factor contributing to the additional administrative costs was the increased use of the Job Service associated with the treatments.
- o The net impact of each of the treatments on UI costs was negative, with the increase in administrative costs being more than offset by the decrease in the cost of benefit payments. The net impact was a reduction of \$56 per claimant for the strengthened work test, enhanced placement, and job-search workshop treatment, and a \$46-\$48 per claimant reduction for the other two treatments. Thus, the experimental treatments proved cost-effective, and their impact on UI costs was sizeable.

Thus, the general conclusion to be drawn from the Charleston demonstration is that the way in which the Job Service monitors the UI work test does indeed matter. Strengthening and regularizing the way in which initial Job Service registration was handled in Charleston and stopping UI payments for failure to report appeared to be a cost-effective procedure. On the other hand, the effectiveness of the enhanced placement services and the job-search workshops instituted in Charleston was ambiguous, since no clear-cut relationship to reemployment success could be established. It is uncertain whether the procedures adopted in Charleston would have effects similar to those observed in the demonstration if they were implemented in other UI jurisdictions. The results of such replications would appear to depend importantly on how Job Service registration for UI claimants is currently handled in such jurisdictions, and on the general nature of the local labor market and prevailing UI statutes.

CHAPTER I

INTRODUCTION

The system of state Unemployment Insurance (UI) programs provides financial assistance to workers who have recently been separated from their jobs and who exhibit continuing attachment to the labor market. In order to ensure that UI funds are used most efficiently, the Employment Service (ES) is charged with both helping claimants find jobs and developing information with which to assess their continuing labor market attachment. In this report, we analyze an experimental effort to improve this process--the Charleston Claimant Placement and Work Test Demonstration. The project, which operated from February to December 1983, provided an opportunity to examine a variety of administrative practices and claimant outcomes in a carefully designed setting. As we will subsequently show in more detail, the results of this demonstration appear promising, and DOL has begun to consider implementing similar demonstrations elsewhere. The state of South Carolina has also implemented a modified version of the demonstration treatments statewide.

In this introductory chapter we provide some background to the Charleston project. Section A describes the historical and policy environment within which the demonstration operated. Section B outlines the specific objectives of the demonstration and discusses how these objectives pertained to the more general programmatic goals of the UI system. Section C provides an overview of the remainder of the report.

A. BACKGROUND

To remain eligible for benefits, as required by all state UI laws, a claimant must be able to work, be available for work, and must be free of disqualification for causes, such as failure to seek work or refusal to accept suitable employment. A claimant who is unable to work or unavailable for work may be declared ineligible for UI benefits for as long as the condition continues, whereas disqualifications for cause last for a period explicitly specified in state laws. The process of identifying noncompliance with these continuing eligibility standards is termed "eligibility determination"—or, in more common, if less precise terms, the administration of the UI "work test." In general, states differ widely in how they both define and administer these eligibility standards.

When the UI system was established in the 1930s, it seemed only reasonable that the system's operations be integrated with the existing Employment Service. Such coordination, it was believed, would provide UI recipients with maximum exposure to existing job openings and, through reports of refusals of suitable job offers or evidence of failure to seek work actively, would enable the ES to provide objective information which could be used to enforce the UI work test. Hence, by assigning to the ES the twin role of job-search assistance and work-test monitoring, it was believed that UI benefit payments could effectively be monitored and controlled.

However, attempting to implement this dual role in the UI/ES relationship has posed several difficulties over the past 40 years. Many of these problems are caused by an inherent conflict between the desire of the ES to act as an efficient labor-market intermediary and the

inefficiencies that arise in requiring the ES also to monitor job-search behavior.¹ For example, compulsory ES registration by UI recipients may lead to a vastly expanded use of ES services by job-attached individuals who may not in fact benefit from those services. Similarly, having the ES enforce job-search requirements may create a conflict of interest between its goals and the goals of the clients it seeks to serve. On purely administrative grounds, the UI/ES relationship also poses problems in that information flows between the two agencies may be imperfect. For example, ES workers might not be aware of all relevant UI statutes and regulations, and would thus be unable to document suspected violations adequately. In addition, ES workers may have little incentive to gather the type of information required by UI officials, particularly if they feel that action against claimants who appear to be violating the regulations is unlikely.

The recent financial difficulties experienced by many state UI trust funds have amplified these issues. As a result of the 1982-83 recession, outstanding federal loans to UI jurisdictions totaled more than \$10 billion, and many states were forced to institute sharply increased UI tax rates to keep their systems solvent. Although a more complete and efficient enforcement of UI statutes would not eliminate such financial problems, the potential capacity of the system to reduce overpayments seems quite probable. For example, a recent GAO study (1984) of random audits of UI claimants in five states cited overpayment rates ranging from 7 to 24 percent and estimated that annual overpayments in these states alone amounted to nearly \$400 million. Since, as clearly established in that

¹ For a brief historical summary of the nature of this conflict, see Stevens (1980), pages 53-54.

report, the most frequent reason for overpayments was the failure of claimants to seek work actively, a tightening of UI administrative procedures appears to offer one route for lowering benefit expenditures. Not only might such a procedure reduce expenditures directly by increasing the likelihood of disqualifying workers who violate state regulations, but it may also deter some UI claims being made by individuals who know they cannot meet these regulatory requirements. Of course, it is always possible that claimants may modify their behavior in a manner whereby they comply with the law but still do not exhibit any more immediate return to work. In that case, no benefit savings would be accrued, and the additional monitoring costs would represent a net drain on the UI system. Little is known about the size of all such effects. Whether increased monitoring of the UI work test through the ES would meet a strict benefit-cost test and whether such an activity can be integrated within the traditional labor-market exchange functions of the ES is currently an open question. Examining that question was the primary, overall objective of the Charleston project. In the next section, we describe how that objective was to be attained.

B. OBJECTIVES OF THE DEMONSTRATION

The overall purpose of the Charleston demonstration was to introduce specific changes in ES and UI operating procedures in a manner whereby the possible effects of the changes on the outcomes of claimants could be analyzed in an experimental setting. The specific procedural changes to be implemented fell into three general categories:

1. Strengthened monitoring of the UI work test through the ES
2. Enhanced placement of UI claimants by the ES
3. Increased ability of UI claimants to find work on their own

While all these changes were generally designed to improve the interactions between the ES and the UI system (thereby introducing savings to the UI trust fund), rather different theoretical perspectives underscored each of them. In this section, we briefly review these perspectives. Later, in Chapter II, we describe the design of the demonstration in detail and indicate those specific services that were implemented to achieve these goals.

The proposition that the UI work test can be better monitored through the ES is founded primarily on the belief that current monitoring efforts are rather modest. Even in those states that require UI claimants to register with the ES, large numbers of claimants are exempt from this requirement (possibly for good reasons), and often little if any action is taken against claimants who do not register even when required to do so. Other avenues by which the ES monitors the UI work test are largely incidental to its primary labor-exchange function. For example, the refusal of a job offer from an ES referral may raise an "availability for work" issue, but ES staff are unable consistently to follow-up on all referrals to gather this information. Direct tests of job-search activity or of refusals of suitable work are administered even more rarely. Empirical evidence also suggests that monitoring efforts are relatively modest. Determination rates (that is, the rate at which nonseparation, nonmonetary eligibility issues are investigated) average about 20 per one-

thousand claimant contacts nationally, with some states (including South Carolina, the site of the present demonstration) exhibiting rates that are well below 10 per 1,000.¹ Since, as shown in Section A, random audits of the UI population report much higher rates of failure to seek work actively, it is presumed that devoting additional resources to the monitoring process might be worthwhile. The Charleston demonstration examined some ways for doing so by focusing on the interaction between the ES and UI. Other work-test monitoring efforts that occur solely within the UI system (e.g., as part of the weekly claims process or during eligibility reviews) were not affected by the demonstration.

Prior evaluations of the labor-market services provided by the ES have been characterized by severe methodological problems that have occurred primarily because the data used in these evaluations are largely nonexperimental, having been collected from actual ES operating records. Differentiating between those factors that lead to the use of ES and those that determine the claimant's experiences has proved to be very difficult with such nonexperimental data.² Nevertheless, there are strong a priori reasons for believing that the information and other services provided by the ES can speed a claimant's return to work. That judgment applies especially to claimants who have relatively inadequate job-search skills and access to job-relevant information. Thus, a major goal of the Charleston Claimant Placement and Work Test Demonstration was to design a way to provide job-placement and job-search services to UI claimants within

¹ These figures are taken from Corson, Hershey, and Kerachsky (1984).

² See Katz (1977) for a discussion of this problem.

a context that might facilitate an evaluation of the impact of those services. Moreover, the demonstration focused on targeting those services toward groups of claimants who appeared most likely to benefit from them. By adopting such procedures, it was hoped that the prior problems caused by large numbers of UI claimants imposing demands on ES-provided services that were not really appropriate for them could be mitigated.

Still another general goal of the demonstration might be mentioned-- to improve UI/ES coordination. This goal was in part necessitated by the other principal goals. By designing procedures to improve such coordination, it was hoped both that the UI work test could be enforced more effectively and that the ES could provide services to UI claimants more efficiently. Prior attempts to achieve such results may have been unsuccessful because they addressed only ES or only UI concerns, without devising ways to improve the institutional relationship between the two agencies. In the next chapter, we briefly describe some of the operational procedures that were implemented in the Charleston demonstration to address this situation.

C. OUTLINE OF THE REPORT

The remainder of this report consists of four additional chapters. In Chapter II, we provide a summary of the design of the Charleston demonstration and show how that design can yield conclusions about whether the demonstration met its goals. We also provide some basic background information on the nature of the Charleston labor market, the UI laws and administrative activities in South Carolina, and the specific sample of claimants studied in the demonstration.

Chapter III provides the basic analytical core of this report. It begins by describing some procedural impacts of the demonstration, such as how many claimants failed to respond to the notifications incorporated in the strengthened work test, and how many received ES-provided services. Demonstration statistics on nonmonetary determinations and benefit denials are also examined. Following this descriptive analysis, Chapter III focuses explicitly on the outcomes of the demonstration--most importantly on the employment record of UI claimants in various treatment categories and on the overall impact of the demonstration on UI benefits paid.

Chapter IV complements these analyses of claimants' responses to the demonstration by enumerating the administrative costs of the various activities. Some nonquantitative analysis of problems in implementing the treatments is also provided. Then, in Chapter V, this administrative information is combined with the data on demonstration outcomes to develop an overall assessment of the project.

CHAPTER II

DESIGN OF THE DEMONSTRATION

In this chapter, we briefly summarize the design of the Claimant Placement and Work Test Demonstration. The general purpose of this summary is to provide sufficient background for interpreting the subsequent analytical results in the proper policy perspective. A more detailed discussion of the operational procedures instituted during the demonstration is contained in the final report of the implementation contractor (see Johnson et al., 1984). The present chapter is based primarily on that report.

In Section A of this chapter we provide some general background about the Charleston labor market and describe why this location was chosen for the demonstration. We also describe the UI and ES operating procedures used in that location prior to the demonstration. In Section B, we summarize the actual services that were implemented as part of the demonstration and indicate how these services differ from prior practices. We also briefly summarize some of the issues that would arise in attempting to replicate these changes elsewhere. Finally, in Section C, we outline the formal design of the demonstration by describing how the various services were assigned to claimants and how the particular target groups to be included were defined. A brief summary of the characteristics of claimants participating in the demonstration is also provided.

A. BACKGROUND ON THE CHARLESTON SITE

Charleston was chosen as the site for the Claimant Placement and Work Test Demonstration primarily because the three Job Service offices in

that site had been participating in the Job Service Demonstration Office Program since 1981.¹ As part of that program, the Charleston offices had undertaken a pilot test of new self-registration procedures. Thus, they were somewhat experienced with adopting innovative operating methods and were familiar with the necessity of evaluating these procedures. Although no strong claim was made that the Charleston offices were "representative" of Job Service offices nationally, both claimant characteristics and office operating procedures are similar to those found elsewhere (see the discussion of these issues below). In addition to the prior demonstration experience of the local Job Service offices, considerable support for the demonstration was expressed at the state level. In particular, the head of the state agency was committed to improving the coordination of ES and UI activities, and his support of the demonstration was critical to its successful implementation.

Two constraints on demonstration activities further enhanced the potential policy significance of the demonstration. First, although the South Carolina Employment Security Commission was given a modest grant of \$25,000 to defer some demonstration-related expenses, these funds could not be used for additional staff positions. Consequently, alterations in operating procedures within the demonstration occurred under a relatively fixed budgetary framework. Second, the state of South Carolina stipulated that the demonstration could be conducted only if the new procedures to be implemented were integrated appropriately with existing UI and ES operations. Hence, designers of the demonstration were not free to develop

¹ The three offices were Charleston, North Charleston, and Moncks Corner.

entirely new and untried procedures but, instead, were to make only marginal alterations to current methods. Because South Carolina Job Service and UI operating procedures are broadly similar to those of many other states, it is possible that the services which were considered effective in the demonstration could, with minor modifications, be implemented elsewhere at minimal incremental budgetary cost.

The Charleston Job Service and UI offices serve three counties (Charleston, Dorchester, and Berkeley) that constitute the Charleston/North Charleston SMSA.¹ The population of the SMSA is about 400,000, with a labor force of approximately 180,000. During 1983, the unemployment rate for the Charleston/North Charleston SMSA averaged about 8.2 percent, as compared with 9.6 percent for both the entire state of South Carolina and the nation as a whole. Although the unemployment rate for Charleston was somewhat below the national rate, it was still far above the approximate 5 percent rate that prevailed throughout most of the 1970s. Therefore, the labor market in 1983 cannot be regarded as a strong one. During 1983, however, the unemployment rate fell by more than two percentage points, and overall employment rose significantly. Hence, in interpreting some of the results of the demonstration, it is important to keep in mind this improving labor market. The Charleston labor force consists of a higher fraction of minority-group members (approximately 30 percent) than does the nation as a whole. Government employment is also more important in Charleston than in the nation generally (respectively, 31 percent of the

¹ The same area served by the three Job Service offices is served by a single main UI office with an itinerant office staffed two days a week in the rural part of the SMSA.

employment in 1980 versus 18 percent). Conversely, the manufacturing industry is less important, constituting only approximately 13 percent of total employment, as compared with 22 percent nationally.

During fiscal year 1982, the three Charleston Job Service offices processed a total of 36,000 applicants, approximately 38 percent (13,600) of whom were UI claimants. Among the claimants, approximately 8,700 were new or renewal ES applicants. In most respects, this client caseload resembled the national ES caseload, with the notable exception that the Charleston caseload consisted of a higher proportion of minority applicants. Characteristics of jobs listed and of referral and placement activities were also largely similar to national patterns, with the exception that, because the Charleston Job Service participates in an important substitute teacher program for the public schools, a significant fraction of placements tend to be short-term in the "public-administration" category.

During the same period (FY 1982), almost 40,000 UI initial claims were filed in Charleston, and 13,600 UI first payments were made. By comparing this latter number to the number of new ES applicants who were claimants, we can calculate that, at most, 65 percent of individuals who received a UI first payment registered with the Job Service prior to the demonstration. The actual registration percentage was probably lower, since it is likely that some claimants who did not receive a payment registered with the Job Service.

Although the normal operating procedures used by the Charleston Job Service and UI offices prior to the demonstration are described in detail in the SRI implementation report (Johnson et al., 1984), the procedures

that pertain to monitoring the UI work test can be summarized here.¹ In monitoring the work test, the UI and ES offices interacted in two primary ways. First, all UI claimants (except the approximately 30 to 40 percent who filed as part of a mass claim)² were instructed to register with the Job Service. Although copies of the UI applications of these claimants were sent to the Job Service, relatively little was done to monitor which claimants failed to report--in part because copies of applications were provided for all claimants (including those who were not required to report) and were not very useful for identifying those who did not report, and in part because some ambiguity existed about exactly when and how claimants were required to register.

Second, the Job Service offices used a "Form 263" to inform UI about potential eligibility violations. The form was forwarded to UI for any individual who refused a job referral or job offer, failed to answer a call-in or to report to work, had a restriction on work availability (such

¹ The UI office was of course also involved in monitoring the work test independently of the Job Service. Two general activities occurred. First, claimants, on their weekly mail claims, were required to affirm that they were "able, available, actively seeking work, registered for work, and did not refuse any job offer," although no specific information was necessary (such as a list of employers contacted). Second, claimants were scheduled for periodic eligibility reviews, at which time they completed a questionnaire that focused on issues pertaining to availability for work and job-search activities. These procedures were not changed by the demonstration, although some efforts were instituted during the demonstration to verify the job-search activities reported during the eligibility review. This activity applied equally to claimants in all the demonstration treatments, and it should not affect the comparisons among treatments reported here.

² In South Carolina, mass claims can be filed by employers on behalf of their employees in short, temporary layoff situations. In these cases, the claimants do not go to the UI office, nor are they referred to the Job Service. In 1981, about 30 percent of all initial claims in Charleston were mass claims. and in 1982 the figure was 40 percent.

as transportation or child-care problems), or had returned to work. This information exchange had proved to be less than perfect from both UI and ES perspectives. UI workers often felt that many of the Form 263s were unnecessary or inadequately documented. Conversely, ES staff were frustrated by what they saw as an inadequate use of the information they provided. Thus, a major goal of the demonstration was to rectify this situation.

It is difficult to compare these Charleston procedures with national patterns because of the wide variety of procedures employed elsewhere.¹ With respect to ES registration, for example, the procedures adopted by states (and localities within states) to determine which claimants are required to register with the ES and on how that process is monitored, if at all, vary widely. In general, it appears that, on the one hand, the pre-demonstration Charleston procedures may have led to the referral of somewhat greater numbers of UI claimants to the ES than was true elsewhere, since two groups of job-attached individuals who are routinely excluded from registration (those who, while not on mass layoff, have definite recall expectations and those who usually find work through union hiring halls) were not excluded in South Carolina. On the other hand, because the UI and ES offices are not co-located in Charleston and because ES registration was not a precondition to UI benefit receipt (as it is in some jurisdictions), monitoring of the registration requirement may have been less intensive in the demonstration site than it is nationally.

¹ These various procedures are described in Corson, Hershey, and Kerachsky (1984).

Since even less is known about the general level of ES/UI coordination with respect to other availability issues, it is also difficult to compare procedures adopted in Charleston with those adopted elsewhere. This comparison is made even more difficult by variations among state statutes that define requirements for continuing UI eligibility. South Carolina ranks relatively low in UI determination rates for nonseparation issues and, hence, low in its denial rate.¹ However, whether these rankings should be attributed to relatively loose ES/UI coordination or to other factors (such as the nature of South Carolina laws or to a low level of UI-generated determinations) is unknown. The Corson, Hershey, and Kerachsky (1984) study of the UI work test found that, after controlling for a number of statutory and labor market variables, South Carolina's benefit denial rates for nonseparation issues were lower than those in most states, although most of the differences were not statistically significant. From an overall perspective, the demonstration site was thus probably representative of states that enforce the UI work test only to an average or relatively little extent.

B. SERVICES TO BE IMPLEMENTED IN THE DEMONSTRATION

Achieving the demonstration goals--(1) strengthening the UI work test through the ES, (2) enhancing ES placement activity, and (3) enhancing the ability of UI claimants to find employment on their own--necessitated instituting several new services and procedures not only in the Charleston

¹ In 1982, South Carolina's figure of 7.0 determinations per 1,000 claimant contacts for nonseparation issues ranked 44th among 51 UI jurisdictions. The state's denial rate was 43rd during the same time period.

Job Service and UI offices but also in the central state office. In this section, we briefly describe these services; in the next section, we show how they were "packaged" for evaluation purposes. For ease of exposition, we have organized this section as follows. We first discuss the revised ES registration procedures that were instituted in Charleston, and then show how UI/ES communications were altered, primarily through improvements in how the Form 263s were handled. Together, these services were part of the demonstration goal to strengthen the UI work test. We then describe the procedures that were adopted to increase claimant placements and to teach job-search skills.

1. Revised ES Registration Procedures

The new ES registration procedures that were developed for the Charleston demonstration involved two elements: delayed registration and improved monitoring. Delayed registration was implemented by sending a call-in notice to UI claimants at the time their first UI check was issued, requiring them to register at a local Job Service office within one week or face a possible loss of benefits. Only claimants who had filed as part of a mass claim were exempted, and no notice was sent to this group. This requirement represented a change from previous procedures in several ways. First, it exempted from registration those monetarily eligible claimants who never receive a first check (who number almost 40 percent), thereby helping to alleviate the impact of the registration requirement on Job Service resources. Second, it provided a definite date by which registration was to be accomplished. Third, the registration requirement was explicitly monitored through a computerized matching of UI call-in requests and ES registration files. Finally, when a match was not found,

the claimant was sent a notice to report to UI for a fact-finding interview, and no benefits were paid until he or she had done so. At that time, a nonmonetary determination was issued, and, depending on the results of that determination, the claimant could be denied benefits until he or she registered with the ES, or longer if an availability-for-work issue was identified.

The notices sent to claimants, the matching of UI and ES files, and the cessation of benefit payments were all undertaken through the central office computer facility, with some manual checking of the results in the local Job Service office. Thus, much of the administrative burden was handled by computer, and monitoring claimants' compliance with the registration requirement occurred routinely, in a timely fashion. This process represented a key element of the demonstration treatments.

2. Improved UI/ES Communication and Coordination

Improvements in UI/ES coordination focused on the ways in which the Form 263 was used in Charleston. Additional training was provided to both the UI and ES staff workers who were involved with these forms, so as to clarify which issues were and which issues were not relevant to assessing availability for work under the applicable South Carolina law. Various procedures were also instituted to ensure feedback between local UI and Job Service offices on the relevance and accuracy of the information being transferred, including the development of more detailed forms. This improved information flow was also accompanied by a commitment from UI to take action (by instituting a fact-finding process) when warranted.

3. Enhanced Placement Services

The enhanced placement services offered to claimants during the demonstration involved a variety of changes in specific office operating procedures that were intended to increase placements by providing more intensive services to claimants than were provided previously.¹ Most importantly, all claimants who received this treatment were administered a placement interview, with which they were expected to receive either a job referral or a job-development attempt (if they were considered job ready), and through which they were also placed in the job matching system. In addition, they were trained how to use job information service listings, information on out-of-state job openings (for claimants who were willing to relocate), and current local job information so that they could develop an effective job-search strategy. Claimants who received enhanced placement services were also told that, if they were not reemployed nine weeks later, they would be sent a second call-in notice requiring them to report to the ES for a return visit. Enhanced services were also provided during this second visit.

4. Job Search Workshops

The principal service included in the demonstration to improve the job-search skills of claimants was the job-search workshop. The workshop

¹ During the demonstration, additional efforts were also taken to increase the number of job orders listed at the ES by using information on UI claimants' application forms to determine whether a job opening (not already listed with the ES) existed. Because job orders generated in this way would be available to all ES users (not just to claimants in a particular demonstration treatment), this service could not be specifically evaluated. Rather, its success in generating job orders is described separately in an appendix to this report.

lasted approximately three hours and provided a forum for discussing basic search and interviewing strategies, and for making relevant labor-market information available. A detailed workshop curriculum was developed for these purposes. The workshops were offered to claimants who had been collecting UI benefits for four weeks. Such a delay was chosen as a compromise between the desirability of offering such services relatively early in a claimant's unemployment spell and the desirability of not offering such services to claimants who could easily find work on their own.

These four services were thus intended to meet the goals of the demonstration within the budgetary and procedural constraints outlined previously. We now examine how those services were implemented, so as to evaluate their effectiveness.

C. DESIGN OF THE EVALUATION

In order to evaluate the demonstration services, claimants were randomly assigned (using social security numbers) to the different treatments in Charleston during the period from February to December 1983. In this section, we examine (1) how the claimant sample for the evaluation was defined, (2) how the treatments were assigned, (3) the types of data that were collected, and (4) the general characteristics of the research sample. We conclude with a few comments about the validity and generalizability of the evaluation design.

1. Sample Definition

The potential sample for the evaluation included all UI claimants in the Charleston office who received a first payment between late February

and December 1983, with the exception of mass claimants, as described below. These claimants were categorized into 45 weekly "cohorts," so as to identify the date at which they entered the demonstration. Although it had originally been planned to consider the first three months as a "pilot" test to work out the details of the demonstration services, only very minor changes were ultimately made in the demonstration following the pilot phase. Thus, it was decided that all but the first two cohorts of the pilot sample be included in the final analysis in order to obtain as large a sample size as possible.¹ Thus, the final sample included cohorts 3-45.

Although a number of possible exclusions from this comprehensively defined sample were considered, only one such exclusion was finally adopted—mass layoff claimants, whose employers considered them to be job-attached. Because these claimants could readily be identified (their claims were filed on a special form) and because they were not likely to be affected by the demonstration services, it was decided that they be omitted from the demonstration. Other than this restriction, sample coverage of those receiving a UI first payment was quite complete. In particular, non-mass-layoff claimants who reported a definite return-to-work date (who are often excluded from ES registration requirements in other jurisdictions) were included in the demonstration.²

¹ The first two weeks were excluded to avoid start-up problems.

² In some states, claimants who are union members (and therefore find employment through the union) are also excluded from ES registration requirements. Because South Carolina is a "right-to-work" state, such hiring procedures are restricted by law, and union members are subject to the usual UI work test. Thus, they were included in the demonstration.

2. Assignment of Treatments

For purposes of evaluation, the four services described in Section B were combined into three "treatment" category groupings and a fourth control-group category. Because both the enhanced placement services and job-search workshops required claimants to maintain close contact with the Job Service, it was decided that these services be offered only in conjunction with the revised ES registration procedures and the improved UI/ES communication procedures (we refer to these two services together as the "strengthened work test" treatment). For similar reasons, it was decided that job-search workshops be offered only in conjunction with the enhanced placement activities, thereby maximizing the information flow to claimants in the workshops. Hence, in the final design, claimants were assigned to one of four equal-size¹ treatment groups:

Treatment Group 1

- Strengthened Work Test
- Enhanced Placement
- Job Search Workshops

Treatment Group 2

- Strengthened Work Test
- Enhanced Placement

Treatment Group 3

- Strengthened Work Test

Treatment Group 4

- Control group (no demonstration services assigned)

¹ In the final implementation of the demonstration, it proved impractical to offer job-search workshops to claimants at the Moncks Corner office. Claimants who normally would have been assigned to treatment group 1 at that office were instead assigned to treatment group 2. Overall, then, treatment group 2 was slightly larger and treatment group 1 slightly smaller than one-fourth of the entire sample.

South Carolina's pre-demonstration ES registration procedures (i.e., informing claimants at the time of the initial UI claim benefit rights interview that registration was required, but monitoring this requirement unsystematically) were changed for the control group by informing all claimants at the time of the initial claim that they might be called-in to the ES, but that, even if they were not, they could use the ES on a voluntary basis. The control-group claimants were not sent a call-in notice, and, if they reported to the Job Service office, they received regular as opposed to enhanced services, as did the claimants in group 3. They were also exempted from reporting availability issues on Form 2635. However, all claimants continued to be subject to those UI work-test monitoring activities that were conducted by the Charleston UI office. These changes in South Carolina's pre-demonstration procedures were made for two reasons. First, it would have been operationally difficult to have UI staff identify control group claimants at the time of the initial claim and hold a separate benefit rights interview (these were done in group sessions) for them. Second and more important, it was felt that it was important to follow a clearly defined set of procedures to facilitate drawing comparisons with the procedures followed in other jurisdictions. Thus, estimates of the impact of the strengthened work test procedures cannot be interpreted strictly as the difference between the new procedures and the pre-demonstration procedures; however, our judgment is that the changes that were instituted in the pre-demonstration procedures probably had only a minor impact on important outcome measures, such as weeks of UI benefits received.

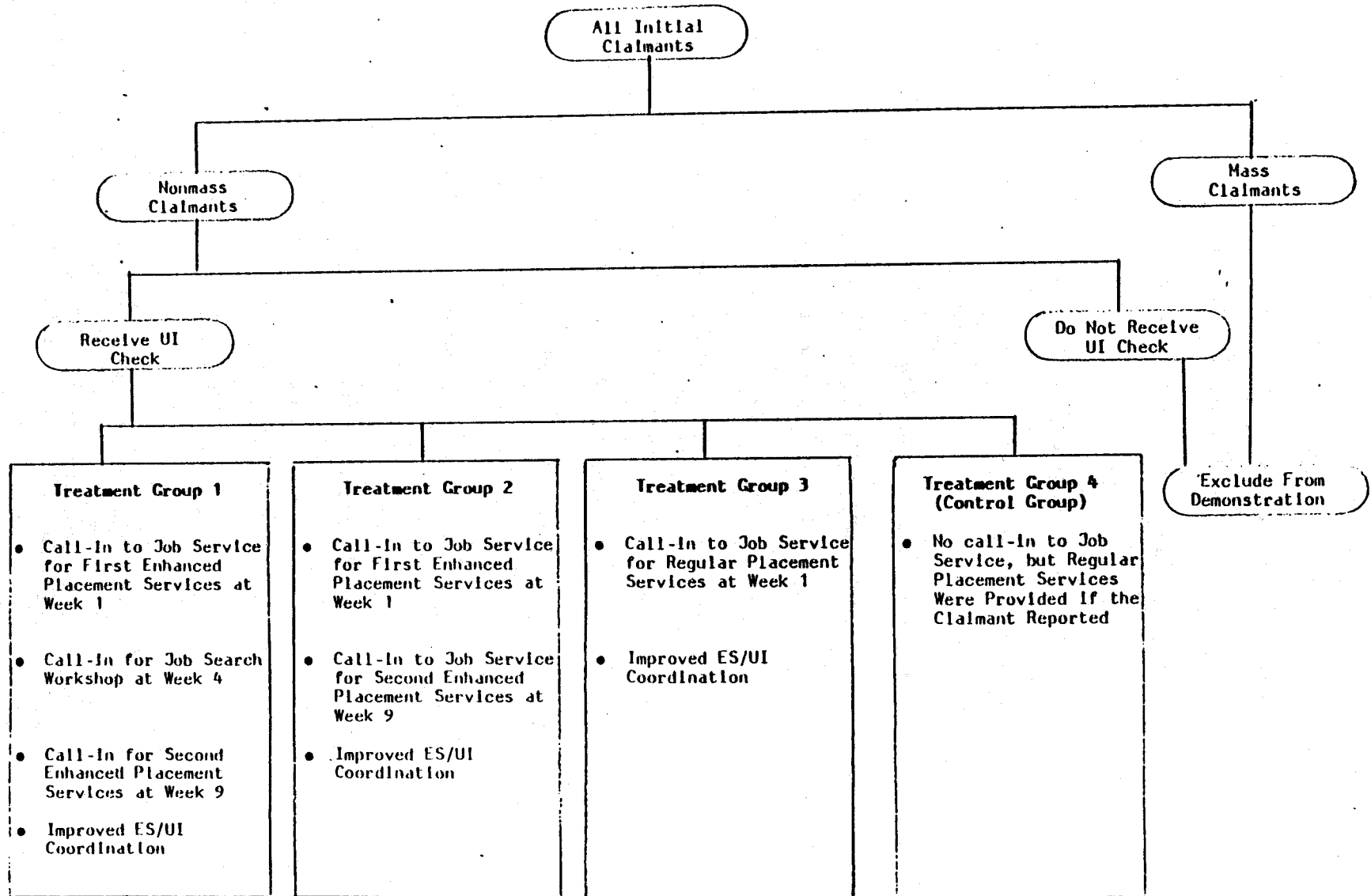
Figure II.1 provides a summary of the assignment process and the treatments. Notice that claimants in treatment groups 1 and 2 received multiple call-ins to the Job Service if they remained active claimants. These additional call-ins were monitored in the same way as the initial call-in, and, hence, additional work test elements were associated with the provision of job-search workshops and enhanced placement. That is, the strengthened work test for treatment group 1 included up to three call-ins for ES services, while in treatment group 3 only one call-in occurred.

The assignment of services among the four treatment groups facilitates evaluating each of the services individually, although, as stated above, work-test elements were associated with the ES services. The effects of the strengthened work test using improved coordination and one call-in can be estimated by comparing the outcomes for groups 3 and 4; the effects of enhanced placement including the two call-ins can be estimated by comparing the outcomes for groups 2 and 3; and the effects of the job-search workshops including the additional call-in can be estimated by comparing the outcomes for groups 1 and 2. Sample sizes in the evaluation were large enough so that relatively small differences between the groups in outcomes such as weeks of UI benefits collected could be detected with a high level of statistical power.¹

¹ Specifically, we estimated that given the expected sample sizes of approximately 1,500 per treatment group, differences between the groups of 1.2 weeks in UI benefits collected could be detected with 90 percent power. Differences of as small as 0.9 weeks could be detected with 70 percent power.

FIGURE II.1

TREATMENT GROUP ASSIGNMENT IN THE CLAIMANT
PLACEMENT AND WORK TEST DEMONSTRATION



NOTE: Weeks refer to weeks beginning with the first UI payment.

3. Data Collection

All of the data for evaluating the Claimant Placement and Work Test Demonstration were derived from regular UI and ES records, with the exception of data pertaining to the demonstration reporting requirements. UI application and benefit payment records provided the basic source of data for the evaluation. These records also included information on nonmonetary determinations, denials, and appeals, although this latter information was available only in hard-copy form. The basic UI records were supplemented in four ways to develop as complete a file as possible on claimants in the demonstration. Data describing the notices sent to claimants provided information on the responses of claimants to the demonstration-initiated reporting requirements. Data from the Employment Service Automated Records System (ESARS) were collected to indicate the nature of services received at the Charleston Job Service offices. UI wage reporting data (when available) were also collected for each claimant in the research sample to provide measures of post-UI earnings. Finally, data from the approximately 20 percent of the claimants in the demonstration who participated in the Continuous Wage and Benefit History (CWBH) system were collected. These questionnaire data provided somewhat more detailed information on claimants' backgrounds (such as their expectation of recall or their family composition) that could be used in some of the analysis. In addition to these details on claimants participating in the demonstration, administrative data were collected on demonstration costs. The methodology used to develop these data is described in detail in Chapter IV.

4. Description of the Research Sample

Table II.1 records the basic characteristics of the sample of claimants used for the evaluation. Included in the table are all claimants in cohorts 3-45, except those for whom one or more of the values for these descriptive variables were missing. In all, this research sample numbered 5,675 individuals.¹ As described above, these claimants were assigned to each of the four treatment groups on the basis of social security numbers, with the 185 claimants in the Moncks Corner office who would have been in treatment group 1 being assigned instead to group 2 for operational reasons.

As might have been expected given the random nature of this assignment, the treatment groups differed very little in their basic characteristics. In all of the groups, the mean age was about 33 years, approximately 44 percent were nonwhite, 42 percent were women, education averaged slightly more than 12 years, and potential UI durations averaged nearly 23 weeks. Weekly benefit amounts and wage-replacement ratios were also quite similar among the treatment categories. For these variables, however, the similarity across the treatment groups masked major differences between men and women for the UI variables. Separate calculations (not reported here) showed that the mean weekly benefit amount for men (\$104) was substantially higher than for women (\$85). Overall, wage-replacement ratios were also significantly lower for men (0.59) than for women (0.69), probably because of the effect of the maximum

¹ The full sample of all claimants in cohorts 3 to 45 totaled 5,921. This larger sample is used for some descriptive tabulations, and the smaller sample is used for analyses that control for claimant characteristics.

TABLE II.1
CHARACTERISTICS OF THE RESEARCH SAMPLE

Characteristic	Treatment Group								Total Sample	
	1		2		3		4		Mean	Standard Deviation
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation		
Age (years)	32.98	10.61	33.48	10.86	33.20	11.19	33.49	11.24	33.30	10.99
Race (= 1 if nonwhite)	0.44	0.50	0.45	0.50	0.44	0.50	0.43	0.50	0.44	0.50
Sex (=1 if female)	0.42	0.49	0.37	0.48	0.42	0.49	0.40	0.49	0.40	0.49
Education (years)	12.26	2.59	12.14	2.60	12.28	2.48	12.13	2.50	12.20	2.54
Potential UI Duration (weeks)	22.90	4.60	23.02	4.44	22.95	4.56	22.92	4.50	22.95	4.52
Weekly Benefit Amount	96.58	26.39	96.41	26.86	96.14	26.54	95.87	26.80	96.24	25.11
Wage Replacement Ratio ^a	0.63	0.25	0.63	0.26	0.63	0.25	0.63	0.25	0.63	0.25
Sample Sizes	1,232		1,598		1,417		1,428		5675	

^a Defined as 52 times the WBA divided by base period wages.

weekly benefit amount in South Carolina (\$118 in 1983). For most of the other variables in Table II.1, the basic characteristics did not differ between men and women in the sample.

For most of the research to be reported in later chapters, the characteristics included in Table II.1 were used as control variables in the analysis. These represented the largest set of background variables available from the regular UI records. As described above, additional background information was available for those in the sample who participated in the CWBH program. However, because sample sizes for this group were considerably smaller, those data were used only occasionally in the analysis.

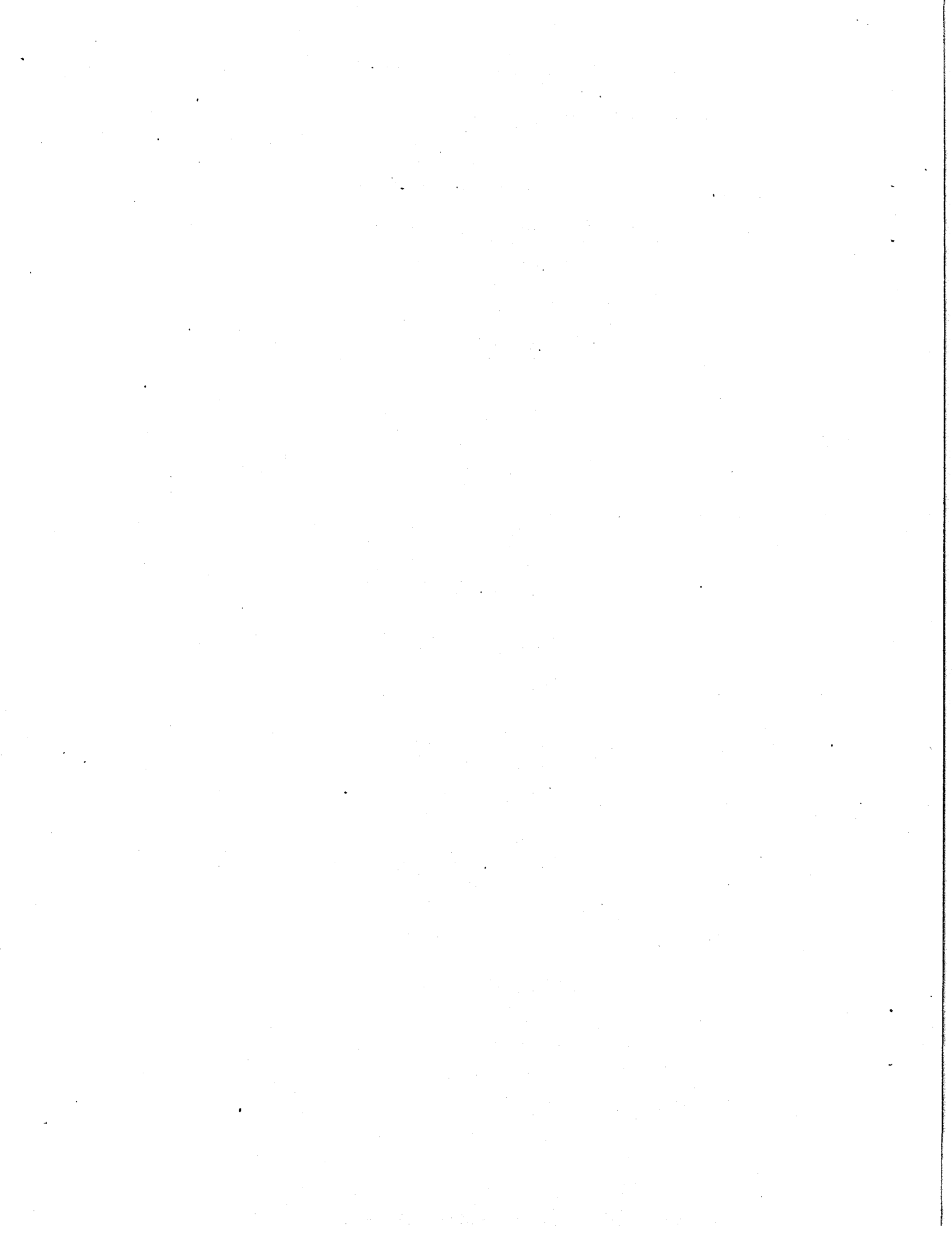
5. Validity and Generalizability of Results

In conclusion, then, the design of the Claimant Placement and Work Test Demonstration possessed a number of advantages. Most important, its use of random assignment facilitated drawing straightforward estimates of the effects of the demonstration services, whereas most prior evaluations have been forced to cope with the major problems involved in using non-experimental data. The treatments were also "packaged" in a manner whereby comparisons among treatment categories offer some insight into the effects of specific components, and the treatments were implemented in what appeared to be a uniform and consistent manner. Hence, the design offered considerable "internal" validity.

Whether the results from the Charleston Demonstration can easily be generalized is more problematic. Although considerable care was taken to develop treatments that could be implemented elsewhere, these were necessarily incorporated into an ongoing administrative system. Probably

none of the treatments could thus be replicated precisely elsewhere.

Charleston's unique labor-market characteristics and its prevailing UI laws and degree of enforcement pose similar problems to developing generalized results. Nevertheless, the strong basic design of the demonstration made it likely that some lessons would be learned from it, and, with care, that those lessons could be generalized to other jurisdictions.



CHAPTER III

ANALYSIS OF CLAIMANTS' BEHAVIOR AND IMPACTS ON CLAIMANTS

In this chapter we examine the impact of the demonstration treatments on claimants. The discussion is largely descriptive, focusing on claimants' interactions with treatment procedures, their use of the Job Service, and their employment and UI experiences. Variations in impacts by treatment group and for various population subgroups are also reported. An analysis and discussion of evidence on the reasons why the observed treatment impacts occurred is reserved for the final chapter.

The discussion is divided into four sections. The first describes the response of claimants to the reporting requirements of the demonstration. This section also examines the services provided to claimants who reported to the Job Service. The second section then examines the impact of the demonstration on nonmonetary determinations, denials, and appeals. The third section discusses employment impacts based on data on ES placements and from UI quarterly wage records. The final section focuses on the primary measure of treatment impacts, weeks of UI collected. It provides a detailed examination of how the impacts vary by treatment group and by various population subgroups.

A. CLAIMANTS' USE OF AND RESPONSE TO DEMONSTRATION TREATMENTS

The impacts of the demonstration treatments on claimants were due largely to the requirements to report to the Job Service. Some claimants who reported also received special services that were expected to affect the duration of UI benefit receipt and subsequent employment. In later sections, we examine the eventual impact of these requirements and services

on employment and unemployment outcomes; in this section, we describe claimants' initial responses through a discussion of both their responses to the reporting requirements and their use of the Job Service.

1. Claimants' Responses to Reporting Requirements

The reporting requirement that affected the greatest number of claimants was the call-in to the Job Service issued at the time of the first UI payment. Table III.1 presents data on responses to this call-in (and to the subsequent ones) for the three treatment groups that were subject to the various call-ins. As indicated, about 25 percent of those who were subject to the initial call-in failed to report within the one-week period required by the notice.¹ These 25 percent were notified to report to UI for fact-finding purposes, and about 40 percent of this group ultimately reported to UI. Most of these claimants then reported to the Job Service and received the appropriate services.

Claimants who also received notices to report for the job-search workshop or their second visit to the Job Service were somewhat more likely to respond; still, about 20 percent did not report within the required time period--two weeks with these notices. As with the first notice, inactive claimants were not expected to report; however, with both the first and second notices, the percentages who did not report were too high to be

¹ As implemented, claimants who had reported to the Job Service within the 30 days prior to the end of the one-week period satisfied the reporting requirement. This meant that claimants who reported to the Job Service prior to the initial notice were not penalized if they did not report again after receiving the notice. This relaxation of the initial rule that required claimants to report after receiving the notice was partially implemented manually during the pilot phase and was implemented automatically by computer thereafter. The data presented here take into account the manual adjustment.

TABLE III.1

CLAIMANTS' RESPONSE TO REPORTING REQUIREMENTS BY TREATMENT GROUP

Reporting Requirement	Treatment Group			Total ^a
	1	2	3	
First Job Service Call-in				
Percent called-in	100.0%	100.0%	100.0%	100.0%
Percent of call-ins who failed to report	26.0	25.1	24.5	25.1
Job Search Workshop Call-in				
Percent called-in	50.7	n.a.	n.a.	50.7
Percent of call-ins who failed to report	19.1	n.a.	n.a.	19.1
Percent of call-ins who reported but left workshop	8.6	n.a.	n.a.	8.6
Second Job Service Call-in				
Percent called-in	46.2	45.3	n.a.	45.7
Percent of call-ins who failed to report	21.2	21.2	n.a.	21.2
Sample Sizes	1,277	1,666	1,493	4,436

n.a. = not applicable

^a Total percent is calculated over treatment groups subject to the call-in.

explained solely by claimant inactivity. Clearly, some individuals who were still filing claims, or who otherwise would have filed claims, did not report as instructed.

A final finding reported in the table concerns attendance at the job-search workshop. Claimants were required to report to the workshop, but South Carolina's UI laws did not permit instituting a requirement that claimants attend the workshop. Thus, individuals who reported but left before conducting the workshop were considered to have fulfilled the requirement, and about 9 percent of the claimants who reported left before completing the three-hour workshop. Thus, these individuals, while satisfying the work-test requirement, probably benefited very little from the workshop.

In addition to the aggregate data on claimant reporting, it is instructive to compare the characteristics of claimants who reported with those who did not, and to examine whether reporting behavior changed during the demonstration. In particular, some South Carolina officials expected that the proportion of claimants responding to the notices would increase over time, as it became evident in Charleston that UI benefits would cease if a claimant did not report as required. We addressed these issues by using a simple descriptive regression that estimated response to the initial call-in as a function of claimant characteristics, UI- and demonstration-related variables, and time (as measured by the cohort number for the claimant).¹ As indicated in the previous chapter, we use this

¹ This equation and subsequent ones that involve a binary dependent variable were also estimated using the Probit technique. Since the ordinary least squares (OLS) regressions were quite similar, we have reported those here because the OLS coefficient estimates can be interpreted more easily than the Probit coefficients.

basic set of explanatory variables for much of our analysis (Table II.1 presented the means and standard deviations and a more detailed definition of each of these variables). We have focused on the initial call-in because all claimants in the strengthened work-test treatment groups received this call-in. Analyses of the other call-ins led to similar results with respect to the characteristics of claimants who responded.

The results of this analysis are reported in Table III.2 for the entire sample and separately for men and women. The results show that, as one would expect, no differences occurred by treatment group, since the initial call-in was identical for each treatment group which received the strengthened work test. However, other characteristics of claimants did affect response. In particular, women had significantly higher response rates than men (by 6.4 percentage points),¹ and the response rate also increased with age. Several other claimant characteristics had a statistically significant impact on the response to the call-in, but in these cases the response was different for men and women. For example, more years of education increased the response rate for men but not for women, and the race variable was significant for women but not for men. Furthermore, a test of the homogeneity of the relationships for men and women showed that it was appropriate to estimate separate regressions for these two groups of claimants. In our subsequent analyses, we also report results separately for men and women, and a common finding is that the

¹ Note that since the dependent variable equalled one if the claimant did not report, as directed, a negative coefficient for a characteristic implied a higher response rate for claimants with the given characteristic.

TABLE III.2
ANALYSIS OF THE DETERMINANTS OF RESPONSE
TO THE INITIAL CALL-IN^a

Explanatory Variables	Men	Women	Total
Treatment Group 1	0.010 (0.434)	0.017 (0.695)	0.014 (0.859)
Treatment Group 2	0.013 (0.654)	-0.029 (-1.197)	-0.001 (-0.074)
Age	-0.002† (-1.781)	-0.002† (-1.889)	-0.002* (-2.474)
Race	-0.010 (-0.536)	0.045* (2.205)	0.013 (0.926)
Sex	— —	— —	-0.064* (-4.555)
Education	-0.008* (-2.491)	-0.001 (-0.256)	-0.006* (-2.218)
UI Potential Duration	0.001 (0.592)	0.001 (0.329)	0.001 (0.483)
Wage Replacement Ratio	0.030 (0.734)	0.167* (2.303)	0.066† (1.899)
Cohort	-0.002* (-3.149)	-0.000 (-0.410)	-0.002* (-2.839)
Intercept	0.425* (4.727)	0.136 (0.896)	0.364* (4.893)
² R	0.005	0.011	0.009
F-statistic	2.434	3.364	5.321
(Degrees of Freedom)	(8,2539)	(8,1690)	(9,4237)

NOTE: The t-statistics are reported in parentheses.

^a The dependent variable equalled one if the claimant did not report to the Job Service as a result of the initial call-in, and it equalled zero otherwise.

*Coefficient statistically significant at the 95 percent confidence level for a two-tailed test.

†Coefficient statistically significant at the 90 percent confidence level for a two-tailed test.

response to the demonstration differed substantially between these two groups.

A final coefficient of interest in the regression is the coefficient for cohort. The coefficient was significant for men but not for women, and it provides some support for the hypothesis that claimants will be more likely to report as instructed the longer a call-in policy is in effect. However, while the response rate increased somewhat throughout the demonstration, a substantial proportion of claimants still had not reported as directed by the end of the demonstration. This finding can be illustrated graphically in Figure III.1, where failure to report to the initial call-in was slightly above 25 percent for the first half of the demonstration, was slightly below 25 percent for a period shortly thereafter, and fluctuated considerably from week to week towards the end of the demonstration.

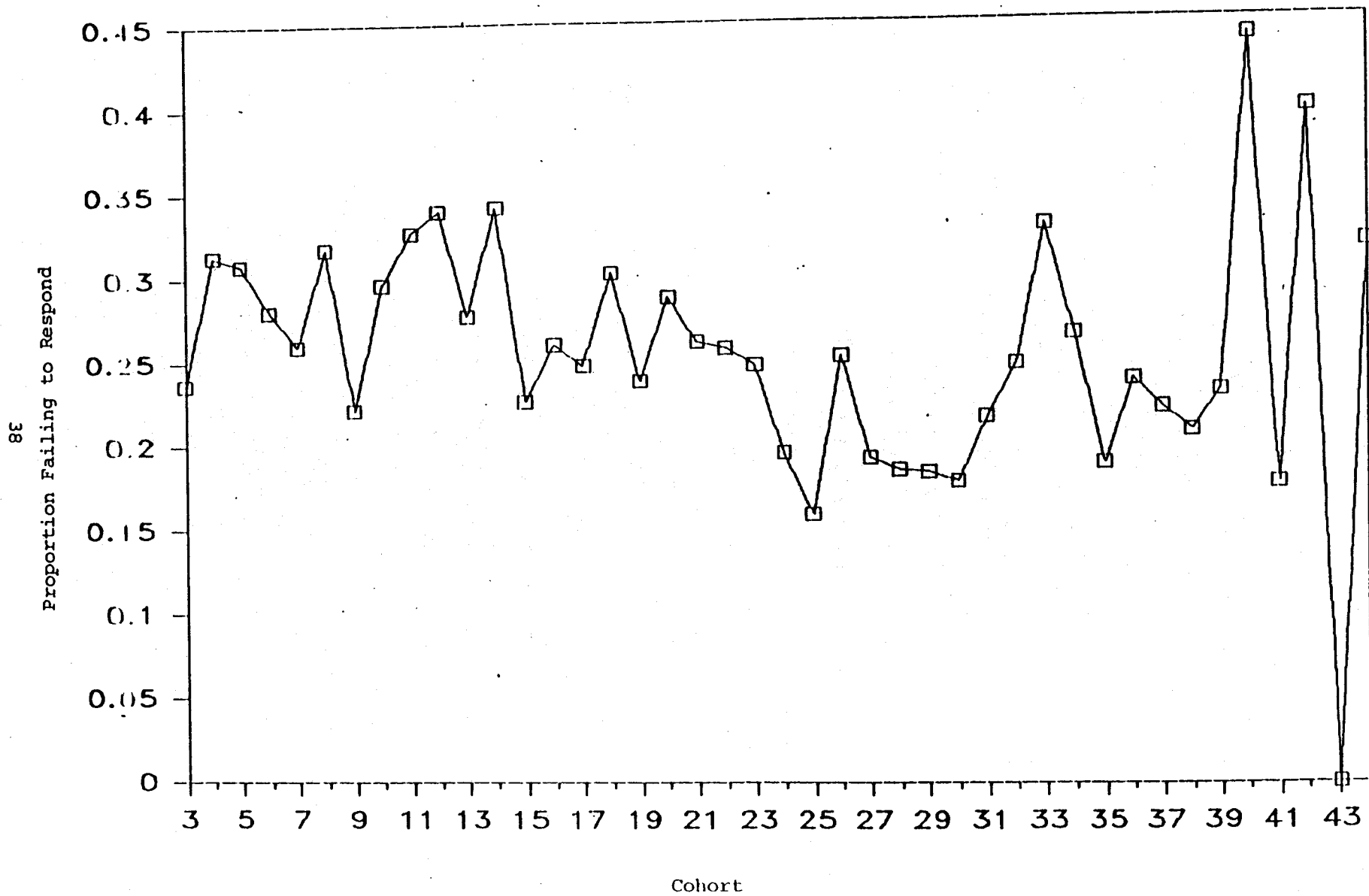
2. Use of the Job Service

In this section, we examine data on the services provided to claimants by the Job Service. Data on the impact of these services, as measured by Job Service placements and obtained employment, are reported later in our analysis of the employment impacts of the treatments.

The treatments provided in the demonstration were expected to affect the use of the Job Service in two ways. First, the call-ins to the Job Service required of the experimental treatment group were expected to lead to a greater level of service use relative to the control group, whose Job Service use was purely on a voluntary basis. In addition, because treatment groups 1 and 2 were called-in more frequently than group 3, their level of use was expected to be greater than for group 3. Second, treatment groups 1

FIGURE III.1

PROPORTION OF CLAIMANTS FAILING TO
RESPOND TO THE INITIAL CALL-IN, BY COHORT



and 2 received enhanced placement services when they reported, as opposed to the regular placement services provided to the other two treatment groups. While a regular placement interview did lead to an appropriate service assignment, an enhanced placement interview was expected to lead to either a job referral or a job-development attempt, as long as the claimant was considered job ready.¹ Since almost all UI claimants should have been job ready, claimants in treatment groups 1 and 2 were expected to be more likely to receive a job referral or a job-development attempt than were claimants in the other two groups.

Data on ES services are reported by treatment group in Table III.3.² These data generally conform to our expectations. Over 85 percent of the claimants in the experimental treatment groups received a service from the ES (as compared with only 35 percent in the control groups); thus, these treatments clearly increased Job Service use. In addition, claimants in treatment groups 1 and 2 were more likely to receive job referrals and job-development attempts than were claimants in treatment group 3, and claimants in all three experimental groups were more likely to receive these services than those in the control group. Moreover, these differences were particularly pronounced for job-development attempts, indicating that this component was an important feature of the enhanced

¹ The enhanced placement interview consisted of several other special components (e.g., the provision of labor market information, TJTC vouchering, if appropriate, and a review of out-of-state job openings if the claimant was interested), but the provision of either a referral or a job-development attempt was the most important.

² Only services received between 30 days before and 180 days after the UI first payment date are reported, so as to exclude services received during unemployment periods other than the ones included in the demonstration.

TABLE III.3

JOB SERVICE USE BY TREATMENT GROUP

Reporting Requirement	Treatment Group			
	1	2	3	4
Percent of Claimants Receiving:				
Job referral	45.8%	43.4%	36.8%	21.8%
Long term job referral	44.3	41.4	34.7	20.9
Job development attempt	62.1	64.2	33.4	8.9
Job search workshop	42.9	0.0	0.0	0.0
Counseling	2.2	2.7	2.8	1.8
Testing	10.0	10.0	9.4	6.7
Referral to supportive service	3.9	5.4	4.4	2.8
Training	0.3	0.9	0.4	0.5
TJTC determination	1.9	1.2	1.5	0.8
Local office contact	80.1	73.8	69.6	21.8
Percent of Claimants Receiving Any Service ^a	88.4	87.9	85.4	35.3
Sample Sizes	1,277	1,666	1,493	1,485

^aThis variable excludes purely administrative transactions that did not involve claimant contact with the Job Service office.

placement service. Finally, other services unrelated to the demonstration were used relatively equally across the treatment groups, as one would expect.

Another difference can be observed among the groups: although control-group claimants were less likely than those in the other groups to receive any service, control-group claimants who did report to the Job Service were more likely to receive a job referral than were those who reported in the other groups. That is, in the control group, about 60 percent of the claimants who received a service received a job referral, in contrast to 50 percent in groups 1 and 2 and 40 percent in group 3. This finding is not particularly surprising, since control-group claimants who reported to the Job Service were presumably those individuals who expected to benefit from the visit.

Further information on Job Service use is provided in Table III.4, in which we present regressions on the probability of receiving a job referral or a job-development contact, the two major services provided by enhanced placement. The impact of required registration and enhanced placement stands out dramatically, with claimants in treatment groups 1 and 2 having over a 50 percentage point greater probability of receiving at least one of these services than control group claimants. Again, claimants in treatment group 3 were also more likely to receive these services than those in the control group, but were less likely than the treatment group 1 and 2 claimants.¹ Several other variables are also of interest. In

¹ This pattern of treatment impacts also held when separate regressions were estimated for job referrals and job-development contacts, with the enhanced placement treatments having a larger impact on job-development attempts (a 50 to 53 percentage point increase) than on job referrals, (a 20 to 25 percentage point increase).

TABLE III.4

DETERMINANTS OF RECEIVING A JOB REFERRAL
OR JOB DEVELOPMENT ATTEMPT^a

Explanatory Variables	Men	Women	Total
Treatment Group 1	0.518* (23.437)	0.595* (23.363)	0.549* (32.726)
Treatment Group 2	0.504* (24.862)	0.625* (25.381)	0.550* (35.001)
Treatment Group 3	0.346* (16.260)	0.261* (10.584)	0.310* (19.200)
Age	0.001† (1.699)	0.000 (0.186)	.001 (1.436)
Race	0.047* (3.007)	-0.010 (-0.539)	0.025* (2.081)
Sex	—	—	-0.030* (-2.484)
Education	0.009* (3.305)	0.009* (2.224)	0.009* (4.017)
UI Potential Duration	-0.009* (-4.911)	-0.003 (-0.960)	-0.008* (-5.010)
Wage Replacement Ratio	-0.070* (-2.019)	-0.014 (-0.222)	-0.062* (-2.074)
Cohort	0.001 (0.928)	-0.001 (-1.375)	-0.000 (-0.066)
Intercept	0.349* (4.573)	0.219 (1.626)	0.345* (5.345)
R ²	0.19	0.27	0.22
F-Statistic	92.01	95.801	161.91
(Degrees of Freedom)	(9,3388)	(9,2267)	(10,5664)

NOTE: The t-statistics are reported in parentheses.

^a The dependent variable equalled one if the claimant received a job referral or job development attempt, and it equalled zero otherwise.

*Coefficient statistically significant at the 95 percent confidence level for a two-tailed test.

†Coefficient statistically significant at the 90 percent confidence level for a two-tailed test.

particular, cohort was insignificant, indicating that the probability of receiving these ES services remained the same throughout the demonstration. Men and women again differed, with women slightly less likely to receive a service than men; this finding is somewhat surprising, given our previous analysis which indicated that women were more likely to respond in a timely manner to the initial call-in.¹ Other results are that education was positively correlated with receipt of services, black males were more likely to have received services than white males, and the UI variables had an impact on the receipt of services by males but not by females. Longer potential duration of UI benefits was correlated with a lower probability of receiving a service, perhaps because individuals who were eligible for a relatively long duration had relatively high pre-UI wages, and they may not have expected to have benefited from the Job Service.

A final issue on the receipt of ES services pertains to the possible impact of the treatments on non-UI claimants who use the Job Service. Since the enhanced placement and strengthened work-test treatments were expected to increase the level of service use by a typical UI claimant, some concern was expressed that these treatments would adversely affect the services provided to other users of the Job Service. While the demonstration design did not address this question directly, the question of adverse impacts can be examined by comparing the level of services provided in Charleston during the demonstration with the level of services provided prior to the demonstration. Although factors external to the demonstration which may affect the level of services probably changed

¹ This result also held when separate regressions were run for job referrals and job-development attempts.

between the two periods, the comparison should indicate whether a major decline in services for nonclaimants occurred. Data on this question are reported in Table III.5. These data show no indication that services to other users were diminished by the increased level of services to UI claimants. In fact, the number of claimants who applied for services during the demonstration period declined by one-third relative to the pre-demonstration period, while the number of other applicants increased. This finding was probably attributable in part to the fact that, under the demonstration, the control group was not required to report to the Job Service and to the delayed registration provisions of the treatments. That is, claimants were told not to report to the Job Service until they had received a first payment, whereas all pre-demonstration claimants were told to report at the time of their initial claim, although not all did so. In addition, the data show that the level of job-development contacts, job referrals, and job placements increased for claimants as expected, but that the levels of these services also increased for the nonclaimants. Thus, there is no evidence that the demonstration treatments adversely affected non-UI users of the Job Service.

B. NONMONETARY DETERMINATIONS, DENIALS, AND APPEALS

A key element of each of the three demonstration treatments was the strengthened UI work test, which differed from the work test applied to the control group in several ways. Most importantly, as described in Chapter II, all claimants in the demonstration treatment were notified to report to the Job Service for placement services at the time the UI first payment was mailed; the control group did not receive such a notice. Failure to report

TABLE III.5

CHARLESTON EMPLOYMENT SERVICE ACTIVITIES:
COMPARISON OF PRE-DEMONSTRATION TO DEMONSTRATION PERIOD

	(1)	(2)	(3)	(4)
	Pre-Demonstration Period (March-December 1982)	Demonstration Period (March-December 1983)	Percentage Change (1) to (2)	Adjusted Percentage Change (1) to (2) ^a
New Applicants	19,946	18,407	-7.7	n.a.
Claimants	6,822	4,559	-33.2	n.a.
Other	13,124	13,848	5.5	n.a.
Job Development Contacts	6,609	8,945	35.4	38.4
Claimants	2,305	4,136	79.4	118.8
Other	4,304	4,809	11.7	11.1
Job Referrals	29,390	36,766	25.1	27.2
Claimants	7,521	9,304	23.7	35.5
Other	21,869	27,462	25.6	24.3
Placements	14,832	16,837	13.5	14.6
Claimants	2,322	2,553	10.0	15.0
Other	12,510	14,284	14.2	13.5

^aThe percentage change was adjusted to account for the change in the caseload, using the change in the number of new applicants to make the adjustment.

led to a call-in to UI for fact-finding purposes,¹ and a stop was placed in the payment file so that no further UI payments could be made until the work-test issue was resolved through a formal nonmonetary determination.

For treatment groups 1 and 2, a similar process occurred nine weeks later, when all active claimants were notified a second time to report to the Job Service. Failure to respond to this notice within two weeks generated a stop payment and a call-in to UI for fact-finding. For treatment group 1, failure to call-in for the job-search workshop (4 weeks after the first call-in) also generated a stop payment and call-in to UI for fact-finding.

A final element of the strengthened work test was that Job Service staff received training on the UI regulations, concepts, and definitions pertaining to the work test. The form used to send information on potential work-test issues from the Job Service to UI was also modified by creating three versions that were designed to report more detailed and explicit information. The first form (263-A) reported information on failure to accept a referral, failure to report for a job interview, and failure to accept an offer of suitable work; the second (263-B) reported information on unreasonable job requirements or availability; and the third (263-C) reported job placements or obtained employment. In addition, UI was expected to return annotated forms to the Job Service when it was judged that a work test issue did not exist. These procedures were

¹ As stated previously, receipt of a service within 30 days after the date the computer check was performed satisfied the reporting requirement. Thus, individuals who had voluntarily reported to the Job Service prior to receiving the formal notice were not required to report a second time.

designed to improve the exchange of information between the local Job Service and UI offices.

The strengthened work test was expected to enhance identifying potential work-test violations (i.e., the number of nonmonetary determinations for nonseparation issues was expected to increase). Consequently, the number of denials for work-test issues was also expected to increase, and, correspondingly, the number of appeals was expected to rise. Since treatment group 1 was required to report to the Job Service up to three times, treatment group 2 up to two times, and treatment group 3 once, it was also expected that the increase in nonmonetary determinations, denials, and appeals would be greatest for treatment group 1 and smallest for treatment group 3. Finally, there were no particular expectations about how the strengthened work test would affect the probability that a determination would lead to a denial and the probability that a denial would lead to an appeal, but changes in these probabilities are clearly of interest because of their impact on the cost-effectiveness of the treatments.

We now turn to an examination of these issues. The discussion is organized into two sections: (1) overall results by treatment group and (2) the characteristics of claimants who were denied benefits.

1. Nonmonetary Determinations by Treatment Group

Overall nonmonetary outcomes are reported in Table III.6 by treatment group. The data, as expected, show that the claimants in the three treatment groups had higher rates of nonseparation issue nonmonetary determinations than those in the control group, and the differences were statistically significant. Moreover, the rate of nonmonetary determina-

TABLE III.6

NONMONETARY DETERMINATIONS, DENIALS, AND APPEALS
BY TREATMENT GROUP^a

Outcome	Treatment Group			
	1	2	3	4
Percent of Claimants with a Nonmonetary Determination	18.4	16.7	13.4	5.8
Mean Number of Nonmonetary Determinations ^b	0.22	0.20	0.16	0.08
Percent of Claimants with a Nondemonstration-Generated Nonmonetary Determination	5.3	5.6	5.4	5.8
Percent of Claimants with a Denial	8.7	9.2	7.4	4.2
Percent of Claimants with a Demonstration-Generated Denial	6.0	5.8	3.6	0.0
Percent of Determinations That Led to a Denial	47.1	54.9	55.3	71.7
Percent of Claimants with an Appeal	0.6	0.5	0.1	0.3
Percent of Denials That Were Appealed	7.3	5.2	1.8	8.2
Percent of Claimants with Successful Appeals ^c	0.2	0.1	0.1	0.0
Percent of Appeals That Were Successful ^c	25.4	12.5	53.9	0.0
Sample Sizes	1,277	1,666	1,493	1,485

^aAll determinations, denials, and appeals for nonseparation issues are included except the small proportion decided in the state UI office.

^bThese numbers take into account multiple nonmonetary determinations.

^cModified decisions are counted as successful.

tions was significantly higher in treatment groups 1 and 2 than in treatment group 3, presumably reflecting the impact of the increased reporting requirements for those two groups. A comparison of the mean number of determinations with the percentage who had a determination shows that some claimants had more than one determination. Data presented in the table indicate that about 3 percent of those in groups 1 and 2 had two or three determinations, and that 2 percent in group 3 and 1.5 percent in the control group had two or three determinations. Data in the table also show that the increased rate of determinations for the strengthened work-test groups did not reduce the number of potential issues identified through regular processes, since the rate of non-demonstration-generated determinations was similar across all treatment groups.

The higher rates of nonmonetary determinations for the treatment groups were reflected in higher denial rates in those groups relative to the control group. However, whereas determinations occurred two to three times more frequently in the treatment group, denials occurred only about two times more frequently than in the control group, since about half the treatment-group determinations led to a denial, as compared with over 70 percent of the control-group determinations. Thus, the additional issues identified by the strengthened work test were less likely to lead to a denial than those identified through regular processes. However, the overall proportion of denials was probably still high enough to warrant the additional administrative costs associated with these determinations. In fact, the data in the table show that about 6 percent of the claimants in groups 1 and 2 had a denial that was generated by the demonstration, generally by the reporting requirement. This percentage was lower in group

3, primarily because only one call-in to the Job Service was required for that group.

The final set of data in Table III.6 present information on appeals. These data indicate that considerably less than 1 percent of the claimants in any treatment group were involved in an appeal, both because relatively few claimants had their benefits denied and because relatively few of these filed an appeal. Moreover, there was no indication that the denials for treatment group cases were more likely to lead to an appeal than those for the control group. Although treatment-group appeals appear to have been more successful than those for the control group, the differences were not statistically significant.

Table III.7 presents more information on the nature of the determinations, for the first nonmonetary determination received by claimants during the demonstration. The data indicate that the first call-in to the Job Service accounted for over half of the determinations for the groups who received the strengthened work test. For those in groups 1 and 2, the additional call-ins for the job-search workshop and for the second placement interview also generated some determinations.

Interestingly, the attempt to improve the flow of information between the Job Service and UI through training and the revised forms (see above) generated very few determinations—less than one-half of a percent in groups 1 and 2, and none in group 3. Data not presented in the table on the overall level of these special communications indicate that a total of 126 forms (263A and 263B) were sent to UI to indicate possible work test violations. Of these, about half led to a call-in to UI, and no action was taken on the remaining ones, generally because the claimant was no longer

TABLE III.7

SELECTED CHARACTERISTICS OF THE INITIAL NONMONETARY
DETERMINATION BY TREATMENT GROUP

Characteristic	Treatment Group			
	1	2	3	4
Source of Determination				
First Job Service call-in	50.6%	59.0%	64.3%	0.0%
JSW call-in	16.2	0.0	0.0	0.0
Second Job Service call-in	11.1	12.2	0.0	0.0
Job Service-UI communication	0.4	0.4	0.0	0.0
Other	21.7	28.4	35.7	100.0
Total	100.0	100.0	100.0	100.0
Date of Determination Relative to the Claim's Effective Date				
0-2 weeks	46.7%	40.4%	43.3%	47.6%
3-4 weeks	35.4	42.3	38.7	31.4
5-7 weeks	10.0	12.5	11.9	7.0
8 or more weeks	7.9	4.9	6.2	14.0
Total	100.0	100.0	100.0	100.0
Outcome				
Eligible	49.8%	43.9%	43.5%	19.8%
Indefinite denial	17.9	23.0	27.5	61.6
Denial for specific weeks	32.3	33.1	29.0	18.6
Total	100.0	100.0	100.0	100.0
Denial Rate				
Demonstration-generated determinations	39.7	46.2	41.1	n.a.
Non-demonstration-generated determinations	88.2	81.0	84.5	80.2
Total	50.2	56.1	56.5	80.2
Reason Given for Decision				
Eligible:				
Problem with mail	22.0%	24.4%	14.8%	0.0%
Did not receive call-in	39.8	34.1	37.5	0.0
Engaged in job search	10.2	4.1	6.8	0.0
Other	28.0	37.4	40.9	100.0
Total	100.0	100.0	100.0	100.0
Ineligible:				
Illness	12.0%	10.3%	15.2%	5.8%
No transportation	5.1	10.3	11.6	1.4
Did not report as directed ^a	37.6	25.2	12.5	2.9
Failed to make active job search	23.1	23.2	28.6	2.9
Out of town	4.3	7.1	9.8	4.3
Other	17.9	21.9	22.3	14.6
Total	100.0	100.0	100.0	100.0
Sample Sizes	235	278	200	86

n.a. = not applicable

^aIncludes cases that did not report in a timely fashion.

active. Among the call-ins, about 40 percent were dropped because no issue was apparent or the claimant had become inactive. Even by the end of the demonstration, another 36 percent were still pending, and only 16 percent of the call-ins had led to a denial. Thus, even if all the pending cases led to denials, the proportion of these communications that led to a denial would be 28 percent (9 percent if no pending cases led to a denial). A third special form, the 263C, reported placements and obtained employment, and it was expected that it might help uncover cases in which individuals who had become employed would continue to claim UI benefits. This form was used more frequently than the other two special forms (166 were sent to UI), but in the vast majority of cases the claimants were inactive, as expected. Only 4 percent led to a call-in, and all of these were still pending a decision as of the end of the demonstration.

In conclusion, the attempts to improve communication between the Job Service and UI, through training and transmitting relatively detailed information on potential work test issues, probably led to some additional benefit denials, but the denial rate was relatively low. In addition, transmitting information on placements and obtained employment uncovered very few potential fraud cases, and the cost to examine these cases probably outweighed any benefit.

The data in Table III.7 also report information on determination timing, outcomes, and reasons for the outcomes. With respect to timing, most of the initial determinations occurred early in an individual's UI claim, with over 40 percent within the first two weeks of the effective date and another 30 to 40 percent within the next two weeks. Such timing indicates that the determinations were made promptly once an issue was

identified. Next, as stated above, the proportion who were determined to be eligible among the strengthened work-test groups was higher than for the control group, and for those who were denied benefits a greater proportion were for specific weeks as opposed to an indefinite disqualification. This was the case because many of the experimental group denials were for failing to report to the Job Service; however, once the claimant reported, a nonmonetary issue was no longer applicable.

With respect to the reasons given for the determination, over half of the strengthened work-test treatment decisions that led to the claimant being declared eligible were made because the claimants said that they did not receive the call-in or they had been having mail problems and the notice arrived late. Since many of these claimants apparently received their benefit checks under separate cover, these statements about problems with receiving the notice might have been challenged, but a decision was made by the state to accept mail-related reasons at face value and to declare the claimant eligible. More vigorous examination of these statements or the inclusion of the call-in notice with the benefit check could probably raise the denial rate in future applications of these procedures.¹

The major reasons for the denial of benefits were failure to make an active job search and, for the strengthened work-test group, failure to report as directed. Not surprisingly, this latter reason was more prevalent when more reporting was required. Other claimants who said they

¹ The check and the call-in notice were mailed separately for operational reasons pertaining to the fact that only a portion of the claimants received notices during the demonstration.

were ill or had no transportation were declared ineligible for benefits because they did not fulfill the able and available requirements.

2. Characteristics of Claimants Who Were Denied UI Benefits

In this section, we examine the characteristics of claimants whose benefits were denied, based on the regressions on the denial rate reported in Table III.8. The results indicate that the denial rate was higher (as we saw above) for claimants in the three strengthened work-test treatment groups than for claimants in the control group. The difference was about 5 percentage points for groups 1 and 2, and 3 percentage points for group 3.

Cohort was another demonstration-related variable that was significant in all the regressions, which showed that the probability of a denial dropped during the demonstration. Several possible reasons explain why this drop occurred, but unfortunately we cannot distinguish among them. First, as shown above, the probability of responding to the initial call-in increased somewhat during the demonstration, and this could be expected to reduce denials. Second, unemployment rates fell steadily throughout the demonstration, presumably leading to a reduced duration of unemployment spells (see discussion below), and thus to a reduced probability of a denial. And, third, some nonmonetary determinations may not have been completed as of the end of our observation period, and further denials may have occurred subsequently.

The regressions also show that women and blacks were more likely to have a denial than, respectively, men and whites, a finding which may pertain to the fact that these groups had longer unemployment durations

TABLE III.8
DETERMINANTS OF THE DENIAL RATE

Explanatory Variables	Men	Women	Total
Treatment Group 1	0.049* (3.815)	0.061* (3.470)	0.054* (5.199)
Treatment Group 2	0.051* (4.313)	0.050* (2.957)	0.050* (5.141)
Treatment Group 3	0.028* (2.238)	0.033† (1.940)	0.030* (3.012)
Age	-0.000 (-0.899)	-0.001* (-2.322)	-0.001* (-2.265)
Race	0.037* (4.037)	0.044* (3.536)	0.040* (5.492)
Sex	— —	— —	0.021* (2.820)
Education	-0.003† (-1.861)	-0.006* (-2.382)	-0.004* (-2.864)
UI Potential Duration	-0.000 (-0.079)	0.006* (2.367)	0.001 (1.395)
Wage Replacement Ratio	0.053* (2.625)	0.081† (1.831)	0.050* (2.670)
Cohort	-0.001* (-4.291)	-0.002* (-4.039)	-0.002* (-5.788)
Intercept	0.076† (1.715)	0.025 (0.026)	0.070† (1.737)
² R	0.021	0.022	0.023
F-statistic	9.111	6.606	14.335
(Degrees of Freedom)	(9,3388)	(9,2267)	(10,5664)

NOTE: The t-statistics are reported in parentheses.

^a The dependent variable equalled one if the claimant's benefits were ever denied for a nonseparation issue, and it equalled zero otherwise.

*Coefficient statistically significant at the 95 percent confidence level for a two-tailed test.

†Coefficient statistically significant at the 90 percent confidence level for a two-tailed test.

and, hence, were at risk of a denial for longer periods.¹ Age and education were negatively correlated with the denial rate, and more generous potential UI benefits were correlated with higher denial rates.

C. EMPLOYMENT IMPACTS

Two types of information on employment were collected in the demonstration: ESARS data on placements and UI wage record data. Because the wage record data were available only with a lag, sample sizes were smaller for this data source than for the relatively complete ESARS data.² However, the ESARS data provide information only on employment relative to ES-provided services, whereas the wage record data are comprehensive, missing only the relatively few jobs uncovered by UI. For this reason, most of our analysis of employment impacts was based on the wage record data. The ESARS placement data were used primarily to indicate the types of experiences that claimants had at the Job Service.

Table III.9 summarizes the basic data on employment impacts by treatment group. Overall, depending on the particular treatment category, between 8 and 13 percent of the claimants in our sample were reported to have received a placement through an ES job referral. About three-fourths

¹ In the reporting and ES use regressions, we found that the relationships for men and women differed enough that separate regressions were appropriate. For the denial rate, this was not the case, and the differences between men and women were captured by the sex dummy variable.

² Specifically, ESARS data were available for cohorts 3-45, for the entire sample of 5,921. Wage record data for the first quarter after UI benefits started were available only for cohorts 3-32, for a total sample of 4,094. Only individuals in cohorts 3-19 had wage record data for two quarters after their UI benefits started, and for this group the sample size totaled 2,345.

TABLE III.9

EMPLOYMENT-RELATED OUTCOMES OF THE DEMONSTRATION

Outcome	Treatment Group			
	1	2	3	4
Placement Results				
Percent with Long-Term Placement	9.79*	11.22*	7.50	6.46
Percent with Medium-Term Placement	1.49	1.08	1.41	0.94
Percent with Short-Term Placement	0.78	1.14	0.74	0.81
Percent "Obtained Employment"	4.54	—	—	—
Number in Sample	1,277	1,666	1,493	1,485
Wage Record Results				
Percent with Wages First Quarter After BYB	53.48	53.94	55.04	52.04
Mean First Quarter Wage ^a	\$1,912	\$2,077*	\$1,951	\$1,925
Ratio of Mean First Quarter Wage to Quarterly Base Period Wage ^a	0.73	0.81	0.77	0.74
Number in Sample	905	1,166	1,041	982
Percent with Second Quarter Wages	65.81*	62.27	62.52	61.64
Mean Second Quarter Wage ^a	\$2,279	\$2,432*	\$2,250	\$2,221
Ratio of Mean Second Quarter Wage to Quarterly Base Period Wage ^a	0.92	0.94	0.90	0.88
Number in Sample	506	660	595	584

^a Computed only for those with positive wages in quarter.

* Treatment group mean significantly different from control group (group 4) mean at .05 level of significance on a one-tailed test. No significance tests were calculated for the ratio figures.

of those received long-term placements¹--that is, for jobs expected to last more than 150 days. As was expected, given the enhanced services available, such long-term placements were significantly more likely to occur in treatment groups 1 and 2 than in groups 3 and 4. Still, even claimants in the control group (group 4)--who used the job service on a voluntary basis--obtained a number of placements. The finding that additional long-term placements were obtained by individuals in treatment groups 1 and 2 continued to hold in regression analyses that controlled for differences in the characteristics of claimants. These results (reported in Table III.10) indicate that, other things equal, long-term placements were 3 to 4 percent more likely to occur in the enhanced placement groups. Practically all of this difference was due to increased placements for males. For females, most of the differences among the treatment categories were not statistically significant. It is possible that this difference by sex arose because of the nature of the employment opportunities available through the enhanced placement service, but we did not have adequate information to verify that possibility.

The ESARS data provided one additional measure of ES-related job finding--statistics on individuals who "obtained employment" following participation in specific services, notably the job-search assistance component of treatment group 1. Since these data were collected through a mail survey in which nonresponse was quite high, they undoubtedly

¹ It should be emphasized that the data in Table III.9 refer to the numbers of claimants who obtained placements, not to the number of placements per se. Specifically, the large number of short-term placements that occur in the Charleston offices (primarily in connection with their substitute teacher program) are not apparent in Table III.9 because these placements were obtained by relatively few individuals.

TABLE III.10

PROBABILITY OF RECEIVING A LONG-TERM PLACEMENT^a

Explanatory Variable	Men	Women	Total
Treatment Group 1	0.041* (2.601)	0.018† (1.289)	0.031* (2.852)
Treatment Group 2	0.066* (4.593)	0.005 (0.376)	0.043* (4.184)
Treatment Group 3	0.022† (1.443)	-0.015 (-1.129)	0.007 (0.638)
Age	-0.001† (-1.856)	-0.001 (-1.500)	-0.001* (-2.360)
Race	0.012 (1.114)	-0.008 (-0.819)	0.004 (0.516)
Sex	— —	— —	-0.040* (-5.123)
Education	0.002 (0.904)	0.001 (0.565)	0.001 (0.995)
Potential Duration	-0.006* (-4.224)	-0.007* (-3.679)	-0.006* (-5.696)
Wage Replacement Ratio	-0.090* (-3.671)	-0.097* (-2.804)	-0.089* (-4.608)
Cohort	-0.000 (-0.205)	-0.001* (-2.461)	-0.001 (-1.411)
Intercept	0.263* (4.866)	0.315* (4.311)	0.294* (7.040)
R ²	0.015	0.014	0.021
F-statistic	5.799	3.648	12.099
(Degrees of Freedom)	(9,3388)	(9,2267)	(10,5664)

^a

The dependent variable equalled one if the claimant received a long-term job placement and zero otherwise.

* Coefficient statistically significant at the 95 percent confidence level for a two-tailed test. A one-tailed test was used for the treatment groups.

† Coefficient statistically significant at the 90 percent confidence level for a two-tailed test. A one-tailed test was used for the treatment groups.

understated the number of claimants who found employment following the completion of the job-search assistance component. Nevertheless, the data indicated that about 4.5 percent of the claimants in treatment group 1 were credited with obtaining employment following (and perhaps as a result of) participation in the job-search workshop.

With respect to the wage record data in Table III.9, about 54 percent of all claimants had obtained some covered employment in the first quarter following the quarter in which they began receiving UI. Although recorded employment in that quarter was higher in all of the demonstration treatment groups than in the control group, none of these differences was statistically significant. Among those individuals with employment, first quarter wages were higher for treatment group 2 than for the other groups, and its wages were also high relative to mean base period wages. The absolute sizes of differences among the groups were not large, however, and no consistent pattern was apparent among the other three groups.

The wage record results for the second quarter after the UI benefit start date were similar to those for the first quarter. Overall, approximately 63 percent of the claimants in the sample had some employment in the second quarter. Employment in treatment group 1 was significantly greater than in the control group. And wages in treatment group 2 were significantly greater than those of the other groups. It is possible that some of these effects derive from the increased level of long-term placements received by groups 1 and 2. However, since most of these differences were relatively small in absolute terms, it was difficult to investigate that connection in detail.

Thus, our general conclusion from examining the raw data in Table III.9 was that, although a few of the differences between the groups were statistically significant, there was no evidence of very strong treatment effects on employment-related outcomes. To investigate that issue further, we ran a series of ordinary least squares regressions on the various employment impacts described in Table III.9. All of these regressions included the basic set of control variables used previously. Because wages are approximately log-normally distributed, the natural logarithm of quarterly wages was used as a dependent variable in these regressions rather than the wage itself. In addition, the logarithm of the average quarterly base period wages was added to the quarterly wage regressions in a further effort to control for the determinants of claimants' post-UI wages. All regressions were run for the entire sample and for men and women separately.

These basic regression results for employment outcomes are reported in Tables III.11 through III.14. In the 12 equations reported, only two of the treatment group dummy variables had coefficients that were significantly different from zero at the 95 percent level--a result that might have occurred by chance. Hence, the regression results tended to support the findings from the unadjusted data--that treatment effects on employment-related outcomes tended to be relatively small and to exhibit no consistent pattern among the groups.

Estimated coefficients for several of the other independent variables in Tables III.11 through III.14 were statistically different from zero, and these warrant a brief discussion. Other things equal, women were less likely to be reemployed than men in both the first and second

TABLE III.11

PROBABILITY OF EMPLOYMENT IN FIRST QUARTER
AFTER START OF UI BENEFITS^a

Explanatory Variable	Men	Women	Total
Treatment Group 1	-0.008 (-0.287)	0.039 (1.120)	0.010 (0.429)
Treatment Group 2	0.017 (0.617)	0.001 (0.033)	0.013 (0.585)
Treatment Group 3	0.002 (0.063)	0.063* (1.856)	0.026 (1.163)
Age	-0.004* (-4.101)	0.000 (0.034)	-0.002* (-3.059)
Race	-0.031 (-1.484)	0.048* (1.954)	0.002 (0.140)
Sex	— —	— —	-0.095* (-5.803)
Education	-0.007† (-1.793)	0.013* (2.585)	0.000 (0.069)
Potential Duration	-0.010* (-3.496)	-0.016* (-2.767)	-0.013* (-5.072)
Wage Replacement Ratio	-0.311* (-6.282)	-0.142 (-1.329)	-0.257* (-5.746)
Cohort	-0.001 (-0.441)	-0.001 (-0.843)	-0.001 (-0.889)
Intercept	1.236* (11.216)	0.747* (3.341)	1.113* (11.507)
R ²	0.024	0.017	0.027
F-statistic	6.414	3.193	11.434
(Degrees of Freedom)	(9,2403)	(9,1664)	(10,4076)

^a The dependent variable equalled one for those with a positive wage for the quarter and zero otherwise.

* Coefficient statistically significant at the 95 percent confidence level for a two-tailed test. A one-tailed test was used for the treatment groups.

† Coefficient statistically significant at the 90 percent confidence level for a two-tailed test. A one-tailed test was used for the treatment groups.

TABLE III.12

QUARTERLY WAGES FOR THE FIRST QUARTER
AFTER START OF UI BENEFITS^a

Explanatory Variable	Men	Women	Total
Treatment Group 1	0.157* (1.799)	-0.175 (-1.500)	0.032 (0.453)
Treatment Group 2	0.114 (1.425)	-0.097 (-0.847)	0.034 (0.529)
Treatment Group 3	0.114 (1.351)	-0.036 (-0.324)	0.055 (0.812)
Age	-0.002 (-0.852)	-0.001 (-0.226)	-0.001 (-0.562)
Race	-0.349 (-5.558)	-0.080 (-0.975)	-0.247* (-4.950)
Sex	—	—	-0.280* (-5.081)
Education	-0.018 (-1.660)	0.024 (1.419)	-0.004 (-0.523)
Potential Duration	0.035* (4.317)	0.048* (2.806)	0.036* (5.078)
Wage Replacement Ratio	1.000* (3.185)	1.487* (3.563)	1.076* (4.747)
Log Base Period Wage	0.718* (6.043)	0.717* (6.634)	0.710* (9.006)
Cohort	-0.002 (-0.615)	0.011 (2.213)	0.002 (0.802)
Intercept	0.672 (0.577)	-0.981 (-0.792)	0.404 (0.506)
R ²	0.119	0.096	0.144
F-statistic	19.086	8.077	33.424
(Degrees of Freedom)	(10,1409)	(10,762)	(11,2181)

^a The dependent variable was the log of the quarterly wage. Regressions were run only over those with positive wages.

* Coefficient statistically significant at the 95 percent confidence level for a two-tailed test. A one-tailed test was used for the treatment groups.

† Coefficient statistically significant at the 90 percent confidence level for a two-tailed test. A one-tailed test was used for the treatment groups.

TABLE III.13
 PROBABILITY OF EMPLOYMENT IN SECOND QUARTER
 AFTER START OF UI BENEFITS^a

Explanatory Variable	Men	Women	Total
Treatment Group 1	0.018 (0.484)	0.075† (1.536)	0.038 (1.316)
Treatment Group 2	-0.007 (-0.215)	0.0117 (0.252)	0.002 (0.065)
Treatment Group 3	-0.022 (-0.632)	0.059† (1.280)	0.006 (0.228)
Age	-0.006* (-5.057)	0.001 (0.862)	-0.003* (-3.202)
Race	0.035 (1.371)	0.018 (0.546)	0.028 (1.373)
Sex	—	—	-0.076* (-3.598)
Education	-0.004 (-0.947)	0.006 (0.855)	-0.002 (-0.448)
Potential Duration	-0.001 (-0.182)	-0.010 (-1.332)	-0.003 (-1.087)
Wage Replacement Ratio	-0.218* (-3.587)	-0.196 (-1.378)	-0.185* (-3.299)
Cohort	-0.001 (-0.467)	0.008* (2.267)	0.002 (1.180)
Intercept	1.065* (7.891)	0.695* (2.389)	0.932* (7.709)
<hr/>			
R ²	0.026	0.013	0.020
F-statistic	4.367	1.282	4.743
(Degrees of Freedom)	(9,1455)	(9,867)	(10,2341)

^a The dependent variable equalled one if the claimant had a positive wage for the quarter and zero otherwise.

* Coefficient statistically significant at the 95 percent confidence level for a two-tailed test. A one-tailed test was used for the treatment groups.

† Coefficient statistically significant at the 90 percent confidence level for a two-tailed test. A one-tailed test was used for the treatment groups.

TABLE III.14
 QUARTERLY WAGES FOR SECOND QUARTER
 AFTER START OF UI BENEFITS^a

Explanatory Variable	Men	Women	Total
Treatment Group 1	0.071 (0.678)	0.089 (0.643)	0.068 (0.814)
Treatment Group 2	0.132† (1.356)	0.131 (0.956)	0.125† (1.538)
Treatment Group 3	0.080 (0.786)	-0.930 (-0.693)	0.009 (0.112)
Age	-0.002 (-0.584)	0.007 (1.441)	0.003 (0.925)
Race	-0.340* (-4.568)	0.044 (0.436)	-0.201* (-3.350)
Sex	— —	— —	-0.197* (-2.932)
Education	-0.009 (-0.702)	0.048* (2.202)	0.007 (0.618)
Potential Duration	0.033* (3.319)	0.072* (3.299)	0.035* (3.891)
Wage Replacement Ratio	1.146* (2.977)	1.997* (3.695)	1.088* (3.772)
Log of Base Period Wage	0.753* (5.052)	0.582* (4.109)	0.629* (6.110)
Cohort	-0.001 (-0.184)	0.036* (3.636)	0.014* (2.396)
Intercept	0.408 (0.282)	-1.513 (-0.936)	0.806 (0.779)
<hr/>			
R ²	0.106	0.094	0.097
F-statistic	11.475	5.049	14.279
(Degrees of Freedom)	(10,964)	(10,488)	(11,1462)

^a

The dependent variable was the log of the second quarter wage. Regressions were run only over those with positive wages.

* Coefficient statistically significant at the 95 percent confidence level for a two-tailed test. A one-tailed test was used for the treatment groups.

† Coefficient statistically significant at the 90 percent confidence level for a two-tailed test. A one-tailed test was used for the treatment groups.

quarters. Women were also likely to have lower wages than men in both of these quarters, even after controlling for their base period wage. Minorities were also likely to have lower quarterly wages than were non-minorities, but, in this case, there was no differential employment impact. Age had a significant negative effect on reemployment for males. But that effect did not appear in the wage data, nor did age seem to be a deterrent to reemployment for women. Finally, the estimated effects of UI benefits receipt might also be mentioned. Following prior research, two variables--the claimants' potential UI duration and the claimants' wage-replacement ratio (here defined as the weekly UI benefit divided by the average weekly base period wage)--were included in the regressions. Standard job-search theory predicts that these variables should have negative effects on reemployment (because more generous UI benefits will prompt claimants to hold out for better jobs) and positive effects on reemployment wages (because those who do accept jobs will obtain better ones). Generally, these predictions seemed to be supported by the data. All of the coefficients for the UI-related variables had the expected signs, and many of these coefficients were statistically significant. However, because examining the incentive effects of unemployment insurance was not a primary research focus of the present study, we did not explore all of these estimates fully.¹

¹ We did, however, investigate several alternative forms of the wage equations to examine the consequences of possible interactions between the wage replacement and base period wage variables. The variations examined were (1) constraining the coefficients of the log of base period wages to be 1.0 (as theory might suggest); (2) omitting the WRR variable; and (3) estimating the equation in linear rather than in log-linear form. Although each of these variants had major effects on some of the coefficients of the independent variables, the coefficients for the treatment variables were changed little.

In conclusion, then, our analysis did not provide any strong indications of the effects of the demonstration treatment on claimants' reemployment success. The demonstration services did generate some additional placements, and those may have had some very modest effects on employment. However, overall, major impacts were not apparent in the data. Of course, it should be recognized that the UI wage records data we were using possess a number of shortcomings for examining such outcomes. They provide no indication of the precise timing of reemployment, nor do they contain any information on hours worked. Other information on the nature of jobs is similarly lacking. Indeed, some employment can be completely missed by these data if it occurs in industries not covered by UI or if for some reason wage records are delayed in processing. However, because of the random assignment used in the demonstration, there appears to be no very good reason why any of these shortcomings should have affected our basic (absence of) findings pertaining to employment. Hence, we focused the remainder of our research on whether the demonstration treatments had any direct effects on UI benefits collected.

D. IMPACTS ON UI BENEFITS

The principal measure of the success or failure of the demonstration treatments is their impact on UI benefits, since an important objective of the demonstration was to reduce UI benefit outlays. In this section, we examine this impact by using the number of weeks of UI benefits collected as the outcome of interest. (In Chapter IV, we examine the impacts of the demonstration on administrative costs, and, in Chapter V, we estimate the cost-effectiveness of the demonstration treatments.)

Our expectations about the number of weeks of UI collected were that each of the experimental treatments would reduce average weeks relative to the control group, and that, among the experimental treatments, group 1 would have the largest impact, group 2 the next largest, and group 3 the lowest. This latter set of expectations were based on the fact that group 3 received the strengthened work test, group 2 the strengthened work test and enhanced placement, and group 1 the strengthened work test, enhanced placement, and the job-search workshop; and each of these treatment components was expected to reduce the weeks of UI collected. We now turn to this analysis, examining first the results by treatment group and then how the results varied by claimant characteristics.

1. Results by Treatment Group

The basic results are reported in Table III.15 by treatment group and for two sets of cohorts. The data on mean weeks of benefits show that the treatments did reduce weeks of UI benefits collected relative to the control group. For our full analysis sample (cohorts 3 to 45), the difference was .7 weeks for treatment group 1 and .5 weeks for the other two treatment groups, and these differences were statistically significant at at least the 90 percent confidence level for a one-tailed test. No significant differences occurred among the three experimental groups. Since our observation period for the later cohorts enrolled in the demonstration was relatively short, we also restricted the sample to cases for which we had a minimum of six months of follow-up data to examine whether these differences held up. If claimants who had denials or gaps in benefit collection early in their claim period came back later to collect benefits, the impact of the treatments might have been diluted. However,

TABLE III.15

WEEKS OF UI COLLECTED BY TREATMENT GROUP AND COHORT

Cohort	Treatment Group			
	1	2	3	4
Cohorts 3-45				
Weeks Collected				
1-2	10.3%	10.5%	11.2%	7.6%
3-4	7.4	7.4	6.5	7.9
5-7	12.9	10.9	11.9	9.8
8-9	7.1	7.8	6.4	7.1
10-12	11.4	11.9	11.1	12.3
13-16	12.5	12.7	15.3	13.9
17-20	10.7	9.5	9.5	10.5
21-25	10.8	12.1	10.5	13.3
26 or more	16.8	17.2	17.7	17.7
Total	100.0	100.0	100.0	100.0
Mean	14.8	15.0	15.0	15.5
Standard Deviation	10.5	10.5	10.5	10.1
Sample Sizes	1,277	1,666	1,493	1,485
Cohorts 3-32				
1-2	9.3%	9.8%	10.8%	6.2%
3-4	7.8	7.0	6.3	7.7
5-7	12.0	10.5	12.1	9.5
8-9	7.3	8.1	6.9	7.3
10-12	11.2	11.9	10.6	14.0
13-16	12.4	12.3	13.2	12.4
17-20	9.7	8.2	8.5	9.0
21-25	9.6	11.6	10.0	11.2
26 or more	20.8	20.7	21.7	22.6
Total	100.0	100.0	100.0	100.0
Mean	15.6	15.7	15.7	16.4
Standard Deviation	11.1	11.0	11.2	10.7
Sample Sizes	973	1,271	1,152	1,090

for this restricted sample (cohorts 3 to 32), we also found significant differences in weeks of UI benefits collected. In fact, the differences were slightly larger--.7 to .8 weeks. Since the results did not change qualitatively, we used the entire sample for our remaining analysis.

The data on the distribution of weeks collected indicates how the demonstration treatments affected claimants. As can be seen, a greater proportion of claimants collected only 1 or 2 weeks in the experimental treatments relative to the control group, and these differences were statistically significant. They presumably were due to the initial call-in. Another difference in the distribution occurred in weeks 5 to 7, when the job-search workshop call-in was required for group 1, and a relatively large percentage of group 1 claimants stopped collecting during this period relative to the control group. The final Job Service call-in occurred in weeks 10 to 12 for groups 1 and 2, respectively, but in this case no clear impact emerged relative to the other groups.

However, these comparisons, with the exception of the week 1 to 2 comparisons, are not really appropriate. For example, the claimants in the control group who would have stopped collecting UI in weeks 1 or 2, if they received the experimental treatments, must show up somewhere in the benefit weeks distribution, and their presence confounds the comparisons of interest. One way to address this problem is to compare claimants who collected, say, at least 4 weeks, and to examine what proportion of this sample then left the rolls in weeks 5 and 6 as a measure of the impact of the job-search workshop. However, this type of comparison is also problematic, since it compares subsets of the treatment groups that are defined by an outcome variable--in this case, weeks of benefits

collected. Since this outcome is affected by the treatment, membership in each treatment subset cannot be considered random, and we cannot infer that any differences arose because of the treatments. Despite this possible problem, we examined this distribution of UI weeks collected using subsets of claimants.

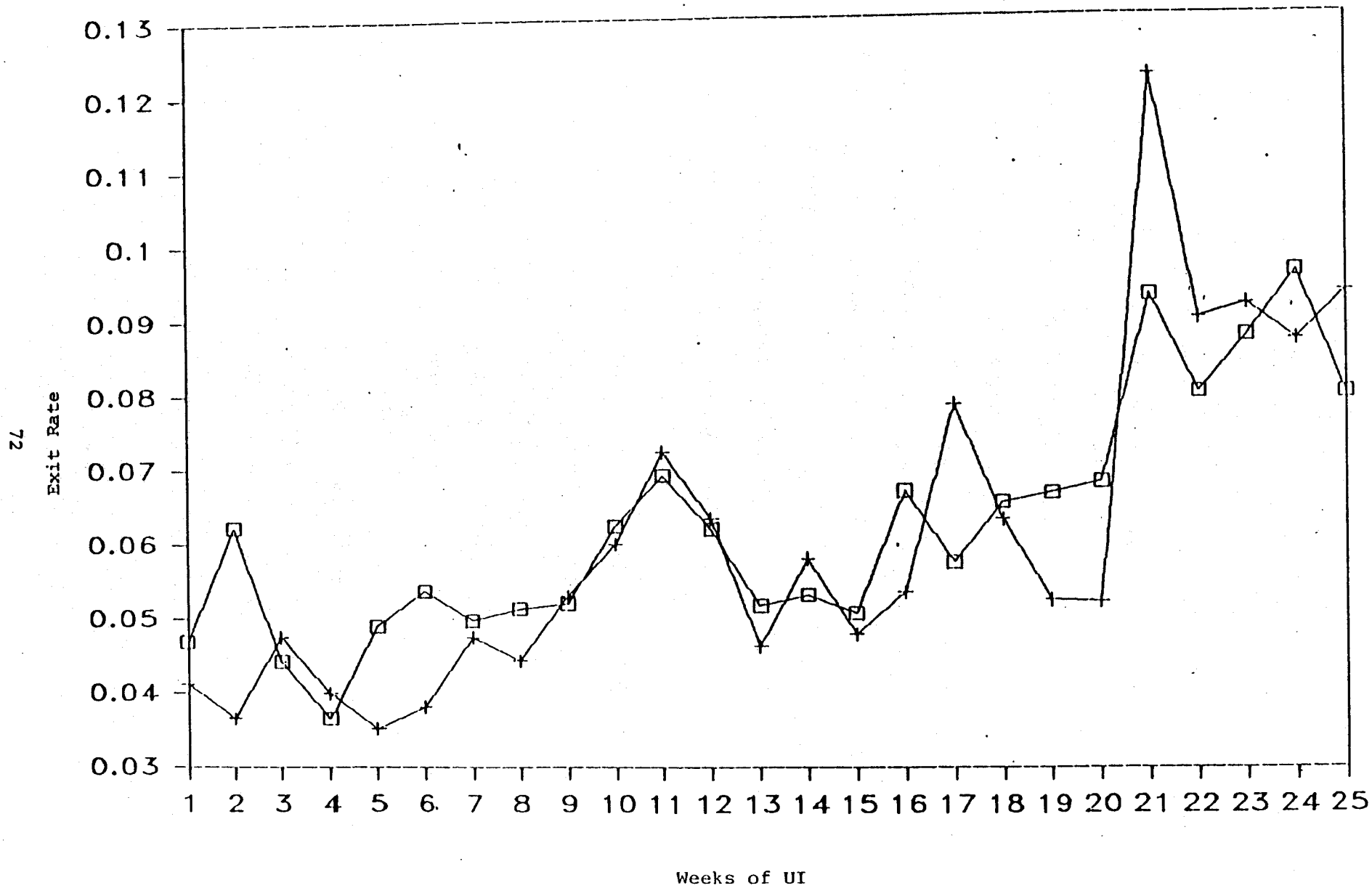
First, we computed the probability of leaving the UI roles for each benefit week. That is, we computed the probability of not collecting "n + 1" weeks of benefits given that "n" weeks were collected. Although some differences occurred among the three experimental groups, we combined them for illustrative purposes, and these exit rates from UI are shown in Figure III.2 for the experimental and control groups. This graph shows, for example, that 4 to 5 percent of the claimants who collected one week of benefits did not collect any more weeks of benefits, and that this exit rate was higher for the treatment group than for the control group claimants. Furthermore, the treatment group exit rates were clearly higher than the control group rates in week 2 as well, and again in weeks 5 to 8. Elsewhere in the distribution the exit rates were closer, although they jumped around considerably towards the end of the distribution, in part because of the smaller sample sizes available for the calculations.¹

A second way to examine the exit rates is to use a regression to control in part for differences in the samples. We did so by using the basic set of explanatory variables described in Chapter II and used previously in this chapter. The coefficients obtained for the treatment

¹ The sample size became progressively smaller because claimants who had stopped collecting were removed from the sample. Claimants who were not eligible for additional weeks were also removed from the computation.

FIGURE III.2

EXIT RATE FROM UI FOR TREATMENT AND CONTROL GROUPS, BY WEEKS OF UI



group variables are reported in Table III.16. Separate regressions were not run for each exit rate; instead, they were grouped as in the distributions reported in Table III.15. As we have discussed previously, the results indicate that the probability of leaving UI in weeks 1 or 2 was about 3 percentage points higher for each of the treatment groups than for the control group, and that the differences were statistically significant. The only other significant result was that group 1 and 3 claimants who collected at least 4 weeks of benefits had a higher probability of collecting 5 to 7 weeks than did control group claimants. This result may reflect the impact of the job-search workshop call-in for group 1, but for group 3, no additional call-in occurred. As appeared to be the case from examining the UI weeks distribution, the second call-in did not have a measurable impact.¹

2. Results by Claimant Characteristic

It is important to examine how these treatment effects varied by type of claimant, since the treatments might be made more cost-effective by applying them to particular groups of the claimant population. However, before proceeding with this examination, we report the results of controlling for claimant characteristics and other factors in our analysis of UI weeks collected (see Table III.17). As we found for the unadjusted results, the analysis indicates that all three treatment groups had a

¹ This result could have occurred in part because, although this call-in occurred 9 weeks after the first call-in, it did not coincide with the same number of weeks of benefits collected for each claimant. Claimants with a break in the collection of benefits could have collected fewer weeks of benefits at the time of the call-in than those with no break in the benefit stream.

TABLE III.16

IMPACT OF TREATMENTS ON PROBABILITY OF LEAVING
UI IN SELECTED WEEKS^a

Weeks	Marginal Impact of Treatment Groups Relative to the Control Group		
	1	2	3
1-2	0.028* (2.389)	0.028* (2.602)	0.033* (2.937)
3-4	-0.003 (-0.250)	-0.004 (-0.366)	-0.011 (-1.003)
5-7	0.047* (3.189)	0.019 (1.390)	0.036* (2.522)
8-9	0.007 (0.486)	0.016 (1.233)	-0.004 (-0.285)
10-12	0.002 (0.134)	0.011 (0.607)	-0.009 (-0.504)
13-16	-0.004 (-0.211)	-0.002 (-0.096)	0.025 (1.361)
17-26 ^b	-0.028 (-1.005)	0.001 (-.054)	-0.036 (-1.332)

^a The coefficients represent the marginal impact of each treatment group on the probability of collecting, for example, 3 or 4 weeks given that at least two weeks were collected. The coefficients were estimated using the basic set of explanatory variables: treatment group, age, race, sex, education, potential duration, wage replacement ratio, and cohort.

^b Potential duration of regular UI was used as the end point if it was less than 26 weeks.

* Coefficient statistically significant at the 95 percent confidence level for a two-tailed test.

† Coefficient statistically significant at the 90 percent confidence level for a two-tailed test.

TABLE III.17

DETERMINANTS OF WEEKS OF UI BENEFITS COLLECTED^a

Explanatory Variable	Men	Women	Total
Treatment Group 1	-1.14* (-2.35)	-0.15 (-0.24)	-0.76* (-1.98)
Treatment Group 2	-1.15* (-2.59)	0.31 (0.51)	-0.61* (-1.69)
Treatment Group 3	-0.83* (-1.79)	-0.20 (-0.33)	-0.55† (-1.50)
Age	0.10* (6.36)	0.10* (4.85)	0.10* (7.88)
Race	2.55* (7.47)	0.86* (1.97)	1.87* (6.95)
Sex	— —	— —	3.59* (12.93)
Education	0.06 (0.97)	-0.12 (-1.27)	-0.01 (-0.13)
Potential Duration	0.35* (8.46)	0.48* (5.73)	0.39* (10.54)
Wage Replacement Ratio	2.53* (3.35)	0.69 (0.44)	1.86* (2.73)
Cohort	-0.11* (-8.50)	-0.16* (-8.70)	-0.13* (-11.95)
Intercept	1.91 (1.14)	7.07* (2.15)	2.61† (1.77)
R ²	0.07	0.08	0.10
F-statistic	29.53	21.35	64.73
(Degrees of Freedom)	(9,3388)	(9,2267)	(10,5664)

^a

The dependent variable was weeks of UI collected.

* Coefficient statistically significant at the 95 percent confidence level for a two-tailed test. A one-tailed test was used for the treatment groups.

† Coefficient statistically significant at the 90 percent confidence level for a two-tailed test. A one-tailed test was used for the treatment groups.

statistically significant impact on weeks of UI collected, reducing weeks by .55 for treatment group 3, .61 for group 2, and .76 for group 1 relative to the control group. The relative magnitude of these differences was as expected, although the differences among treatment groups were not statistically significant.

The results for other explanatory variables were not surprising. As measured by UI benefit weeks, older individuals had longer unemployment durations than younger individuals, as did blacks and females relative to, respectively, whites and males. Cohort was another variable of interest, which was negatively correlated with weeks collected, probably because unemployment rates dropped steadily throughout the demonstration. Thus, it was impossible to distinguish between the effects of the unemployment rate and the duration of the demonstration, and, hence, we used only the cohort variable in our analysis.¹ Finally, potential duration was (perhaps by definition) positively correlated with UI weeks, as was the wage replacement ratio for men. The coefficient of that latter variable was slightly lower than often found in studies of UI duration, but it had the expected positive disincentive effect.

Finally, perhaps the most interesting result shown in the table pertains to the differences in the treatment effect for men and women.² The coefficients for the treatment variable were insignificant for women and highly significant for men, with the impacts of the treatments varying

¹ Separate estimates using the unemployment rate had no impact on the coefficients of the other variables.

² A statistical test of this relationship showed that it differed enough that separate regressions by sex were appropriate.

from 0.8 weeks to 1.15. These differences for men and women mirrored those reported previously. Men were more likely to fail to report to the Job Service than women and, hence, were probably more likely to drop off the UI roles earlier than they would have in the absence of the experimental treatments. Nevertheless, they were also more likely than women to receive a job referral or job-development attempt from the Job Service, and to be placed in a job. Moreover, the treatments had a significant impact on the placement rate for men but not for women. Finally, although the denial rate was higher for women than for men, this difference was not correlated with the treatments. The combination of all these differences led to the differential treatment impacts.

While these male/female differences are interesting, they are not particularly useful to policymakers, since any attempt to target the demonstration treatments toward specific claimant groups would not be based on the sex of the claimants. Therefore, we explored these differential treatment effects further to determine whether underlying differences existed between the male and female samples that helped explain our findings. Given the relatively limited information on claimants available from UI records, we could examine only a few of the large number of possible reasons for the male/female differences, but several areas of analysis proved promising. First, data on the pre-layoff industry of the sample was available for about 90 percent of the regression sample, and we used these data to explore differences in treatment effects by industry. The analysis showed that a large proportion of the male sample came from the construction industry (36 percent, compared with 3 percent for women), and that the treatment effects were larger for men in the construction industry than for

those not in that industry (see Table III.18). The treatments reduced UI benefits by about 2 weeks for males in the construction industry, and by under one week for those in other industries. No significant effects were found for women, which was not surprising given the very small number in the construction industry.¹

While the claimant's industry was clearly an important determinant of treatment effects, we were also interested in other possible explanations for the male/female differences. In particular, we expected that workers who were job-attached (i.e., those who expected to be recalled by their pre-layoff employer) would be less affected by the treatments than those who were not job-attached. Data on recall expectations were available for a subset of the claimants (about 20 percent) from the interview administered as part of the Continuous Wage and Benefit History program. We thus used this CWBH sample to examine the impact of recall expectations on the treatment effects.² This analysis is reported in Table III.19. While the smaller sample size available for this analysis reduced our ability to detect treatment effects, some estimates were significant.³ For the whole sample, we found no treatment effects for

¹ We also examined treatment effects for other industries that included sizeable portions of our sample (manufacturing, services, and government), but treatment impacts did not differ for claimants from these industries.

² We also examined the impact of family composition and the work status of spouse on weeks of UI benefits. Some of these variables had a significant impact on weeks of UI collected, but they did not affect the treatment impacts.

³ The estimated treatment impacts were somewhat larger for the CWBH sample (over one week) than for the entire analysis sample, reflecting in part the greater variability in the results associated with smaller samples.

TABLE III.18

TREATMENT EFFECTS ON WEEKS OF UI BENEFITS
BY SEX AND INDUSTRY^a

	Men	Women	Total
Nonconstruction Industry			
Treatment Group 1	-0.88† (-1.34)	-0.14 (-0.21)	-0.51 (-1.10)
Treatment Group 2	-0.83† (-1.35)	0.23 (-0.36)	-0.30 (-0.61)
Treatment Group 3	-0.38 (-0.60)	-0.19 (-0.29)	-0.24 (-0.53)
Construction Industry			
Treatment Group 1	-2.06* (-2.23)	2.30 (0.58)	-1.81* (-1.98)
Treatment Group 2	-1.98* (-2.49)	2.02 (0.58)	-1.78* (-2.25)
Treatment Group 3	-2.07* (-2.40)	2.25 (0.70)	-1.79* (-2.11)

^a Effects were estimated using a regression model with the basic set of control variables plus a construction industry dummy variable and treatment interaction effects for the construction industry. The total sample size was 5,187.

* Coefficient statistically significant at the 95 percent confidence level for a one-tailed test.

† Coefficient statistically significant at the 90 percent confidence level for a one-tailed test.

TABLE III.19

TREATMENT EFFECTS ON WEEKS OF UI BENEFITS
BY SEX AND RECALL STATUS^a

	Men	Women	Total
Expected Recall			
Treatment Group 1	-1.84 (-0.85)	1.95 (0.59)	-0.35 (-0.19)
Treatment Group 2	-1.12 (-0.55)	0.16 (0.06)	-0.74 (-0.44)
Treatment Group 3	-2.58 (-1.19)	3.27 (1.12)	-0.52 (-0.30)
Did Not Expect Recall			
Treatment Group 1	-2.10† (1.61)	-0.46 (-0.28)	-1.46† (-1.45)
Treatment Group 2	-2.64* (-2.21)	-0.21 (-0.15)	-1.65* (-1.80)
Treatment Group 3	-1.45 (-1.15)	-1.41 (-0.89)	-1.46† (-1.48)

^a Effects were estimated using a regression model with the basic set of control variables plus a recall dummy variable and treatment interaction effects for the recall variable. The CWBH sample of 1,016 was used.

* Coefficient statistically significant at the 95 percent confidence level for a one-tailed test.

† Coefficient statistically significant at the 90 percent confidence level for a one-tailed test.

claimants who expected recall and significant negative ones for those who did not expect recall. This result generally held up for men but not for women, despite the fact that recall expectations did not differ substantially by sex.¹ Thus, there is some evidence that recall expectations may provide a useful way for program operators to reduce the burden of UI claimant registration on the ES without substantially reducing the impact of such a policy.

The available evidence also suggests that any impact will occur only for men, regardless of the independent impact of recall expectations, and that this may in part be an industry phenomenon. To explore this question further, we estimated an additional set of regressions using the CWBH data which combined the recall and construction industry variables, and the results for the various groups of claimants are reported in Table III.20.² These results show statistically significant results only for men in the construction industry, including both those who expected and those who did not expect recall.³ Thus, industry may have been the more important explanatory factor, with claimants from the construction industry much more likely to be affected by the demonstration treatments than other claimants. A possible explanation for this result may pertain to the

¹ For the whole CWBH sample, 25 percent expected recall; for men, this number was 27 percent, and for women 22 percent.

² Because of the small overall size of the CWBH sample and the small sizes of particular cells, the point estimates of the impacts should not be given too much attention. In particular, the estimates for women in construction are meaningless because of the very small number in the sample.

³ The percentage of men in construction who expected to be recalled was similar to the percentage for the remainder of the sample.

TABLE III.20

TREATMENT EFFECTS ON WEEKS OF UI BENEFITS
BY SEX, INDUSTRY, AND RECALL STATUS^a

	Men	Women	Total
Nonconstruction Industry			
Expected Recall			
Treatment group 1	-1.43 (-0.58)	2.12 (0.64)	-0.07 (-0.04)
Treatment group 2	-0.41 (-0.18)	-0.23 (-0.08)	-0.30 (-0.17)
Treatment group 3	-0.44 (-0.17)	-3.74 (-1.27)	0.55 (-0.29)
Did Not Expect Recall			
Treatment group 1	-0.93 (-0.56)	-0.22 (-0.13)	-0.58 (-0.50)
Treatment group 2	-1.81 (-1.17)	0.00 (0.00)	-0.97 (-0.90)
Treatment group 3	0.35 (0.21)	-1.46 (-0.85)	-0.49 (-0.41)
Construction Industry			
Expected Recall			
Treatment group 1	-4.75† (-1.61)	5.98 (0.63)	-3.51† (-1.31)
Treatment group 2	-3.16 (-1.22)	10.93 (1.21)	-3.00† (-1.30)
Treatment group 3	-5.66* (-2.02)	8.10 (1.02)	-3.88† (-1.56)
Did Not Expect Recall			
Treatment group 1	-4.25* (-1.81)	3.64 (0.42)	-4.02* (-1.81)
Treatment group 2	-4.56* (-2.22)	11.15 (1.28)	-3.67* (-1.89)
Treatment group 3	-4.86* (-1.82)	2.90 (0.41)	-4.93* (-2.40)

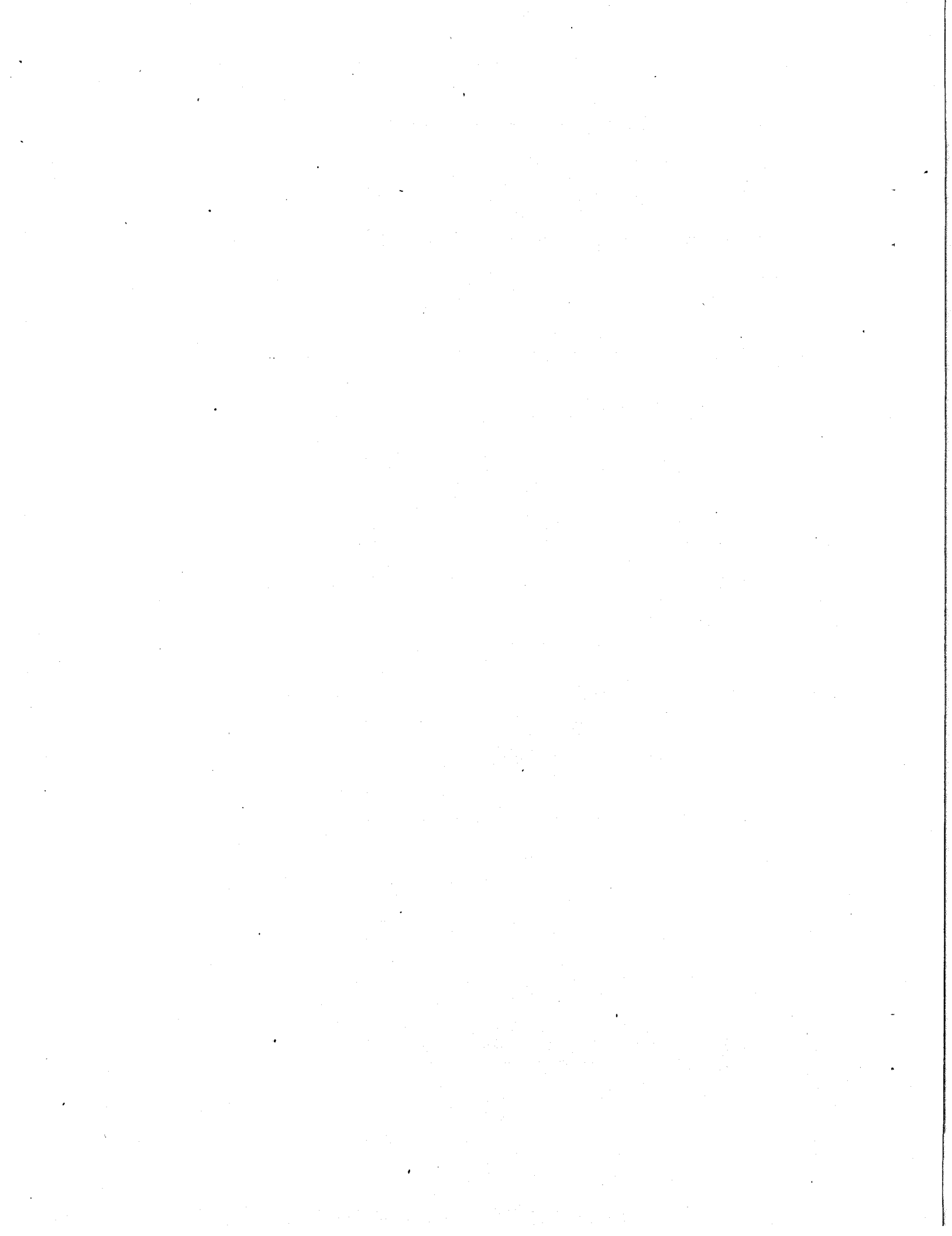
^a Effects were estimated using a regression model with the basic set of variables, construction, and recall dummy variables and treatment interaction effects for the construction and recall variables. The CWBH sample was used with 928 observations.

* Coefficient statistically significant at the 95 percent confidence level for a one-tailed test.

† Coefficient statistically significant at the 90 percent confidence level for a one-tailed test.

casual, part-time nature of some construction employment (particularly during slack periods). That is, some construction workers may prefer not to respond to the initial UI call-ins,¹ preferring instead to opt for short-term employment opportunities. However, we did not have the detailed interview data necessary to verify that possibility.

¹ Entering the construction dummy variable in our regression for failure to respond to the initial call-in showed that claimants in construction were significantly less likely to respond to the call-in than those in other industries.



CHAPTER IV

ADMINISTRATIVE COST AND IMPLEMENTATION ISSUES

Having estimated the impacts of the Claimant Placement and Work Test Demonstration on UI benefit receipt, we must now estimate the impact of the demonstration on South Carolina's administrative costs to assess the cost-effectiveness of the treatments. Specifically, we must estimate the difference in the administrative cost of each demonstration treatment compared with the administrative cost of the control treatment. We expect that the experimental treatments cost more than the control treatment, since they involved more extensive monitoring of claimant activities and more intensive ES services. The only exception to this situation is UI claims activity, which was reduced by the demonstration treatments.

The first section of this chapter outlines the methodology used to draw cost estimates for the treatments. The second section presents the estimates themselves. A final section provides a brief discussion of the operational and implementation issues that arose during the demonstration.

A. COST ESTIMATION METHODOLOGY

As described earlier in this report, several administrative functions performed by the local Job Service and UI offices in Charleston were affected by the demonstration. First, the likelihood of claimant registration and placement interviews at Job Service offices was increased by the call-ins instituted by the demonstration. Second, the breadth of the interviews (and hence their length) was increased by the enhanced-placement procedures used. Third, job-search workshops were conducted by ES staff for one of the demonstration treatment groups. Finally, the

requirement that clients cooperate with the call-ins for registration, interviews, and workshops increased the likelihood of client noncompliance, which in turn increased the number of nonmonetary determinations that had to be made by the UI office.

In addition, the demonstration affected administrative functions at the state level. First, the notices that were sent to claimants and the claimant lists used by the local offices were generated by the Employment Security data processing unit in Columbia.¹ Second, the increase in nonmonetary determinations at the local level increased the probability of appeals, an administrative function handled largely by state staff. Finally, the demonstration treatments increased the administrative responsibilities of staff in Columbia.

1. Changes in Administrative Activities and Costs

The changes in administrative activities had cost implications, the magnitude of which varied by treatment. In order to estimate the cost impact of each treatment, we distinguished between two types of effects. One pertained to changes in the frequency with which some administrative activities (such as nonmonetary determinations) were performed. With respect to call-ins and job-search workshops, the frequency was zero before the demonstration began. The other cost impact pertained to changes in the administrative cost of activities per case--notably placement interviews. In addition, the frequency and cost of some activities (such as processing

¹ Lists of claimants who received notices were sent to the local Job Service and UI offices so that they could monitor claimant flow. Manual checks were also undertaken in the local offices when payments were stopped to ensure that no incorrect notices had been sent. Relatively few problems were encountered (see Section C for a more detailed discussion).

the initial UI claim) were unaffected by the demonstration, and they can thus be ignored.

The net additional cost associated with the demonstration treatments can be thought of as a function of these frequencies and unit costs. To calculate this net additional cost for each treatment, we first computed the average total cost of serving a claimant in each treatment group (including the control group) for those administrative activities affected by the demonstration. We then compared each of these cost figures with the comparable control group figure so as to provide an estimate of the net additional cost associated with each treatment group.

The average cost for treatment group j of activities affected by the demonstration was expressed as:

$$AC_j = f_{1j} C_c + f_{2j} (C_r + C_p + f_{3j} C_e) + f_{4j} C_w + f_{5j} C_n + f_{6j} C_a + W_j C_{wc},$$

where

- f_{1j} : sum of frequencies of receiving a call-in to the Job Service for placement services or the job-search workshop or to UI for fact-finding purposes.
- f_{2j} : frequency of appearing for a placement interview (regular or enhanced service). Note that treatment groups 1 and 2 could receive multiple interviews.
- f_{3j} : frequency that interview was "enhanced," which equals one if claimant was in treatment group 1 or 2.
- f_{4j} : frequency of participating in job-search workshop.

f_{5j} :	frequency of nonmonetary determination.
f_{6j} :	frequency of appeal.
W_j :	number of UI weeks claimed.
C_c :	cost per call-in.
C_r :	cost per reception/registration.
C_p :	cost per regular-service placement interview.
C_e :	net additional cost of "enhanced" interview.
C_w :	cost per job-search workshop participant.
C_n :	cost per nonmonetary determination.
C_a :	cost per appeal.
C_{wc} :	cost per week of UI claimed.

The administrative activity frequencies necessary for this cost calculation were derived from the data collected for the evaluation and presented in the last chapter. For example, the frequency of call-ins in a given treatment group was computed from the data presented in Table III.1, by summing the probabilities of each call-in for service and the probability of not reporting (since failure to report generated a call-in to UI). An exception to this situation occurred for the frequency of placement interviews, since the data routinely collected by the ES and used for our analysis pertain to services such as job referrals rather than to placement interviews. To address this problem, we used the frequency of timely reporting to each call-in plus the frequency with which claimants who failed initially to report subsequently reported to the Job Service. These eventual visits were recorded on forms sent by the Job Service to UI as part of the nonmonetary determination process. For the control group, the frequency of receiving a placement interview was calculated on the

basis of data collected by our on-site analyst specifically for this cost estimation purpose.

The necessary unit costs were estimated on the basis of three sources of data: (1) on-site observation of certain program activities in local offices, (2) interviews with local- and state-office staff, and (3) the state's Employment Security cost-accounting system. The first two types of data were necessary to estimate the direct staff labor time required to perform each of the administrative activities of interest. The cost-accounting data were then used to value this direct labor at pertinent salary levels, and to mark up salary costs for fringe benefits, indirect labor, and nonpersonnel cost. These data were also used for most of the UI functions, since data on unit times for UI are available and used for budgeting purposes.

2. On-Site Observation

On-site observation was used to measure the additional direct labor necessary in local Job Service and UI offices to make the changes in placement interview and nonmonetary determination activities required by the demonstration. Job Service interviewers were observed at the Charleston, North Charleston, and Moncks' Corner offices between August 1983 and February 1984, a representative period for the ES workload.¹ The observation was performed over two- to five-hour blocks of time. The observer was stationed in the interviewing area and recorded the tasks performed, the time taken to perform them, the staff member who performed

¹ While intake in the demonstration ended in December 1983, the provision of demonstration treatments continued for approximately three months.

the tasks, and the treatment groups of the claimants being served. In addition to the placement interview itself, the pre-interview case identification and preparation and post-interview tasks (such as the completion of pertinent forms) were recorded. This ensured that all direct labor associated with the placement interview was taken into account.

Based on the Job Service observation, estimates of the average staff time devoted to regular service and enhanced placement interviews per claimant were made for each treatment group. This direct-labor time estimate forms the basis of the estimates for the cost variables C_p , C_e , and C_n in the above equation. To this direct labor was added a pro-rated portion of all nonproductive labor time recorded during the observation period (coffee breaks, etc.). The resulting time estimate was then valued using the salary and other program cost information described below.

On-site observation of the nonmonetary determination activities of the Eligibility Review Section at the Charleston UI office was also undertaken during the same time period. The tasks in this administrative function included the fact-finding interview and several related activities, such as case file checks, data recording, and case reviews. As with the Job Service placement interviewer observation, the observer recorded the task that was performed, the time taken, the staff member responsible, and the demonstration treatment group to which the claimant belonged. In both offices, the information was recorded on a standard form following a consistent set of observation guidelines.¹

¹The forms and procedures used are described in Corson, Nicholson, and Long (1983).

For nonmonetary determination activities, however, we did not find any difference among the treatments in per-determination costs; thus, the differences in nonmonetary determination frequencies were the only differences among the treatments. To fully account for this fact, we decided to use UI cost model information on the administrative costs of nonmonetary determinations for our estimates, since the information takes into account the small number of activities (filing, etc.) that occur in both central and local offices.¹

3. Staff Interviews

Staff interviews in the Job Service local offices were used to obtain estimates of the direct labor time associated with client reception and registration and with job-search workshops. Program staff were asked to estimate the amount of time they devoted to receiving and registering each client. Their responses were generally consistent with the results of a time study of registration procedures in the Charleston ES office conducted by Bloom Associates in 1981.² The amount of staff time devoted to the reception and registration activities did not appear to vary substantially by demonstration treatment groups. The only noticeable difference among the groups was the fact that demonstration tracking forms did not have to be completed for the control group, and, since this cost was purely a function of the demonstration, it was ignored. In addition, while some manual checking of the computer-generated lists was performed to

¹ It should be noted that our estimates were quite close to those available from the UI cost model.

² For a description, see Bloom Associates (1981).

ensure that claimants did not have their benefits stopped incorrectly, it was assumed that this activity would not be part of an ongoing system, and this cost was ignored.

Obtaining estimates of the staff time devoted to the job-search workshops was relatively straightforward. A single staff member was responsible for the workshops, and he or she followed a regular weekly schedule for workshop preparation, class, and follow-up. To calculate the average staff time devoted per client participating in a workshop, we divided the weekly time devoted by this staff member to the workshops by the average weekly enrollment in workshop classes.

Interviews were also conducted in the central office to collect information on the staff time and other resources devoted to state administrative functions affected by the demonstration. The direct labor devoted to generating UI claimant lists and call-in notices for the demonstration on an ongoing basis was estimated on the basis of interviews with data processing staff.¹ The affected personnel for this function were primarily data processing analysts and a distribution clerk. Direct nonpersonnel costs (primarily for CPU time and data processing forms) were also obtained in these interviews. Interviews with other state staff indicated that, with the exception of the UI appeals process (see below), other ongoing administrative functions were not substantially affected by the demonstration. In particular, the staff resources devoted to Charleston local-office monitoring after the demonstration planning and

¹ Start-up costs for developing the computer software were not included in the cost estimates, since we were interested in the costs of running an ongoing program.

start-up periods was not discernibly different from that necessary for other offices in the state.

These estimates of direct labor time and direct nonpersonnel expenses were used to compute values for the call-ins. As before, a prorated portion of nonproductive labor time was added to these direct labor estimates.

4. Cost Accounting Data

The staff-time estimates (including allocated nonproductive time) that were described in the previous two sections were valued using pertinent salary rates for the program staff who performed the work in question, which were obtained from state cost reports.¹ Salary costs were then marked up for paid leave, fringe benefits, and nonpersonnel expenses. The mark-ups for paid leave and nonpersonnel expenses reflect the amounts of these expenses incurred by the Charleston and state offices as a percentage of total direct wages.² Fringe benefits in fiscal year 1983 for ES and UI employees amounted to 15.03 percent of salary plus \$662 per staff position for health insurance.

To these costs were added the costs associated with administrative overhead. For the Job Service and UI local office activities, this adjustment took into account management staff in those local offices, but

¹These salary rates were computed from wages and hours charged to administrative function codes on the State Employment Security Cost Center Time Distribution Report 3B, covering fiscal year 1983.

²The mark-up for paid leave was computed using the Report 3B, which covered relevant offices for fiscal year 1983. The mark-up for nonpersonnel expenses was based on data from Employment Security Report 61, "Status of Obligational Authority," covering the same offices and time period.

it excluded state staff in Columbia--whose time was judged to be substantially unaffected by the changes in the local-office activities that have been described. Aside from data processing and the appeals process, state administrative responsibilities were altered only during the demonstration planning and start-up periods. These responsibilities were excluded from consideration here because we were interested only in estimating the additional costs to ongoing program operations of the various changes instituted during the demonstration. Administrative overhead was thus estimated as a pro-rated share of local-office management costs for local-office administrative functions, and a share of state-office management costs was used for the data-processing functions.¹

Finally, the costs associated with weeks claimed, nonmonetary determinations, and appeals were estimated on the basis of MPU data developed for use in the UI cost model.² The amount of time necessary for average intrastate weeks claimed, nonseparation issue determinations, and appeals was valued at an appropriate wage rate, and was then marked up for fringe benefits, nonpersonnel expenses, and administrative overhead. In addition, for every intrastate appeal, there is a probability that a higher appeal will be made. Thus, the average time taken for the higher appeal was multiplied by this probability and then valued and marked up in the same way as for an intrastate appeal. The unit cost of an appeal included

¹ These costs were obtained from Report 3B, which covered fiscal year 1983.

² These minutes-per-unit (MPU) estimates are developed for use in budgeting. MPU estimates take into account breaks and other nonproductive time, so no further adjustment was needed for these factors.

both the average cost of the initial intrastate appeal plus the expected value of the cost of a higher appeal.

B. COST ESTIMATES

Table IV.1 presents the estimated values of the variables in the demonstration cost equation. Particular attention should be paid to four of these estimates. First, the added cost of an enhanced placement interview, relative to a regular interview, was \$3.19. This cost reflects our estimate that, on average, an enhanced interview took seven minutes longer than a regular interview (23 versus 16 minutes).

Second, the cost of the job-search workshop per workshop participant was estimated to be \$8.51. Inasmuch as the average number of participants in a workshop was found to be 13.8, the average cost of the workshop (which includes preparation and follow-up as well as the workshop class itself) was \$117.44 per class.

Third, by far the highest unit cost was for an appeal \$140.84. However, the frequency of appeals was extremely low--less than 1 percent of clients for all groups. Thus, the net cost implication of the small increase in appeals we observed for treatment groups 1 and 2 was quite modest.

Fourth, the weeks of UI claimed function was the only one in which the treatments reduced administrative expenditures. Because of the impact of the treatment on weeks claimed, the effect was to reduce demonstration costs by \$0.75 for group 1 and by \$0.54 for groups 2 and 3, relative to the control group.¹

¹ We used weeks of UI collected as opposed to weeks claimed because of data limitations; however, doing so should not substantially affect the comparisons.

TABLE IV.1

PROGRAM-EVENT FREQUENCIES AND UNIT
COST ESTIMATES, BY TREATMENT GROUP

Variable	Treatment Group			
	1	2	3	4
f_{1j}	2.424	1.800	1.245	0
f_{2j}	1.255	1.229	.841	.353
f_{3j}	1.000	1.000	0	0
f_{4j}	.429	0	0	0
f_{5j}	.220	.199	.159	.075
f_{6j}	.0063	.0048	.0013	.0034
W_j	14.8	15.0	15.0	15.5
C_c	\$.54	\$.54	\$.54	n.a.
C_r	1.74	1.74	1.74	1.74
C_p	7.26	7.26	7.26	7.26
C_e	3.19	3.19	n.a.	n.a.
C_w	8.51	n.a.	n.a.	n.a.
C_n	5.89	5.89	5.89	5.89
C_a	140.84	140.84	140.84	140.84
C_{wc}	1.07	1.07	1.07	1.07

n.a. = not applicable.

Placing the values in Table IV.1 into the demonstration cost equation yielded the cost estimates reported in Table IV.2. The net additional cost per claimant was \$17.58 for group 1, which received all demonstration services. This cost was \$4.41 higher than for group 2, which did not participate in the job-search workshops. The difference reflects the cost of the workshop itself, the call-in for the workshop, and a higher probability of nonmonetary determinations and appeals that presumably arose because of noncompliance with the workshop requirement. The net additional cost for group 3 was \$4.72 per claimant. This lower cost, relative to the other two treatment groups, was the result of fewer call-ins, a shorter initial placement interview, no second interview, and a still lower probability of nonmonetary determinations and appeals. Thus, the net additional administrative cost of each of the treatments was quite low, reflecting the fact that the treatments generally made only marginal changes in predemonstration operating procedures. In the one situation where that was not the case (i.e., the reporting requirements), the extensive use of the computer helped keep the additional costs to a modest level.

C. IMPLEMENTATION ISSUES

In interpreting the results of the analyses presented in this report, it is important to determine whether the demonstration treatments were implemented as described in the design. It is also useful for future applications of the treatments to describe problems that arose during implementation and their solution. Both of these topics are discussed in detail on the SRI design report (Johnson et al., 1984), and they are briefly summarized here, drawing heavily on that report.

TABLE IV.2
PROGRAM COSTS PER CLAIMANT,
BY TREATMENT GROUP

Cost	Treatment Group			
	1	2	3	4
Call-ins	\$1.31	\$0.97	\$0.67	n.a.
Placement Interviews	15.29	14.98	7.57	3.17
Job Search Workshop	3.65	n.a.	n.a.	n.a.
Nonmonetary Determinations and Appeals	2.18	1.85	1.12	0.92
Weeks Claimed	15.84	16.05	16.05	16.59
Total Cost	38.27	33.85	25.41	20.68
Net Additional Cost Relative to Control Group	\$17.58	\$13.17	\$4.27	n.a.

n.a. not applicable.

In general, few implementation problems arose during the demonstration, and the treatments were applied as designed. A part-time observer stationed in the main Job Service office carefully monitored the extent to which the designed services were provided. The weekly computer-generated lists were checked for accuracy to identify problems; placement interviews were monitored to determine whether the correct services were delivered, through observations and the use of route slips that recorded services; and trips were made to the UI office to observe the nonmonetary determination process. No major problems were identified, and minor ones were corrected during the pilot phase. As stated earlier, very few changes were made in the design during that phase, and we felt that the pilot sample could be combined with the demonstration phase sample for the analysis.

We now turn to a brief discussion of how each major treatment component was implemented.

1. Strengthened Work Test

The strengthened work-test reporting requirements generated the major change made between the pilot and demonstration phases. During the pilot phase, the initial reporting requirement stipulated that claimants appear in the Job Service office during the week after the call-in notice was sent. Early in the pilot phase, it was recognized that some claimants had gone to the Job Service before the notice was received, and it was decided that they should not be penalized for failing to report in the specified week. Therefore, for the demonstration proper, the requirement was changed to permit services received up to 30 days prior to the date on which the check was issued to satisfy the requirement. Although this

change may have affected the results by diluting the demonstration phase treatments (about 5 percent of the claimants were affected) relative to the pilot phase, it was also partially implemented during the pilot phase by pulling notices manually for some claimants.¹ In addition, very few claimants had nonmonetary determinations that led to the claimant being declared eligible because he or she had already reported to the Job Service. Thus, we do not feel that this change appreciably affected the results, but it is an important change to remember for future applications of this treatment.

A few other minor changes were made in the data processing system to account for the manner in which ES renewals were handled, but these were corrected quickly, and they affected very few claimants.

Finally, the mailing of notices and the institution of specific reporting requirements as part of the demonstration treatments raised a number of questions during the implementation of the South Carolina UI system, particularly with respect to which reasons for failure to report were acceptable and which were not. While this situation was easily resolved, similar questions must be addressed in future applications, particularly in states with little experience with strict reporting requirements.

2. Enhanced Placement Services

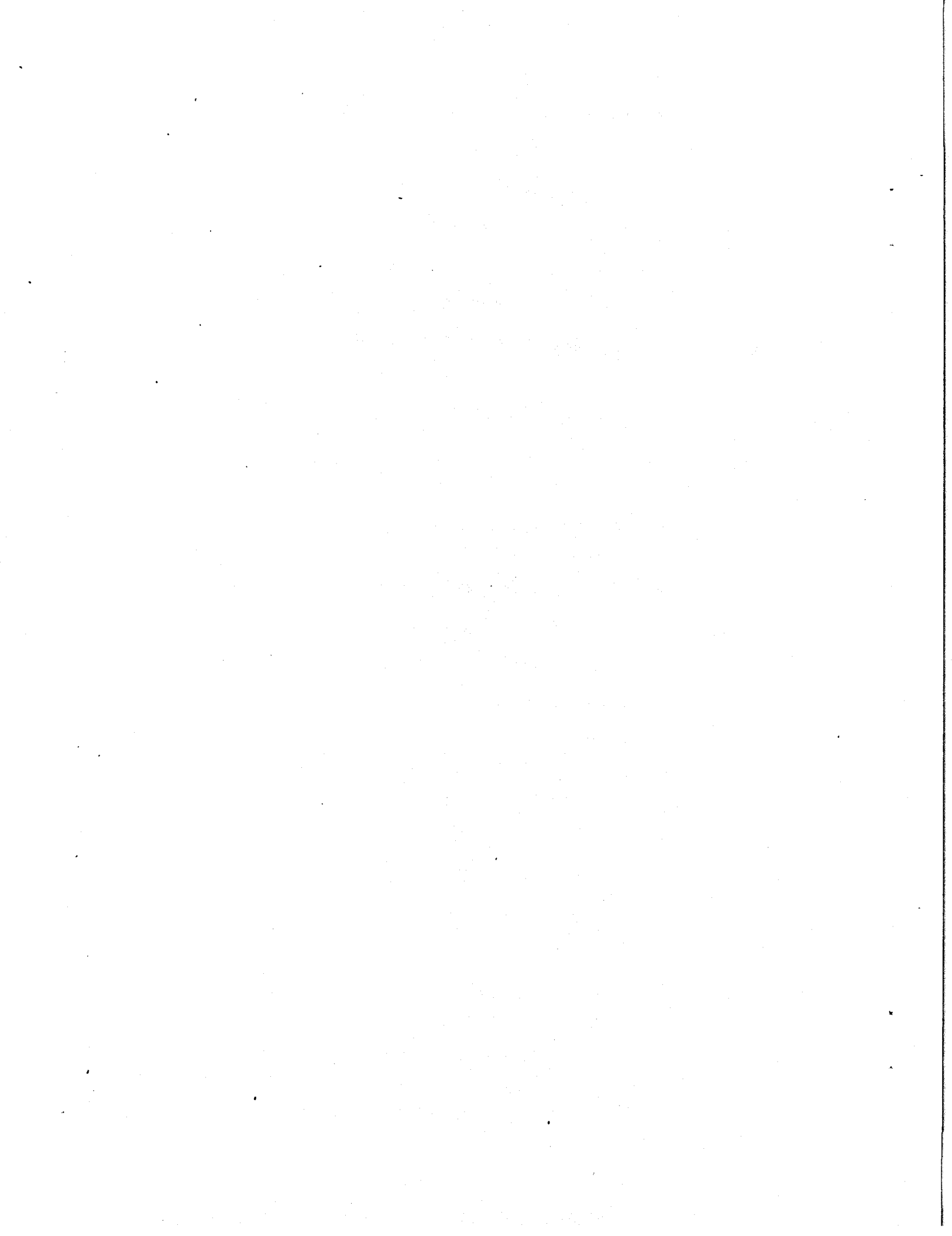
The monitoring of interviews described above showed early in the pilot phase that some interviewers were unsure about what services were to be provided to specific treatment groups. This problem was resolved

¹ These manual adjustments were also made to our data base.

quickly through additional training. The review of services during the demonstration phase indicated that problems were minimal, with, for example, 75 to 80 percent of claimants who were assigned enhanced placement and who reported to the Job Service receiving all of the enhanced placement services. Less than 2 percent received none of the additional services. The reviews conducted by the on-site observer also concluded that the claimants who were supposed to receive regular as opposed to enhanced placement services did not receive the extra attention. Thus, enhanced placement was a discernible treatment. The data on services reported in Chapter III also support this conclusion.

3. Job Search Workshop

The job-search workshop was implemented smoothly as planned, with only minor changes. The only modification of substance was that the general and professional occupation workshops that were conducted during the pilot phase were collapsed into a single workshop. This reduced the resources required, but it did not lead to any appreciable change in the content of the workshop.



CHAPTER V

OVERALL ASSESSMENT AND POLICY IMPLICATIONS

In the previous chapters we examined the impact of the three experimental treatments on the employment of claimants and their receipt of UI benefits. We also examined data on various factors that may explain the demonstration results—namely, claimants' responses to the treatment reporting requirements, their use of the ES, and the degree to which claimants were subject to nonmonetary determinations and denials. Finally, we estimated the administrative cost impacts of the treatments. In this chapter we pull together these various analyses to provide, in the first section, an estimate of the cost-effectiveness of the demonstration treatments and, in the second section, a discussion of evidence on why the outcomes may have occurred. In the final section, we discuss the policy implications of our findings and suggest directions for future applications.

A. THE COST-EFFECTIVENESS OF THE DEMONSTRATION TREATMENTS

The cost-effectiveness of each of the treatments can easily be estimated by comparing the reduction in UI benefits that resulted from each treatment with the additional administrative costs associated with the treatment. We have done so on a per-claimant basis by using the regression-adjusted impact estimates reported in Table III.17 and the administrative cost estimates reported in Table IV.2. The results, which are reported in Table V.1, show that treatment 1 (strengthened work test, enhanced placement, and the job-search workshop) reduced overall UI costs per claimant by \$56. The cost-effectiveness of the other two treatments was lower, \$46 per claimant for treatment 2 (strengthened work test and

TABLE V.1

COST-EFFECTIVENESS OF THE TREATMENTS PER CLAIMANT

	Treatment Group		
	1	2	3
Reduction in Mean UI Benefits			
Weeks collected	.76	.61	.55
Dollars collected ^a	\$73.14	\$58.71	\$52.93
Net Additional Administrative Cost Per Claimant	\$17.58	\$13.17	\$4.72
Net Reduction in UI Costs			
Per claimant ^b	\$55.56	\$45.54	\$48.21
Lower bound ^c	\$18.99	\$10.89	\$12.60
Upper bound	\$92.13	\$80.19	\$83.82

^a The dollars-collected impact estimate equals the weeks-collected estimate times the average weekly benefit amount for the sample of \$96.24.

^b The net reduction equals the additional administrative cost minus the reduction in UI benefits.

^c The upper and lower bounds are one standard deviation from the mean for the estimated impact on UI benefits.

enhanced placement) and \$48 for treatment group 3 (strengthened work test only). Nevertheless, the impacts on overall costs were sizeable for all of the treatments, particularly when one considers the size of the overall UI population. For example, these estimates suggest that the demonstration alone reduced UI costs by almost \$220,000. In addition, while these estimates are subject to sampling error, a one-standard deviation bound for our estimate of UI benefit impacts continues to yield cost-effective impacts.¹ In fact, administrative costs would have to be underestimated by a factor of approximately two to reduce our lower-bound estimates to zero. Thus, our conclusion is quite strong that each of the treatments was cost-effective.²

Although our analysis did not find statistically significant differences in impacts among the treatment groups, a comparison of cost-effectiveness among the treatments suggests that the small impact on UI benefits of the enhanced placement services and second call-in to the Job Service provided in treatment 2 relative to treatment 3 was outweighed by the increased administrative costs associated with those treatment components. On the other hand, the additional component associated with treatment 1 relative to treatment 2 (the call-in for the job-search workshop) appeared to be cost-effective. This result also implies that the

¹ The estimates are also conservative because men, among whom the impact was concentrated, had a higher weekly benefit amount (\$104) than women (\$85), and our estimates are based on the mean weekly benefit amount.

² If the treatments had been applied solely to men or to men in the construction industry, the per-claimant savings would have been considerably higher (\$100 for men and nearly \$200 for men in the construction industry), since the impacts of the treatments were quite large for these two groups.

call-in for the job-search workshop might have explained why treatment 1 appeared to be more cost-effective than treatment 3. These results occur partially because the job-search workshop was less expensive to administer per claimant than the enhanced placement interviews, but they may also be due to the fact the job-search workshop call-in occurred earlier in the benefit stream than the second enhanced placement interview. For this reason, it affected a greater proportion of the claimants.

B. WHY THE TREATMENTS WERE EFFECTIVE

In order to evaluate the impact of the demonstration treatments fully, it is important to understand why they led to a reduction in UI benefits paid to claimants. Relatedly, it is important to understand which components of the treatments led to these impacts. Three general answers appear possible. First, claimants may have found jobs more quickly than they would have in the absence of the treatments, and this may have been due to the impact of ES services--enhanced placement and/or the job-search workshop. Alternatively, the strengthened work test may have encouraged more active job search, thereby increasing employment. Another possible answer is that a greater number of ineligible claimants may have been identified and benefits denied through the formal nonmonetary determination process. The final possibility is that some claimants may not have wanted to comply with the ES registration requirements, and they may have stopped filing claims and either dropped out of the labor force or continued to search for work without UI support.

We attempted to determine analytically the degree to which each of these explanations contributed to the demonstration results; however, this analysis was generally unsuccessful, since the selection of various

treatment components was highly correlated with the outcome variables.¹ Nevertheless, the results presented in Chapter III provide some indications about why the treatments led to a reduction in UI benefits.

With respect to the first possible explanation, the available evidence on employment outcomes provided some indications that the enhanced placement treatment may have slightly increased ES placements. However, our overall conclusion, based on the placement and wage record evidence, was that no strong evidence indicated that the treatments affected the reemployment success of claimants, although available wage record data are too aggregate in nature to answer this question definitively. Thus, it appears that increased employment within the treatment groups probably had, at the most, a minor impact on the results.

The second possible explanation was that the treatments enhanced detecting ineligible claimants. This outcome did occur because the denial rate increased for each of the treatment groups relative to the control group. However, many of the actual denials generated by the demonstration were for short durations, and some claimants who were denied benefits eventually exhausted UI. Thus, while increased detection of ineligible claimants was an important factor in explaining some of the treatment impacts, the formal determination process probably did not itself generate all the savings in UI benefits.

The final explanation (i.e., that claimants responded to the demonstration by leaving the UI rolls without necessarily finding a job)

¹ For example, ES use was highly correlated with UI duration, and the inclusion of an ES use variable in the weeks of benefits regressions yielded a large positive coefficient for ES use. This result also occurred when more complicated selection bias models were used.

was probably also an important factor in the outcomes. The data on responses to the call-in notice showed that many claimants did not respond to the notices, and the data on UI benefit receipt showed that the treatments had a significant impact on the rate at which claimants stopped claiming UI benefits early in their benefit period. Yet, no strong evidence indicated that these claimants necessarily found jobs, although no information was available on whether claimants who left the UI rolls continued to search for work or whether they dropped out of the labor force.

In conclusion, it appears that the reporting requirements, coupled with the cessation of UI payments for failure to report, were probably the most important elements of the treatments. And a major way in which these components had an impact on benefits was to cause claimants to leave the UI rolls both because some were formally denied benefits and because some simply stopped claiming benefits.

C. POLICY IMPLICATIONS

Although, as described in Chapter II, the results of the Charleston demonstration may not be fully generalizable to other jurisdictions, our findings did suggest some policy lessons. Most important, the results of the demonstration clearly indicated that the manner in which the Job Service monitors the UI work test does matter. Specifically, strengthening and regularizing the way in which initial ES registration was handled in Charleston seemed to have been a cost-effective procedure. Similar though probably less dramatic impacts seemed to have been generated by the later ES call-ins incorporated into the demonstration and by the attempt to target ES services toward workers who were not job-attached.

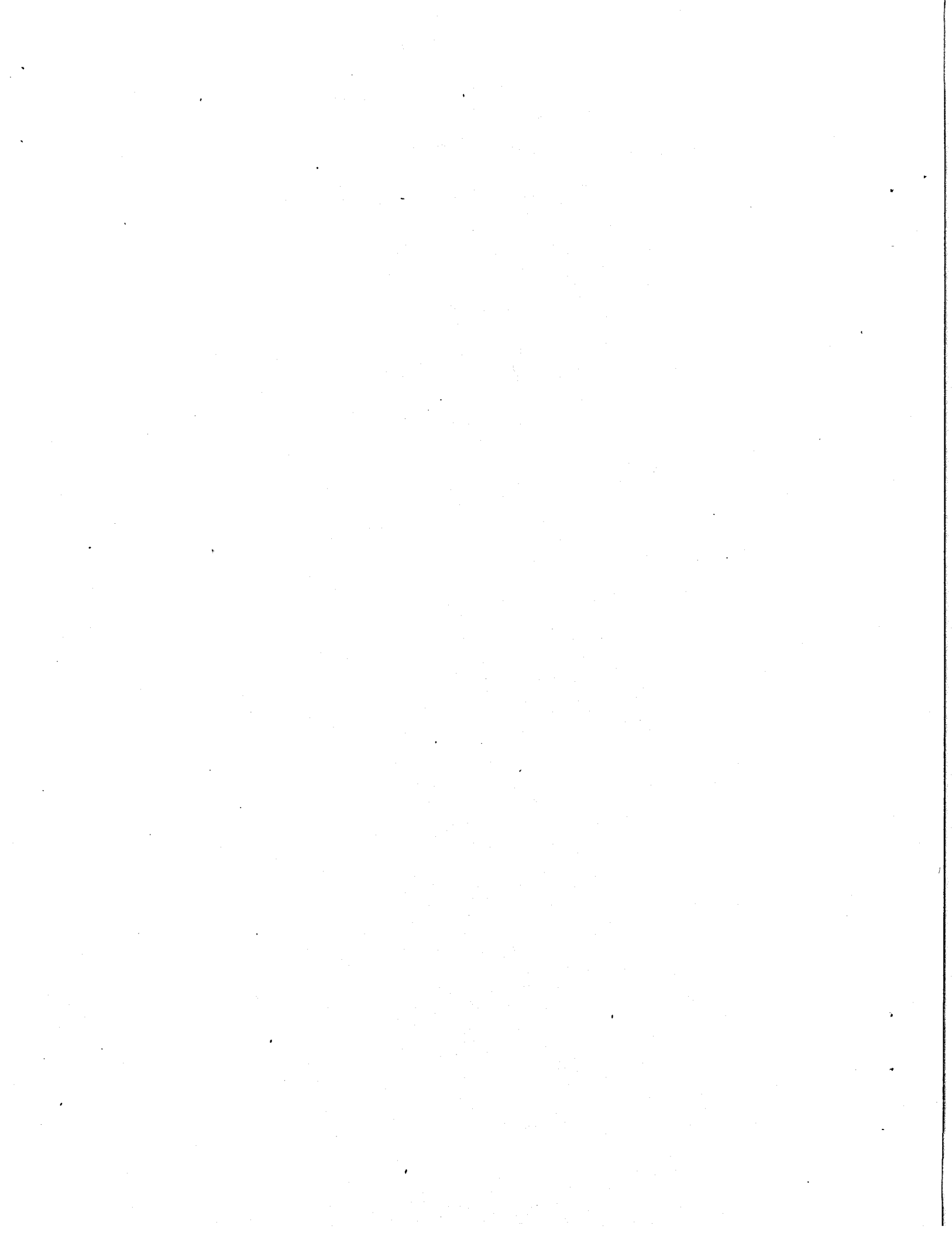
Whether these effects might be replicated elsewhere would seem to depend on at least three factors. First, are existing ES procedures on the UI work test amenable to the same type of strengthening and regularization that occurred in Charleston? In some cases, such improvements may indeed be possible, although the procedures would probably not be identical to those adopted in Charleston. In other instances, however, existing registration procedures may already be quite effective, and results similar to those in Charleston are unlikely. A second factor that affects potential replicability is the nature of the UI claimant caseload. Since the impact of the Charleston demonstration seemed to focus importantly on males in the construction industry, jurisdictions that have only a small part of their caseload in this category or in which special policies already apply may not experience such responses. Similarly, existing UI laws and administrative procedures may also affect the potential impact of Charleston-type treatments. Very different availability for work definitions or UI benefit denial provisions might yield very different results. In general, then, assessing the potential replicability of the Charleston demonstration to other locales requires a careful examination of the circumstances involved. However, it would appear that, at least in some circumstances, replication should be possible.

The policy implications from examining the outcomes of the Charleston project as they pertain to the special employment assistance provided were more ambiguous. Although the demonstration design offered the possibility of identifying such effects in a much clearer way than has been possible in most prior research, the results did not show significant effects. Enhanced placement activities did lead to additional placements,

and some individuals who participated in the job-search workshops were credited with obtaining employment from that activity. However, we were generally unable to identify a significant relationship between these procedural outcomes and more general demonstration outcomes, such as employment or weeks of UI benefits collected. Of course, it should be recognized that all these special services required relatively small administrative expenditures, and our sample sizes may not have been sufficiently large to identify small though cost-effective outcomes. It is possible that such effects would occur in a larger-scale replication of these special services. However, the present demonstration offered only mild support for that possibility.

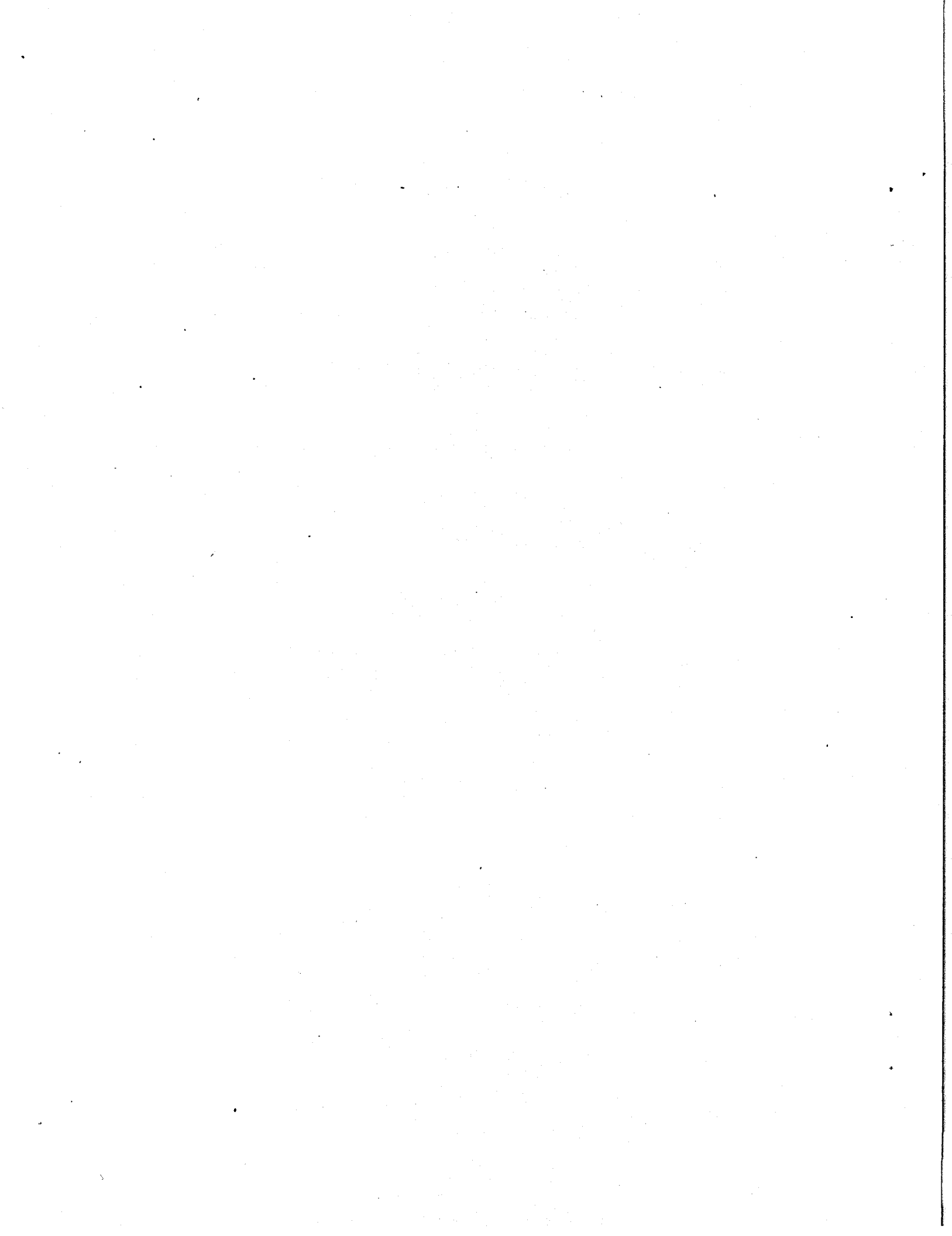
Finally, the Charleston demonstration offered a number of implications for future research on the UI/ES relationship. Most important, it clearly showed that research on Job Service procedures is possible, and that important lessons can be learned from it. Future examinations of the UI/ES relationship may be required to determine whether the results of the Charleston demonstration continue to hold in different economic and administrative environments. In order to sharpen this research, it may be desirable to develop ways to divorce ES work test-monitoring activities further from ES employment assistance activities, so as to measure the effects of these activities independently. Although it may never be possible to separate these activities fully, it may be possible to do so more completely than was done in Charleston, primarily by carefully structuring the call-in procedures. Such a design would not only further help identify cost-effective procedures, but it might also help

clarify long-standing issues about the proper relationship between
Unemployment Insurance and the Job Service.



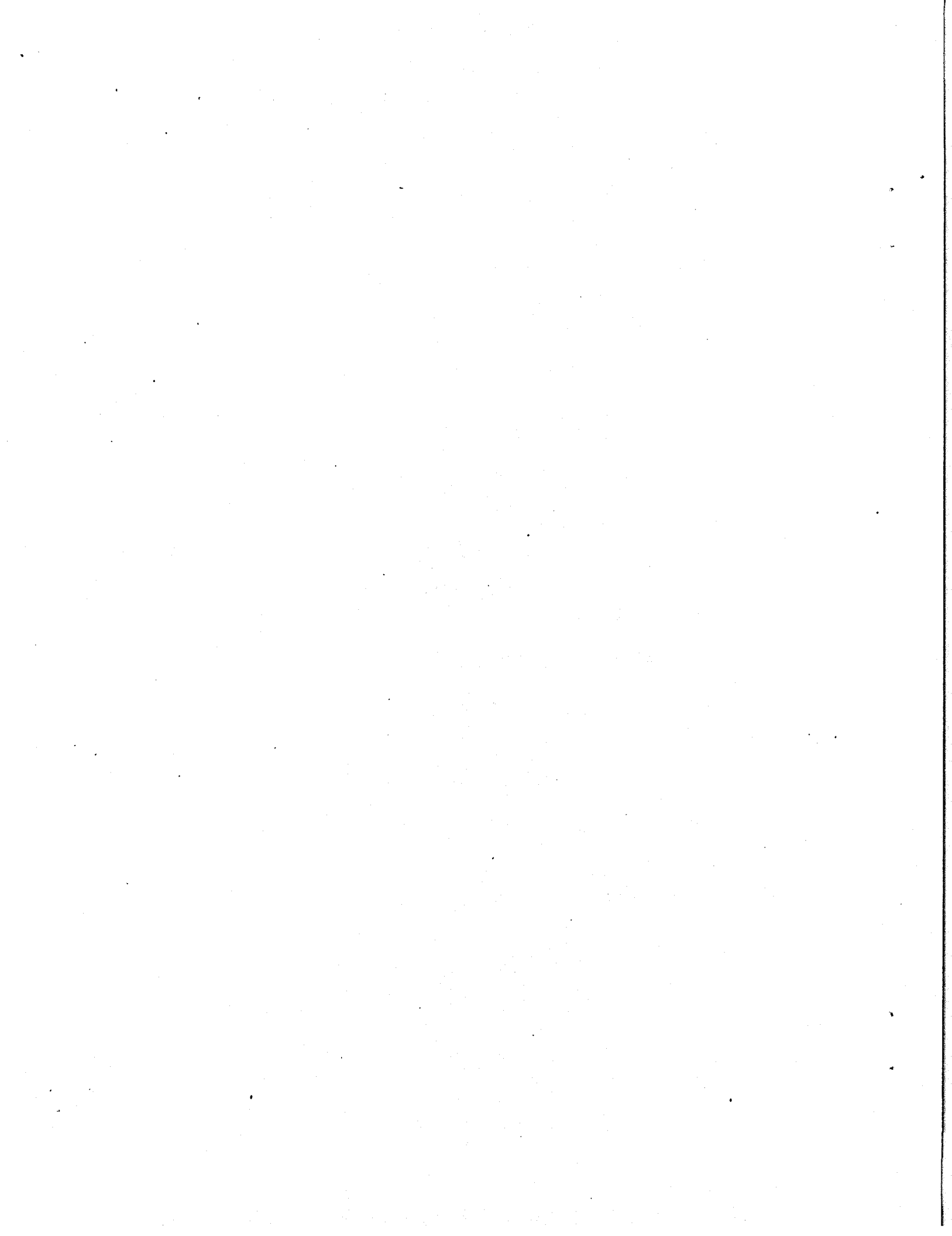
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APPENDIX A:

THE JOB ORDER TREATMENT



In developing the Claimant Placement and Work Test Demonstration, a decision was made to examine a method by which the Employment Service might improve and expand the job openings listed by employers, since it was expected that such a policy would help achieve the objective of improving the employment prospects of UI claimants. The particular method chosen to expand ES-listed job openings was to contact the former employers of UI claimants who had quit or been discharged for cause, in the expectation that such employers would have job openings that they might be willing to list with the ES. This policy, if successful, would benefit all ES applicants, claimants and nonclaimants alike, and, hence, this demonstration "treatment" was not part of the formal demonstration design or evaluation. Instead, it was to be implemented informally, and a largely descriptive, nonrigorous evaluation was to be performed.

This appendix describes the treatment procedures in some detail, and it also provides a brief discussion of what was accomplished.

A. JOB ORDER TREATMENT PROCEDURES

Prior to the demonstration, copies of UI applications in Charleston were sent to the local Job Service office on a routine basis for DOT coding and potentially for monitoring work-test registrations. The job order treatment was designed to use the information on these forms further to develop job orders for the ES.

The job order treatment called for distributing these forms to job order-takers who were to sort through them to exclude cases which would not lead to possible job openings (i.e., cases in which the UI claimant had been laid off for the lack of work). The order-takers were also expected to be familiar with employers who had already listed jobs with the ES, and

these cases were to be removed. The remaining cases were then expected to represent situations in which a job opening was likely to exist.

Once this first step was completed, a list was to be made in a contact log, and the order-takers were to attempt to contact the employers by telephone. They were to try to verify whether a job vacancy existed, and they were then to attempt to obtain a listing by telephone. If the employer wanted additional information, the order-taker was to suggest that an Employment Service Representative (ESR) would visit the employer to describe the Job Service and its activities in more detail. The results of the contact or attempted contact were then to be recorded.

An issue which arose in designing this procedure was whether or not the order-taker should refer to the information he or she had about the possible vacancy. It was decided to try two procedures: the direct approach (in which the order-taker said, "We understand you lost a 'claimant's job title.' Do you have an opening for this or any other position?") and the less direct approach (in which the order-taker asked, "Do you possibly have an opening that we could refer to, say, a 'claimant's job title' or related job?"). In practice, there were two order-takers, one of whom felt comfortable with the direct approach and one who did not, and they each used their own method for approaching employers.

B. RESULTS OF THE JOB ORDER TREATMENT

The job order treatment was never given an adequate test during the demonstration. Early in the pilot phase, an attempt was made to implement this treatment, but since the staff assigned to this task also had other duties they were unable to devote much time to this activity. Moreover, as the Charleston economy improved during the demonstration, job orders

increased substantially, and the ordertakers increasingly devoted their time to taking orders. They had no time to implement this treatment.

Consequently, activity in all components of the treatment declined during the demonstration. Although some employers with potential openings continued to be identified during the entire demonstration, all other activities ceased completely by the end of September, and, in fact, almost no activity was undertaken after early July. This decline in the use of the treatment is shown in Table A.1. Despite this situation, the data in the table indicate that the job-order-increasing procedures may have been somewhat useful, particularly during slack times when staff were available and job orders were low. An examination of UI applications appeared to identify a number of cases each week with potential job openings. While many of these were not contacted (about 60 percent), those who were contacted provided some job orders, with 1.3 orders generated for each 10 calls. Of course, we do not know whether other methods, such as random calls to employers, would achieve similar results. Nevertheless, this experience may be useful to ES program operators who are interested in increasing job orders, and a more adequate test of this treatment may be warranted.

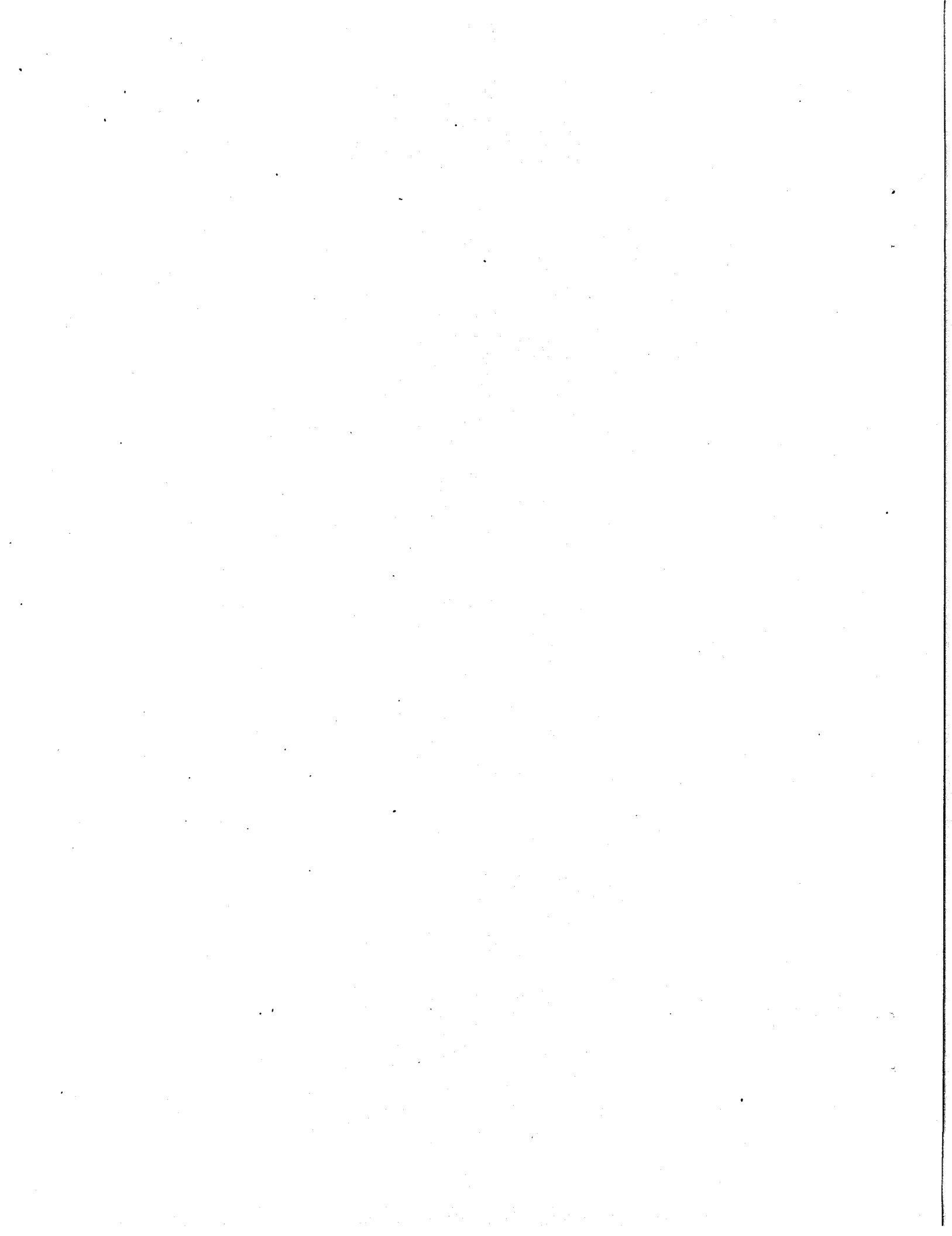
TABLE A.1
 ACTIVITY PERTAINING TO JOB ORDER TREATMENT
 3/4 TO 9/23a

Week Ending	Employers Identified as Having Potential Openings	Number of These Employers Contacted	Number of Job Orders Received
3/4	49	12	1
3/11	29	42	7
3/18	28	17	0
3/25	66	41	6
4/1	36	36	3
4/8	29	11	1
4/15	31	18	3
4/22	26	0	0
4/29	31	12	2
5/6	24	9	1
5/13	28	5	1
5/20	19	16	3
5/27	27	15	3
6/3	13	3	0
6/10	7	2	2
6/17	39	20	1
6/24	32	4	0
7/1	12	6	1
7/8	17	2	0
7/15	18	0	0
7/22	5	2	0
7/29	25	0	0
8/5	7	0	0
8/12	13	0	0
8/19	8	0	0

TABLE A.1 (continued)

Week Ending	Employers Identified as Having Potential Openings	Number of These Employers Contacted	Number of Job Orders Received
8/26	17	0	0
9/2	14	0	0
9/9	13	0	0
9/16	11	0	0
9/23	18	2	2

^a One hundred forty-six (146) employers with potential openings were identified during the remainder of the demonstration, but no other activity occurred.



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